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THE  
WORKS  
OF THE HONOURABLE  
ROBERT BOYLE.

IN SIX VOLUMES.

To which is prefixed

The LIFE of the AUTHOR.

VOLUME THE THIRD.

A NEW EDITION.



*N. Goulet inv. & delin.*

L O N D O N :

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M DCC LXXII.





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THE  
O R I G I N  
O F  
F O R M S and Q U A L I T I E S,

According to the CORPUSCULAR PHILOSOPHY;  
Illustrated by CONSIDERATIONS and EXPERIMENTS.

Written formerly by Way of NOTES upon an ESSAY from NITRE.

Augmented by a DISCOURSE of SUBORDINATE FORMS.

*Audendum est, & veritas investiganda, quam etiamsi non assequamur, omnino tamen propriius,  
quàm nunc sumus, ad eam perveniemus. GALEN.*

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*The Publisher to the Ingenious Reader.*

**I**N this curious and inquisitive age, when men, altogether dissatisfied and wearied out with the wranglings and idle speculations of the schools, are with equal zeal and industry so earnest in their quest and pursuit of a more solid, rational, and useful philosophy, it may prove a work very obliging and meritorious to help and guide them in their studies and researches, and to hang out a light to them (as the *Egyptians* used to do from their highly celebrated *Pbaros*, for direction to the mariners that failed in those dangerous seas near *Alexandria*) whereby they may with better success steer their course through the vast ocean of learning, and make more full and perfect discoveries of hitherto unknown philosophical verities: which has been the chief design of this gentleman of honour, the most excellent and incomparable author, in this treatise now presented to your view, wherein principles are not (as was the mode and guise of former times) obtruded on the world upon the account of a great name, or involved in cloudy and mystical notions, which put the understanding upon the wreck, and yet when with all this labour and toil of the brain they are at last known, prove impertinent and useless to the making out with satisfaction, or so much as tolerably, the ordinary phenomena which nature every day presents the world with; but such as are built upon the firm and immoveable foundation of reason, sense, and experience, plain and obvious, as well to the eye, as to the understanding, and no less accurate and certain in their application. And though the most noble author hath herein, for the main, espoused the atomical philosophy (corrected and purged from the wild fancies and extravagancies of the first inventors of it, as to the origin of the universe, and still imbraced with so much kindness and tenderness by some pretenders, against which he hath so learnedly disputed in his first part *Of the Usefulness of Experimental Philosophy*)



*sophy*) in explicating the appearances; yet, considering the several alterations and additions (the happy product of his penetrating judgment) made therein, I may not scruple to call it a new hypothesis, peculiar to the author, made out by daily observations, familiar proofs and experiments, and by exact and easily practicable chymical processes; whereby one of the most abstruse parts of natural philosophy, the origin of forms and qualities, which so much vexed and puzzled the antients, and which, I would speak with the leave of the *Cartesians*, their ingenious master durst scarce venture upon, or at least was unwilling to handle at large, is now fully cleared and become manifest: so that from this very essay we may well take hope, and joyfully expect to see the noble project of the famous *Verulam* (hitherto reckoned among the *Desiderata*) receive its full and perfect accomplishment; I mean a real, useful, and experimental physiology, established and bottomed upon easy, true, and generally received principles. But I shall not forestall thy judgment either about the excellency of the author, or his subject, who hath so freely communicated to the world those treasures of learning, wherewith his mind is enriched, but shall soon refer you to the work itself, after I have given you these few advertisements.

THE following discourse (as is easily perceivable by divers passages thereof) being written several years since whole and intire, as now it is, I know not whether it will be worth while to intimate, that the author casually turning over of late a very recent chymical writer found in one of his treatises (divers of which he never to this day read over) a part of the fifth experiment of the second section; but, as he professes (and sure is like to be believed) he did not dream that that chymist, or any other author whatsoever, had lighted on that part of the experiment, till a good while after he had made and examined that, among many others, concerning salts, as may be easily guessed by the peculiar uses and applications he made of it. And though he had met with so unlikely an experiment in a writer, who, whether he deserve it or no, has the ill fortune to be much accused of insincerity, and some of whose more easy processes our author (who is yet willing to spare his name, and seems to think his works not useless) could not find to succeed, he should not have taken it upon his authority, no more than he is wont to take other processes, divers of which he yet in the general supposes may be true, upon the relation of other chymists, who, by blemishing their books by things untrue and justly suspicious, are not to be relied on, nor much thanked by wary men. But it will probably appear less pertinent to add any thing further on this subject, than to take notice, that when the author had once consented to the publication of the following papers, he several times wished for an opportunity to make the experiments and observations he now presents to the publick, more full and compleat than they were when addressed to a private friend. But the contagion that drove him from the places where his accommodations for repeating experiments were, obliged him to apply himself to other studies and employments.

AND upon the same account, though he afterwards found many of his notes upon other parts of the essay of Salt-petre, and hath lying by him divers papers concerning sensible Qualities, and Sensation in general, and the Production of second Qualities, together with a collection of Notes about occult Qualities, and some other subjects of kin to those of this book; yet having, upon the freshly intimated occasion, diverted his thoughts to other subjects, he will not ingagè himself to put together and communicate his collections on these subjects by any publick promise.

FURTHERMORE, as the author has in the following disquisitions aimed not at the raising or abetting a faction in philosophy, but at the discovery of the truth; so he is not so solicitous, what every sort of reader will think of his attempts (which is easy to foresee are not like to be over-welcome to the votaries of the school-philosophy) as to refuse a compliance with the desires of his friends, who have been long since very earnest with him,



him, not to spend that time in replies to particular persons, which might be more usefully employed in pursuing further discoveries of nature by experiments. If he meet with any cogent and material objections against any of his chief opinions, he is enough a lover of truth to be disposed to think himself obliged by those who shall shew him his mistakes, and to take occasion to reform them. But if nothing new or weighty be urged, he considers, that he lives in an age, wherein he has observed (even in his own case) that truth, if recommended by real experiments, will in time make its own way, and wherein live store of ingenious men, who for the main, approve the opinions, and probably will not dislike the arguments he has proposed; and who being more at leisure than he to write polemical books, will not silently suffer what they judge truth to be triumphed over, or oppressed by those, who, employing usually but scholastical arguments, may be confuted by answers of the like nature. And therefore he doubts not, but that some learned favourers of the corpuscularian philosophy (of which he hath endeavoured to make out those parts, wherein they almost all agree) will be both able and willing to defend those discoveries by rational disputations, that they have not opportunity to increase by new experiment.

In the mean while, I have no temptation to doubt in the least, but that this curious and excellent piece will be entertained and received by all that have any regard to the great concerns of learning, with that gust, delight, respect, and estimation, which it so highly merits.

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*The Author's Præmial Discourse to the Reader\*.*

AS it is the part of a mineralist, both to discover new mines, and to work those that are already discovered, by separating and melting the ores to reduce them into perfect metal; so I esteem, that it becomes a naturalist, not only to advise hypotheses and experiments, but to examine and improve those that are already found out. Upon this consideration (among other motives) I was invited to make the following attempt, whose productions coming to be exposed to other eyes, than those for which they were first written, it will be requisite to give the publick some account of the occasion, the scope, and some circumstances. And this I shall do the more fully, because the reasons I am to render of my way of writing, in reference to the Peripatetick philosophy, must contain intimations, which, perhaps, will not be useless to some sorts of readers (especially gentlemen) and, by being applied to most of those other parts of my writings that relate to the school-philosophy, may do them good service, and save both my readers and me some trouble of repetitions.

HAVING four or five years ago published a little physico-chymical tract about the different parts and reintegration of nitre, I found as well by other signs, as by the early solicitations of the stationer for a new edition, that I had no cause to complain of the reception that had been given it: but I observed too, that the discourse, consisting chiefly of reflexions that were occasionally made upon the phenomena of a single experiment, was more available to confirm those in the corpuscularian philosophy, that had already somewhat inquired into it, than to acquaint those with the principles and notions of it, who were utter strangers to it; and, as to many readers, was fitter to excite a curiosity for that philosophy, than to give an introduction thereunto. Upon this occasion it came into my mind, that about the time when I writ that essay about salt-

\* The following preface being addressed only to *Pyrophilus*.



petre (which was divers years before it was published) I had also some thoughts of a history of qualities, and that having in loose sheets set down divers observations and experiments proper for such a design, I had also drawn up a discourse, which was so contrived, that though some parts of it were written in such a manner, as that they may serve for expository notes upon some particular passages of the essay; yet those parts with the rest might serve for a general preface to the history of qualities, in case I should ever have conveniency, as well as inclination, to make the prosecuting of it my business; and in the mean time might present that *Pyrophilus*, to whom I writ some kind of introduction to the principles of the mechanical philosophy, by expounding to him, as far as my thoughts and experiments would enable me to do, in few words, what, according to the corpuscularian notions, may be thought of the nature and origin of qualities and forms; the knowledge of which either makes or supposes the most fundamental and useful part of natural philosophy. And to invite me to make use of these considerations and trials about qualities and forms, it opportunely happened, that though I could not find many of the notes written about particular qualities (my loose papers having been, during the late confusions, much scattered by the many removes I had then occasion to make); yet when last winter, being urged to publish my History of Cold (which soon after came forth) I rummaged among my loose papers, I found, that the several notes of mine, that he had met with under various heads, but yet all concerning the origin of forms and qualities, together with the preface addressed to *Pyrophilus* (though written at distant times and places) had two or three years before, by the care of an industrious person, with whom I left them, been fairly copied out together (which circumstance I mention, that the reader may not wonder to find the following book not written uniformly in one continued tenor) excepting some experiments, which having been of my own making, it was not difficult for me to perfect, either out of my notes and memory, or (where I doubted their sufficiency) by repeated trials. So that if the urgency, wherewith divers ingenious men pressed the publication of my new experiments about cold, and my unwillingness to protract it, till the frosty season, that was fittest to examine and prove them, were all past, had not prevailed with me to let those observations be made publick the last winter, they might have been accompanied with the present essay of the Origin of Qualities and Forms, which have been premised to what I have written touching any of the particular qualities, since it contains experiments and considerations fit to be preliminary to them all.

BUT though I was by this means diverted from putting out the following treatise at the same time with the History of Cold, yet I was without much difficulty prevailed with not to alter my intentions of suffering it to come abroad; because divers of my historical accounts of some particular qualities are to be reprinted, which may receive much light and confirmation by the things delivered in this present treatise about qualities and forms in general. To which inducement was added the persuasion of some ingenious persons, who are pleased to confess their having received more information and satisfaction in these papers, than I durst pretend to give them: though indeed the subject is so noble and so important, and does so much want the being illustrated by some distinct and experimental discourse, that not only, if I did not suspect my friends of partiality, I should hope, that it may gratify many readers, and instruct more than a few; but such as it is, I do not altogether despair, that it will prove neither unacceptable nor useless. And indeed the doctrines of forms and qualities, and generation, and corruption, and alteration, are wont to be treated of by scholastical philosophers in so obscure, so perplexed, and so unsatisfactory a way, and their discourses upon these subjects do consist so much more of logical and metaphysical notions and niceties than of physical observations and reasonings, that it is very difficult for any reader of but an ordinary



ordinary capacity to understand what they mean, and no less difficult for any intelligent and unprejudiced reader to acquiesce in what they teach : which is oftentimes so precarious and so contradictory to itself, that most readers (without always excepting such as are learned and ingenious) frighted by the darkness and difficulties wherewith these subjects have been furrounded, do not so much as look after or read over these general and controverted matters, about which the schools make so much noise ; but despairing to find any satisfaction in the study of them, betake themselves immediately to that part of physicks that treats of particular bodies. So that to these it will not be unacceptable to have any intelligible notions offered them of these things, which, as they are wont to be proposed, are not wont to be understood : though yet the subjects themselves, if I mistake not, may be justly reckoned not only amongst the noblest and most important, but (in case they be duly proposed) among the usefulest and most delightful speculations that belong to physicks.

I CONSIDER too, that among those that are inclined to that philosophy, which I find I have been much imitated in calling Corpuscularian, there are many ingenious persons, especially among the nobility and gentry, who having been first drawn to like this new way of philosophy by the sight of some experiments, which for their novelty or pretiness they were much pleased with, or for their strangeness they admired, have afterwards delighted themselves to make or see variety of experiments, without having ever had the opportunity to be instructed in the rudiments or fundamental notions of that philosophy whose pleasing or amazing productions have enamoured them of it. And as our *Pyrophilus*, for whom these notes were drawn up, did in some regards belong to this sort of virtuosi, so it is not impossible, but that such readers, as he was then, will not be sorry to meet with a treatise, wherein though my chief and proper business be the giving some account of the nature and origin of forms and qualities ; yet, by reason of the connexion and dependance betwixt these and divers of the other principal things that belong to the general part of physicks, I have been obliged to touch upon so many other important points, that this tract may in some sort exhibit a scheme of, or serve for an introduction into the elements of the Corpuscularian philosophy.

AND as those readers, that have had the curiosity to peruse what is commonly taught in the schools about forms, and generation, and corruption, and those other things we have been mentioning, and have (as is usual among ingenious readers) quitted the study of those unsatisfactory intricacies with disgust, will not be displeased to find in our notes such explications of those things, as render them at least intelligible ; so it will not, perhaps, prove unacceptable to such readers, to find those matters which the schools had interwoven with *Aristotle's* doctrine, reconciled and accommodated to the notions of the corpuscular physicks.

If it be said, that I have left divers things unmentioned, which are wont to be largely treated by the *Aristotelians*, and particularly have omitted the discussion of several questions, about which they are wont very solemnly and eagerly to contend ; I readily acknowledge it to be true : but I answer further, that to do otherwise than I have done, were not agreeable to the nature of my design, as is declared in the preface to *Pyrophilus* ; and that though most readers will not take notice of it, yet such as are conversant in that sort of authors, will, I presume, easily find that I have not left them unconsulted, but have had the curiosity to resort to several both of the more and of the less recent scholastical writers about physicks, and to some of the best metaphysicians to boot, that I might the better inform myself, both what their opinions are, and upon what arguments they are grounded. But as I found those inquiries far more troublesome than useful, so I doubt not, that my omissions will not much displease that sort of readers, for whose sake chiefly it is that these papers are permitted to be made publick. For



if I should increase the obscurity of the things themselves I treat of, by adding the several obscurer comments (rather than explications) and the perplexed and contradictory opinions I have met with among scholastic writers, I doubt that such persons, as I chiefly write for, would, instead of better comprehending what I should so deliver, absolutely forbear to read it. And there being many doctrines, to which number this we are speaking of seems to belong, wherein the same innate light, or other arguments, that discover the truth, do likewise sufficiently shew the erroneousness of dissenting opinions; I hope it may suffice to propose and establish the notions that are to be embraced, without solicitously disproving what cannot be true, if those be so. And indeed there are many opinions and arguments of good repute in the schools, which do so intirely rely upon the authority of *Aristotle*, or some of his more celebrated followers, that where that authority is not acknowledged, to fall upon a solemn confutation of what has been so precariously advanced, were not only unnecessary, but indiscreet, even in a discourse not confined to the brevity challenged by the nature of this of ours. And there are very many questions and controversies, which, though hotly and clamorously contended about, and indeed pertinent and fit enough to be debated in their philosophy, do yet so much suppose the truth of several of their tenets, which the new philosophers reject; or are grounded upon technical terms or forms of speaking, that suppose the truth of such opinions; or are expressions, whereof we neither do nor need make any use; that to have inserted such debates into such a discourse as mine, would have been not only tedious, but impertinent. As (for instance) those grand disputes, whether the four elements are endowed with distinct substantial forms, or have only their proper qualities in stead of them? and whether they remain in the mixed bodies, according to their forms, or according to their qualities? And whether the former or the latter of those be or be not refracted? These, I say, and divers other controversies about the four elements and their manner of mixture, are quite out of doors in their philosophy, that acknowledge neither, that there are four elements, nor that cold, heat, dryness, and moisture are, in the peripatetic sense, first qualities, or that there are any such things as substantial forms *in rerum naturâ*. And it made me the more unwilling to stuff these papers with any needless school-controversies, because I found upon perusal of several scholastic writers (especially the recenter, who may probably be supposed to be the most refined) that they do not always mean the same things by the same terms, but some employ them in one sense, others in another, and sometimes the same writer uses them in very different senses, which I am obliged to take notice of, that such readers as have consulted some of those authors may not accuse me of mistaking or injuring some of the scholastical terms and notions he may meet with in these papers, when I have only employed them in the sense of other school-writers, which I judged preferable. And this put me in mind of intimating, that whereas, on the contrary, I sometimes employed variety of terms and phrases to express the same thing, I did it purposely, though perhaps to the prejudice of my own reputation, for the advantage of *Pyrophilus*; both I and others having observed, that the same unobvious notions being several ways expressed, some readers, even among the ingenious sort of them, will take it up much better in one of those expressions, and some in another.

BUT perhaps it will be wondered at, even by some of the new philosophers, that dissenting so much as I do from *Aristotle* and the schoolmen, I should overlook or decline some arguments, which some very ingenious men think to be of very great force against the doctrine I oppose. But divers of these arguments being such, as the logicians call *ad hominem*, I thought I might well enough spare them. For I have observed *Aristotle* in his *Physics* to write so very often in so dark and ambiguous a way, that it is far more difficult than one would think, to be sure what his opinion was: and the unlearned

and



and too frequently jarring glosses of his interpreters have often made the comment darker than the text ; so that (though in most it be, yet) in divers cases it is not easy (especially without the expence of many words) to lay open the contradictions of the Peripatetick doctrine, besides that the urging such contradictions are oftentimes fitter to silence an unwary adversary, than satisfy a wary and judicious reader ; it being very possible, that a man may contradict himself in two several places of his works, and yet not be in both of them in the wrong. For one of his assertions, though inconsistent with the other, may yet be consistent with truth. But this is not all I have to say on this occasion. For besides that having, for many reasons elsewhere mentioned, purposely forbore the reading of some very much, and, for aught I know, very justly esteemed discourses about general hypotheses, it is very possible, that I may be a stranger to some of those arguments : besides this, I say, I confess I have purposely forbore to make use of others, which I have sufficiently taken notice of. For some of those ratiocinations would engage him, that should employ them, to adopt an hypothesis or theory, in which perhaps I am not so thoroughly satisfied, and of which I do not conceive myself to have, on this occasion, any necessity to make use : and accordingly I have forbore to employ arguments, that are either grounded on, or suppose indivisible corpuscles called Atoms, or any innate motion belonging to them ; or that the essence of bodies consists in extension, or that a vacuum is impossible ; or that there are such *globuli cælestes*, or such a *materia subtilis*, as the *Cartesians* employ to explicate most of the phenomena of nature. For these and divers other notions, I (who here write rather for the Corpuscularians in general, than any party of them) thought it improper needlessly to take in, discoursing either against those to whom these things appears as disputable, as the Peripatetick tenets seem to me ; or for to satisfy an ingenious person, whom it were not fair to impose upon with notions that I did not myself think proper.

AND on the like account I forbore such arguments as those, that suppose in nature and bodies inanimate, designs and passions proper to living, and perhaps peculiar to intelligent beings ; and (such as) some proofs that are drawn from the theology of the schools ; (which I wish less interwoven with *Aristotle's* philosophy.) For though there be some things which seem to be of this sort (as arguments drawn from final causes in divers particulars that concern animals) which in a sound sense I not only admit, but maintain ; yet since, as they are wont to be proposed, they are liable enough to be questioned, I thought it expedient for my present design to premit them, as things that I do not absolutely need ; though the employing some of them would facilitate my task. And this I did the rather, because I also forbore to answer arguments, that however vehemently and subtilly urged by many of the modern schoolmen of the Roman Catholick communion, are either confessedly, or at least really built upon some theological tenets of theirs, which being opposed by the divines of other churches, and not left unquestioned by some acute ones of their own, would not be proper to be solemnly taken notice of by me, whose business in this tract is to discourse of natural things as a naturalist, without invading the province of divines, by intermeddling with supernatural mysteries ; such as those, upon which divers of the physico-theological tenets of the schoolmen, especially about real qualities, and\* the

\* *Atque hæc sententia* (of the distinction and separableness of quantity from matter) *est omnino tenenda : quanquam enim non possit ratione naturali sufficienter demonstrari, tamen ex principiis theologicis convincitur esse vera, maxime propter mysterium eucharistiæ.* Suarez Disp. Metaph. 40. p. m. 341. paucis interjectis. — *Prima ratio pro hac sententia est, quia in mysterio eucharistiæ Deus separavit quantitatem à substantiis panis & vini, &c.* Et p. m. 342. — *Hæc responsio & sententia* [adversariorum] *sic explicata non potest facile & erodenter impugnari, sistendo in puro naturali ; nihilominus tamen, partim ratione naturali, partim adjuuncto mysterio sufficientissime improbat.*



separableness of accidents from objects of inhesion, are manifestly, if not also avowedly grounded. But to return to the other things I was owing to have left unmentioned, notwithstanding all that I have been saying, I readily acknowledge, that in some recent authors that have been imbracers of the new philosophy, I have met with some passages that might well and pertinently be taken into the following discourse, but that having been (as I formerly intimated) transcribed some years ago, I cannot now so conveniently alter it: which I am the less troubled at, because these few additional arguments, thought fit to illustrate or confirm, being not necessary to make out what has been delivered, may safely be let alone, unless there happen (as it is not unlikely there may) an occasion of reprinting these notes, with such enlargements as may make them the more fit to be an introduction into the corpuscular philosophy.

I HOPE then, upon the whole matter, that I have pitched upon that way that was the most conducive to my design, partly by insisting only on those opinions, whether true or false, which for their importance or difficulty seemed to deserve to be particularly either explicated or disproved; and partly by chusing to employ such arguments, as I thought the clearest and cogentest, and by their assuming the least of any, seemed the easiest to be vindicated from exceptions; without troubling myself to answer objections that appeared rather to be drawn from metaphysical or logical subtilties, or to be grounded upon the authority of men, than to be physical ratiocinations, founded upon experience, or the nature of the things under debate; especially having, in the proposal or confirmation of the truth, so laid the grounds, and imitated the ways of answering what is like to be colourably objected against it, that an ingenious man may well enough furnish himself with weapons to defend the truth, out of the notions, hints, and experiments, wherewith in this tract care has been taken to accompany. And by forbearing to prosecute some of the Peripatetick controversies any further than I have done, will not, I hope, be blamed by them that have observed as well as I, how much those disputes are wont to be lengthened by such frivolous distinctions, as do not deserve to be solemnly examined, especially in such a compendious treatise as ours. For an attentive reader needs not be much conversant with the writings of the modern Peripateticks about such subjects as substantial forms, generation, corruption, &c. to take notice, that it is their custom, when they find themselves distressed by a solid argument, to endeavour to elude it by some pious distinction or other; which is usually so groundless, and so unintelligible, or so nugatory, or so impertinent to the subject, or at least so insufficient for the purpose it is alledged for, that to vouchsafe it a solicitous confutation might question a writer's judgment with intelligent readers; who by such insignificant distinctions are satisfied of nothing so much, as that the framers of them had rather say (that which indeed amounts to) nothing, than not to seem to say something. And of such evasions they may probably be emboldened to make use, by the practice of *Aristotle* himself, to whom such obscure and unsatisfactory distinctions are so familiar, that I remember one of his own commentators\* (and he one of the most judicious) could not forbear, upon a certain text of his master's, to complain of it, and particularly to take notice, that that one distinction of *actu & potentia* runs through almost all *Aristotle's* philosophy, and is employed to shift off those difficulties he could not clearly explicate.

\* The author here meant is the inquisitive Peripatetick *Cabæus*, who in one place hath these words: *Ut hanc questionem solvat, recurrit ad illam distinctionem sibi valde familiarem, quæ utitur Aristoteles in rata sua philosophia, quoties obviam habet aliquam gravem difficultatem, distinguit enim actum vel potentiam* &c. In another these: — *Quæ est distinctio quedam familiaris Aristoteli, quam applicat omnibus rebus, ubi difficultates urgent, & videtur illis vocibus quasi fatali gladio omnes rescindere difficultatis nodos; vix enim est difficultas, cui non putat se satisfacere distinguendo actum & potentiam*.



By which nevertheless I would not be understood to censure or decry the whole Peripatetick philosophy, much less to despise *Aristotle* himself; whose own writings give me sometimes cause a little to wonder, to find some absurdities so confidently fathered upon him by his scholastic interpreters. For I look upon *Aristotle* as one (though but as one amongst many) of those famed ancients, whose learning about *Alexander's* time ennobled *Greece*; and I readily allow him most of the praises due to great wits, excepting those which belong to clear-headed naturalists. And I here declare once for all, that, where in the following tract, or any other of my writings, I do indefinitely depreciate *Aristotle's* doctrine, I would be understood to speak of his physicks, or rather of the speculative part of them (for his historical writings concerning animals I much esteem) nor do I say that even these may not have their use among scholars, and even in universities, if they be retained and studied with due cautions and limitations (of which I have elsewhere spoken).

BUT to resume the discourse whence the Peripatetick distinctions tempted me to digress; by any thing I formerly said I would not in the least disparage those excellent, and especially those modern authors that have professedly opposed the *Aristotelian* physicks (such as *Lucretius*, *Verulam*, *Basso*, *Des Cartes* and his followers, *Gassendus*, the two *Boots*, *Pagnenus*, *Pemle*, *Helmont*) nor to be thought to have made no use of any of their cogitations or arguments. For though some of their books I could not procure when I had occasion to have recourse to them; and though the weakness of my eyes discouraged me from perusing those parts of others that concerned not the subject I was treating of, yet I hope I have been benefited by those I have consulted, and might have been more so by the learned *Gassendus's* little, but ingenious, *Syntagma Philosophiæ Epicuri*, if I had more seasonably been acquainted with it.

BUT whether we have treated of the nature and origin of forms and qualities in a more comprehensive way than others; whether we have by new and fit similitudes and examples and other means rendered it more intelligible than they have done; whether we have added any considerable number of notions and arguments, towards the completing and confirming of the proposed hypothesis; whether we have with reason dismissed arguments unfit to be relied on; and whether we have proposed some notions and arguments so warily as to keep them from being liable to exceptions or evasions, whereto they were obnoxious as others have proposed them; whether (I say) we have done all or any of these in the first or speculative part of this treatise, we willingly leave the reader to judge. But in the second or historical part of it perhaps he will be invited to grant that we have done that part of physicks we have been treating of some little service; since by the lovers of real learning it was very much wished that the doctrines of the new philosophy (as it is called) were backed by particular experiments; the want of which I have endeavoured to supply, by annexing some whose nature and novelty, I am made believe, will render them as well acceptable as instructive. For though, that I might not anticipate what belongs to other papers, I did not make the last section consist of above a deced of them; and though, for the reasons intimated in the advertisements premised to them, I did not expressly mention to *Pyrophilus* all that I could have told him about them; yet I have been careful so to chuse them, and to interweave hints in delivering them, that a sagacious reader who shall have the curiosity to try them heedfully, and make reflections on the several phenomena that in likelihood will occur to him, will (if I mistake not) receive no contemptible information, as of some other things so particularly about the nature of mixtions (which I take to be one of the most important and useful, though neglected and ill understood doctrines of the practical part of physicks) and may probably light upon more than he expects, or I have fully delivered, and perhaps too more than I foresaw.

AND though some virtuosi, more conversant perhaps with things than books; presuming the decay of the Peripatetick philosophy to be every where as great as it is among them in *England*, may think that a doctrine which they look on as expiring, need not have been so solicitously confuted; yet those that know how deep rooting this philosophy has taken (both elsewhere, and particularly) in those academies where it has flourished for many ages, and in some of which it is, exclusively to the mechanical philosophy, watered and fenced by their statutes or their superiors: and he that also knows how much more easy some (more subtle than candid) wits find it plausibly to defend an error, than ingenuously to confess it, will not wonder that I should think that a doctrine so advantaged, though it be too erroneous to be feared, is yet too considerable to be despised. And not to question whether several of those that most condemn the favourers of the Peripatetick hypothesis, as the later discoveries have reduced them to reform it, be not the least provided to answer their arguments (not to question this, I say) there are divers of our adversaries (misled only by education and morally harmless prejudices) who do so much deserve a better cause, than that which needs all their subtilty, without being worthy of it, that I shall think more pains than I have taken very usefully bestowed, if my arguments and experiments prove so happy as to undeceive persons whose parts, too unluckily confined to narrow and fruitless notions, would render them illustrious champions for the truths they are able so subtilly to oppose; and who might questionless perform considerable things, if they employed as much dexterity to expound the mysteries of nature, as the riddles of the schoolmen, and laid out their wit and industry to surmount the obscurity of her works, instead of that of *Aristotle's*.

THERE might be a few other particulars fit to be taken notice of in this preface, but finding that I had already mentioned them in that which I had addressed to *Pyrophilus*, my haste makes me willing rather to refer the reader thither for them, than alter that, or lengthen this (which I should think much too long already, if it were not possible that it may hereafter prove preliminary to more papers than these it is now premised to). So that there remains but one advertisement necessary to be given here, namely, that whereas in the following notes I several times speak of the author of the essay of salt-petre, as of a third person, the occasion of that was, that when these notes, and some about particular qualities, were written, I had a design to make two distinct sorts of annotations upon that essay; in the former whereof (which now comes forth) I assumed the person of a Corpuscularian, and discoursed at that rate. But I had thoughts too (in case God were pleased to grant me life and opportunity) to take a second review both of the treatise itself, and of the notes on it, and on that occasion to add what my ripper thoughts and further experience might suggest unto me. And that in my animadversions I might with the more freedom and conveniency add, explain, alter, and even retract, as I should see cause, I thought it not amiss to write them, as if they were made on the work of another. By which intimation the reader may be assisted to guess how much I intended in the following discourse (in which, as in the prefaces belonging to it; I play the Corpuscularian) to reserve myself the freedom of questioning and correcting, upon the designed review, any thing delivered in these notes; and how much more it was in them my design to bring *Pyrophilus* experiments and queries to illustrate obscure matters, than by hasty assertions to dogmatize about them.



# T H E P R E F A C E.

**T**HE origin, *Pyrophilus*, and nature of the qualities of bodies, is a subject that I have long looked upon as one of the most important and useful that the naturalist can pitch upon for his contemplation. For the knowledge we have of the bodies without us, being for the most part fetched from the informations the mind receives by the senses, we scarce know any thing else in bodies, upon whose account they can work upon our senses, save their qualities: for as to the substantial forms which some imagine to be in all natural bodies, it is not half so evident that there are such as it is, that the wisest of those that do admit them confess, that they do not well know them\*. And as it is by their qualities that bodies act immediately upon our senses, so it is by virtue of those attributes likewise that they act upon other bodies, and by that action produce in them, and oftentimes in themselves, those changes that sometimes we call alterations, and sometimes generation or corruption.

And it is chiefly by the knowledge, such as it is, that experience (not art) hath taught us of these differing qualities of bodies, that we are enabled, by a due application of agents to patients, to exercise the little empire that we have either acquired or regained over the creatures. But I think not the contemplation of qualities more noble and useful than I find it difficult; for what is wont to be taught us of qualities in the schools, is so slight and ill-grounded, that it may be doubted whether they have not rather obscured than illustrated the things they should have explained. And I was quickly discouraged from expecting to learn much from them of the nature of divers particular qualities, when I found that, except some few, which they tell you in general may be deduced (by ways they leave those to guess at, that can) from those four qualities, they are pleased to call the first, they confess that the rest spring from those forms of bodies, whose particular natures the judiciousest of them acknowledge they cannot comprehend. And *Aristotle* himself not only doth (as we shall see anon) give us of quality in general (which yet seems far more easily definable than many a particular quality) no other than such a definition, as is as obscure as the thing to be declared by it; but I observe, not without some wonder, that in his eight books of *Physicks*, where he professedly treats of the general affections of natural things, he leaves out the doctrine of qualities; as after him *Magirus* and divers other writers of the *Peripatetic* physiology have done: which (by the way) I cannot but look upon as an omission, since qualities do as well seem to belong to natural bodies generally considered, as place, time, motion, and those other things, which upon that account are wont to be treated of in the general part of natural philosophy. The most ingenious *Des Cartes* has something concerning some qualities; but though, for reasons elsewhere expressed, I have purposely forbore to peruse his system of philosophy, yet I find by turning over the leaves that he has left most of the other qualities untreated of; and of those that are more properly called sensible, he speaks but very briefly and generally, rather considering what they do upon the organs of sense, than what changes happen in the objects themselves, to make them cause in us a perception sometimes of one quality and sometimes of another. Besides that his explications do many of them so depend upon his peculiar no-

\* *Nego tibi ullam esse formam nobis notam plenè & pland; nostramque scientiam esse umbram in se. Scaliger* (of whose confession to the same purpose more are cited hereafter).

tions (of a *materia subtilis, globuli secundi elementi* and the like) and these, as it became so great a person, he has so interwoven with the rest of his hypothesis, that they can seldom be made use of without adopting his whole philosophy. *Epicurus* indeed, and his scholiast *Lucretius*, have given some good hints concerning the nature of some few qualities. But, besides that even these explications are divers of them either doubtful or imperfect, or both, there are many other qualities which are left for others to treat of. And this is the second and main difficulty which I find in investigating the nature of qualities; namely, that whatever be to be thought of the general theories of *Aristotle* or other philosophers concerning qualities, we evidently want that upon which a theory, to be solid and useful, must be built; I mean an experimental history of them. And this we so want, that except perhaps what mathematicians have done concerning sounds, and the observations (rather than experiments) that our illustrious *Verulam* hath (in some few pages) said of heat in his short *Essay de Formâ Calidi*; I know not any one quality of which any author has yet given us an any-thing competent history. These things I mention to you, *Pyrophilus*, not at all to derogate from those great men whose design seems rather to have been to deliver principles and summaries of philosophy, than to insist upon particulars; but for this purpose, that since the nature of qualities is so beneficial a speculation, my labours may not be looked upon as wholly useless, though I can contribute but a little to the clearing of it; and that since it is so abstruse a subject, I may be pardoned if I sometimes miss the mark, and leave divers things uncompleted; that being but what such great philosophers have done before me.

BUT, *Pyrophilus*, before I proceed to give you my notes upon this part of our author's essay, that you may rightly understand my intention in them, it will be requisite to give you three or four advertisements.

AND first, Whenever I shall speak indefinitely of substantial forms, I would always be understood to except the reasonable soul that is said to inform the human body, which declaration I here desire may be taken notice of once for all.

SECONDLY, Nor am I willing to treat of the origin of qualities in beasts; partly because I would not be engaged to examine of what nature their souls are, and partly because it is difficult in most cases (at least for one that is compassionate enough) either to make experiments upon living animals, or to judge what influence their life may have upon the change of qualities produced by such experiments.

THIRDLY, The occasion of the following reflections being only this, that our author, in that part of his essay concerning salt-petre, whereto these notes refer, does briefly intimate some notions about the nature and origin of qualities; you must not expect that I, whose method leads me but to write some notes upon this and some other parts of this essay, should make solemn or elaborate discourses concerning the nature of particular qualities, and that I should fully deliver my own apprehensions concerning those subjects. For, as I elsewhere sufficiently intimate, that in these first notes I write as a Corporularian, and set down those things only that seem to have a tendency to illustrate or countenance the notions or fancies implied in our author's essay; so I must here tell you, that I neither have now the leisure, nor pretend to the skill, to deliver fully the history, or to explicate particularly the nature of each several quality.

FOURTHLY, But I consider that the schools have of late much amused the world with a way they have got of referring all natural effects to certain entities, that they call real qualities, and accordingly attribute to them a nature distinct from the modification of the matter they belong to, and in some cases separable from all matter whatsoever; by which means they have, as far forth as their doctrine is acquiesced in, made it thought needless or hopeless for men to employ their industry in searching into the nature of particular qualities and their effects. As if (for instance) it be demanded how snow comes



to dazzle the eyes, they will answer, that it is by a quality of whiteness that is in it which makes all very white bodies produce the same effect: and if you ask what this whiteness is, they will tell you no more in substance, than that it is a real entity which denominates the parcel of matter to which it is joined, white; and if you further inquire what this real entity, which they call a quality, is, you will find, as we shall see anon, that they either speak of it much after the same rate that they do of their substantial forms (as indeed some of the modernest teach, that a quality affects the matter it belongs to *per modum formæ secundariæ*, as they speak) or at least they will not explicate it more intelligibly.

AND accordingly, if you further ask them how white bodies in general do rather produce this effect of dazzling the eyes, than green or blue ones, instead of being told that the former sort of bodies reflect outwards, and so to the eye far more of the incident light than the latter; you shall perchance be told that it is their respective natures so to act. By which way of dispatching difficulties they make it very easy to solve all the phenomena of nature in general, but make men think it impossible to explicate almost any of them in particular.

AND though the unsatisfactoriness and barrenness of the school-philosophy have persuaded a great many learned men, especially physicians, to substitute the chymists three principles instead of those of the schools; and though I have a very good opinion of chymistry itself, as it is a practical art; yet, as it is by chymists pretended to contain a system of theoretical principles of philosophy, I fear it will afford but a very little satisfaction to a severe inquirer into the nature of qualities. For besides that, as we shall more particularly see anon, there are many qualities which cannot with any probability be deduced from any of the three principles; those that are ascribed to one or other of them cannot intelligibly be explicated without recourse to the more comprehensive principles of the Corpuscularian philosophy: to tell us, for instance, that all solidity proceeds from salt, only informing us (where it can plausibly be pretended) in what material principle or ingredient that quality resides, not how it is produced; for this doth not teach us (for example) how water even in exactly-closed vessels comes to be frozen into ice; that is, turned from a fluid to a solid body, without the accession of a saline ingredient (which I have not yet found pretended) especially glass being held impervious to salts). Wherefore, *Pyrophilus*, I thought it might much conduce to the understanding the nature of qualities, to shew how they are generated; and by the same way I hoped it might remove in some measure the obstacle, that these dark and narrow theories of the Peripateticks and Chymists may prove to the advancement of solid and useful philosophy. That then which I chiefly aim at, is to make it probable to you by experiments (which I think hath not yet been done) that almost all sorts of qualities, most of which have been by the schools either left unexplicated, or generally referred to I know not what incomprehensible substantial forms, may be produced mechanically; I mean by such corporeal agents, as do not appear either to work otherwise than by virtue of the motion, size, figure, and contrivance of their own parts (which attributes I call the mechanical affections of matter, because to them men willingly refer the various operations of mechanical engines): or to produce the new qualities exhibited by those bodies, their action changes by any other way, than by changing the texture or motion, or some other mechanical affection of the body wrought upon. And this if I can in any passable measure do, though but in a general way, in some or other of each of these three sorts into which the Peripateticks are wont to divide the qualities of bodies, I hope I shall have done no useless piece of service to natural philosophy; partly by exciting you and your learned friends to inquire after more intelligible and satisfactory ways of explicating qualities, and partly by beginning such a

collection of materials towards the history of those qualities, that I shall the most largely insist on, as heat, colours, fluidity, and firmness, as may invite you and other ingenious men to contribute also their experiments and observations to so useful a work, and thereby lay a foundation whereon you, and perhaps I, may superstruct a more distinct and explicate theory of qualities than I shall at present adventure at. And though I know that some of the things my experiments tend to manifest, may likewise be confirmed by the more obvious phenomena of nature, yet I presume you will not dislike my chusing to entertain you with the former (though without at all despising, or so much as strictly forbearing to imploy the latter) because the changes of qualities made by our experiments will, for the most part, be more quick and conspicuous; and the agents made use of to produce them being of our own applying, and oftentimes of our own preparation, we may be therefore assisted the better to judge of what they are, and to make an estimate of what it is they do.

## C O N S I D E R A T I O N S

A N D

## E X P E R I M E N T S

T O U C H I N G T H E

## O R I G I N of FORMS and QUALITIES.

## T H E T H E O R I C A L P A R T.

**T**HAT, before I descend to particulars, I may, *Pyrophilus*, furnish you with some general apprehension of the doctrine (or rather the hypothesis) which is to be collated with, and to be either confirmed or disproved by the historical truths that will be delivered concerning particular qualities (and forms); I will assume the person of a Corpuscularian, and here at the entrance give you (in a general way) a brief account of the hypothesis itself, as it concerns the origin of qualities (and forms); and for distinction's sake, I shall comprize it in the eight following particulars, which, that the whole scheme may be the better comprehended, and as it were surveyed under one prospect, I shall do little more than barely propose them that either seem evident enough by their own light, or may without prejudice have divers of their proofs reserved for proper places in the following part of this treatise. And though there be some other particulars to which the importance of the subjects, and the greatness of the (almost universal) prejudices that lie against them, will oblige me immediately to annex (for the seasonable clearing and justifying of them) some annotations; yet that they



they may, as little as I can, obscure the coherence of the whole discourse, as much of them as conveniently may be shall be included in [ ] paratheses.

I. I AGREE with the generality of philosophers so far as to allow, that there is one catholick or universal matter common to all bodies, by which I mean a substance extended, divisible, and impenetrable.

II. BUT because this matter being in its own nature but one, the diversity we see in bodies must necessarily arise from somewhat else than the matter they consist of. And since we see not how there could be any change in matter, if all its (actual or designable) parts were perpetually at rest among themselves, it will follow, that to discriminate the catholick matter into variety of natural bodies, it must have motion in some or all its designable parts: and that motion must have various tendencies, that which is in this part of the matter tending one way, and that which is in that part tending another; as we plainly see in the universe or general mass of matter, there is really a great quantity of motion, and that variously determined, and that yet divers portions of matter are at rest.

THAT there is local motion in many parts of matter is manifest to sense; but how matter came by this motion was of old, and is still hotly disputed of: for the antient Corpuscularian philosophers (whose doctrine in most other points, though not in all, we are most inclinable to) not acknowledging an Author of the universe, were thereby reduced to make motion congenite to matter, and consequently coeval with it. But since local motion, or an endeavour at it, is not included in the nature of matter, which is as much matter when it rests as when it moves; and since we see that the same portion of matter may from motion be reduced to rest, and after it hath continued at rest, as long as other bodies do not put it out of that state, may by external agents be set a moving again; I, who am not wont to think a man the worse naturalist for not being an atheist, shall not scruple to say with an eminent philosopher of old, whom I find to have proposed among the *Greeks* that opinion (for the main) that the excellent *Des Cartes* has revived amongst us, that the origin of motion in matter is from God; and not only so, but that thinking it very unfit to be believed that matter barely put into motion, and then left to itself, should casually constitute this beautiful and orderly world: I think also further, that the wise Author of things did, by establishing the laws of motion among bodies, and by guiding the first motions of the small parts of matter, bring them to convene after the manner requisite to compose the world, and especially did contrive those curious and elaborate engines, the bodies of living creatures, endowing most of them with a power of propagating their species. But though these things are my persuasions, yet, because they are not necessary to be supposed here, where I do not pretend to deliver any compleat discourse of the principles of natural philosophy, but only to touch upon such notions as are requisite to explicate the origin of qualities and forms, I shall pass on to what remains, as soon as I have taken notice that local motion seems to be indeed the principal amongst second causes, and the grand agent of all that happens in nature: for though bulk, figure, rest, situation, and texture do concur to the phenomena of nature, yet in comparison of motion they seem to be in many cases, effects, and in many others little better than conditions, or requisites, or causes *sine quibus non*, which modify the operation that one part of matter by virtue of its motion hath upon another; as in a watch, the number, the figure, and coaptation of the wheels and other parts is requisite to the shewing the hour, and doing the other things that may be performed by the watch; but till these parts be actually put into motion, all their other affections remain inefficacious. And so in a key, though it were too big or too little, or if its shape were incongruous to that of the cavity of the lock, it would be unfit to be used as a key though it were put into motion; yet, let its bigness and figure

be never so fit, unless actual motion intervene, it will never lock or unlock any thing, as without the like actual motion, neither a knife nor razor will actually cut, how much soever their shape and other qualities may fit them to do so. And so brimstone, what disposition of parts soever it have to be turned into flame, would never be kindled, unless some actual fire or other parcel of vehemently and variously agitated matter should put the sulphureous corpuscles into a very brisk motion.

III. THESE two grand and most catholick principles of bodies, matter and motion, being thus established, it will follow, both that matter must be actually divided into parts, that being the genuine effect of variously determined motion, and that each of the primitive fragments, or other distinct and intire masses of matter, must have two attributes, its own magnitude, or rather size, and its own figure or shape. And since experience shews us (especially that which is afforded us by chymical operations, in many of which matter is divided into parts too small to be singly sensible) that this division of matter is frequently made into insensible corpuscles or particles, we may conclude, that the minutest fragments, as well as the biggest masses of the universal matter, are likewise endowed each with its peculiar bulk and shape. For being a finite body, its dimensions must be terminated and measurable: and though it may change its figure, yet for the same reason it must necessarily have some figure or other. So that now we have found out, and must admit three essential properties of each intire or undivided, though insensible part of matter; namely, magnitude (by which I mean not quantity in general, but a determined quantity, which we in *English* oftentimes call the size of a body) shape, and either motion or rest (for betwixt them two there is no mean): the two first of which may be called inseparable accidents of each distinct part of matter; inseparable, because being extended, and yet finite, it is physically impossible that it should be devoid of some bulk or other, and some determinate shape or other; and yet accidents, because that whether or no the shape can by physical agents be altered, or the body subdivided, yet mentally both the one and the other may be done, the whole essence of matter remaining undestroyed.

WHETHER these accidents may not conveniently enough be called the moods or primary affections of bodies, to distinguish them from those less simple qualities (as colours, tastes, and odours) that belong to bodies upon their account; or whether, with the *Epicureans*, they may not be called the conjuncts of the smallest parts of matter, I shall not now stay to consider: but one thing the modern schools are wont to teach concerning accidents, is too repugnant to our present doctrine to be in this place quite omitted; namely, that there are in natural bodies store of real qualities and other real accidents, which not only are no moods of matter, but are real entities distinct from it, and, according to the doctrine of many modern schoolmen, may exist separate from all matter whatsoever. To clear this point a little, we must take notice, that accident is among logicians and philosophers used in two several senses; for sometimes it is opposed to the fourth predicable (property) and is then defined; That, which may be present or absent without the destruction of the subject; as a man may be sick or well, and a wall white or not white, and yet the one be still a man, the other a wall: and this is called in the schools *accidens predicabile*, to distinguish it from what they call *accidens predicamentale*, which is opposed to substance: for when things are divided by logicians into ten predicaments or highest genus's of things, substance-making one of them, all the nine other are of accidents. And as substance is commonly defined to be a thing that subsists of itself, and is the subject of accidents (or more plainly a real entity or thing that needs not any (created) being, that it may exist): so an accident is said commonly to be *id cuius esse est in esse*; and therefore *Aristotle*, who usually calls substances simply *ὄντα*, entities, most commonly calls accidents *ὀντοειδῆ ὄντα*, entities of entities; these  
needing



needing the existence of some substance or other, in which they may be, as in their subject of inhesion. And because logicians make it the discriminating note of substance and accident, that the former is a thing that cannot be in another, as in its subject of inhesion, it is requisite to know, that, according to them, that is said to be in a subject which hath these three conditions; That however it (1) be in another thing (2) is not in it as a part, and (3) cannot exist separately from the thing or subject wherein it is: as a white wall is the subject of inhesion of the whiteness we see in it, which self-same whiteness, though it be not in the wall as a part of it, yet cannot the self-same whiteness, according to our logicians, exist any where out of the wall, though many other bodies may have the like degree of whiteness. This premised, it will not be hard to discover the falsity of the lately mentioned scholastick opinion touching real qualities and accidents, their doctrine about which does, I confess, appear to me to be either unintelligible, or manifestly contradictory. For speaking in a physical sense, if they will not allow these accidents to be modes of matter, but entities really distinct from it, and in some cases separable from all matter, they make them indeed accidents in name, but represent them under such a notion as belongs only to substances; the nature of a substance consisting in this, that it can subsist of itself without being in any thing else, as in a subject of inhesion. So that to tell us that a quality or other accident may subsist without a subject, is indeed, whatever they please to call it, to allow it the true nature of substance; nor will their groundless distinctions do any more than keep them from seeming to contradict themselves in words, whilst unprepossessed persons see that they do it in effect. Nor could I ever find it intelligibly made out what these real qualities may be that they deny to be either matter or modes of matter, or immaterial substances. When a bowl runs along or lies still, that motion or rest, or globous figure of the bowl is not nothing, and yet is not any part of the bowl; whose whole substance would remain, though it wanted which you please of these accidents: and to make them real and physical entities (for we have not here to do either with logical or metaphysical ones) is, as if, because we may consider the same man sitting, standing, running, thirsty, hungry, weary, &c. we should make each of these a distinct entity, as we do give some of them (as hunger, weariness, &c.) distinct names; whereas the subject of all these qualities is but the same man, as he is considered with circumstances that make him appear different in one case from what he appears in the other: and it may be very useful to our present scope to observe, that not only diversity of names, but even diversity of definitions, doth not always infer a diversity of physical entities in the subject whereunto they are attributed. For it happens in many of the physical attributes of a body, as in those other cases wherein a man that is a father, a husband, a master, a prince, &c. may have a peculiar definition (such as the nature of the thing will bear) belong unto him in each of these capacities; and yet the man in himself considered is but the same man, who, in respect of differing capacities, or relations to other things, is called by differing names, and described by various definitions, which yet (as I was saying) conclude not so many real and distinct entities in the person so variously denominated.

## An EXCURSION about the relative Nature of PHYSICAL QUALITIES.

**B**UT, because I take this notion to be of no small importance towards the avoiding of the grand mistake that hath hitherto obtained about the nature of qualities, it will be worth while to illustrate it a little farther. We may consider then that when *Tubal-Cain*, or whoever else were the smith that invented locks and keys, had made his first lock (for we may reasonably suppose him to have made that before the key, though the comparison be made use of without that supposition) that was only a piece of iron contrived into such a shape; and when afterwards he made a key to that lock, that also in itself considered was nothing but a piece of iron of such a determinate figure: but in regard that these two pieces of iron might now be applied to one another after a certain manner, and that there was a congruity betwixt the wards of the lock and those of the key, the lock and the key did each of them now obtain a new capacity; and it became a main part of the notion and description of a lock, that it was capable of being made to lock or unlock by that piece of iron we call a key, and it was looked upon as a peculiar faculty and power in the key, that it was fitted to open and shut the lock; and yet by these new attributes there was not added any real or physical entity either to the lock or to the key, each of them remaining indeed nothing but the same piece of iron, just so shaped, as it was before. And when our smith made other keys of different bignesses or with different wards, though the first lock was not to be opened by any of those keys, yet that indisposition, however it might be considered as a peculiar power of resisting this or that key, and might serve to discriminate it sufficiently from the locks those keys belonged to, was nothing new in the lock, or distinct from the figure it had before those keys were made. To carry this comparison a little farther, let me add, that though one that would have defined the first lock and the first key would have given them distinct definitions with reference to each other; and yet (as I was saying) these definitions being given but upon the score of certain respects, which the defined bodies had one to another, would not infer that these two iron instruments did physically differ otherwise than in the figure, size, or contrivement of the iron whereof each of them consisted. And proportionably hereunto, I do not see why we may not conceive, that as to those qualities (for instance) which we call sensible, though, by virtue of a certain congruity or incongruity in point of figure (or texture or other mechanical attributes) to our sensories, the portions of matter they modify are enabled to produce various effects, upon whose account we make bodies to be endowed with qualities; yet they are not in the bodies that are endowed with them, any real or distinct entities, or differing from the matter itself, furnished with such a determinate bigness, shape, or other mechanical modifications. Thus, though the modern goldsmiths and refiners reckon amongst the most distinguishing qualities of gold, by which men may be certain of its being true and not sophistified, that it is easily dissoluble in aqua regis, and that aqua fortis will not work upon it; yet these attributes are not in the gold any thing distinct from its peculiar texture, nor is the gold we have now of any other nature than it was in *Pliny's* time, when aqua fortis and aqua regis had not been found out (at least in these parts of the world) and were utterly unknown to the *Roman* goldsmiths. And this example I have the rather pitched upon, because it affords me an opportunity to represent, that unless we admit the doctrine I have been proposing, we must admit, that a body may have an almost infinite number of new real entities accruing to



to it without the intervention of any physical change in the body itself. As for example, gold was the same natural body immediately before aqua regis and aqua fortis were first made, as it was immediately after; and yet now it is reckoned amongst its principal properties, that it is dissoluble by the former of those two menstrooms, and that it is not like other metals dissoluble or corrodible by the latter. And if one should invent another menstruum (as possibly I may think myself master of such a one) that will but in part dissolve pure gold, and change some part of it into another metalline body, there will then arise another new property, whereby to distinguish that from other metals; and yet the nature of gold is not a whit other now than it was before this last menstruum was first made. There are some bodies not cathartick nor sudorifick, with some of which gold being joined acquires a purgative virtue, and with others a power to procure sweat; and, in a word, nature herself doth sometimes otherwise, and sometimes by chance produce so many things that have new relations unto others: and art, especially assisted by chymistry, may, by variously dissipating natural bodies, or compounding either them or their constituent parts with one another, make such an innumerable company of new productions, that will each of them have new operations either immediately upon our sensories, or upon other bodies whose changes we are able to perceive, that no man can know but that the most familiar bodies may have multitudes of qualities that he dreams not of; and a considering man will hardly imagine that so numerous a croud of real physical entities can accrue to a body, whilst in the judgment of all our senses it remains unchanged and the same that it was before.

To clear this a little farther, we may add, that beaten glass is commonly reckoned among poisons; and (to skip what is mentioned out of *Sanctorius* of the dysentery procured by the fragments of it) I remember \* *Cardan* hath a story, that in a cloister where he had a patient then like to die of torments in the stomach, two other nuns had been already killed by a distracted woman that, having casually got free, had mixed beaten glass with peas that were eaten by these three and divers others of the sisters (who yet escaped unharmed). Now though the powers of poisons be not only looked upon as real qualities, but are reckoned amongst the abstrusest ones; yet this deleterious faculty, which is supposed to be a peculiar and superadded entity in the beaten glass, is really nothing distinct from the glass itself (which, though a concrete made up of those innocent ingredients, salt and ashes, is yet a hard and stiff body) as it is furnished with that determinate bigness and figure of parts which have been acquired by comminution; for these glassy fragments being many and rigid, and somewhat small (without yet being so small as dust) and endowed with sharp points and cutting edges, are enabled by these mechanical affections to pierce or wound the tender membranes of the stomach and guts, and cut the slender vessels that they may meet with there; whereby naturally ensue great gripings and contorsions of the injured parts, and oftentimes bloody fluxes, occasioned by the perforation of the capillary arteries and the great irritation of the expulsive faculty, and sometimes also not only horrid convulsions, by consent of the brain and cerebellum with some of the nervous or membranous parts that happen to be hurt, but also dropsies, occasioned by the great loss of blood we were just now speaking of. And it agrees very well with this conjecture, that beaten glass hath divers times been observed to have done no mischief to animals that have swallowed it: for there is no reason it should, in case the corpuscles of the powder either chance to be so small as not to be fit to wound the guts, which are usually lined with a slimy substance, wherein very minute powders may be as it were sheathed, and by that means hindered from hurting the guts (insomuch that a fragment of glass with three

\* *Carden. Contradiſt.* 9. lib. 2. tract. 5. apud *Schenkiu*.



very sharp corners hath been observed to have for above eighteen months \* Iain inoffensive even in a nervous and very sensible part of the body) out of which they may, with the grosser excrements of the lower belly, be harmlessly excluded, especially in some individuals, whose guts and stomach too may be of a much stronger texture and better lined or stuffed with gross and slimy matter than those of others. And accordingly we see that the fragments of sapphires, crystals, and even rubies, which are much harder than glass, are innocently, though perhaps not very effectually, used by physicians (and I have several times taken that without inconvenience) in cordial compositions, because of their being by grinding reduced to a powder too subtle to excoriate or grate upon the stomach or guts; and probably it was upon some such account that that happened which is related by *Cardan* in the same place; namely, that though the three nuns we have been speaking of were poisoned by the glass, yet many others who eat of the other portions of the same mingled peas received no mischief thereby. (But of this subject more † elsewhere).

AND this puts me in mind to add, that the multiplicity of qualities that are sometimes to be met with in the same natural bodies needs not make men reject the opinion we have been proposing, by persuading them that so many differing attributes as may be sometimes found in one and the same natural body, cannot proceed from the bare texture and other mechanical affections of its matter. For we must consider each body, not barely, as it is in itself, an entire and distinct portion of matter, but as it is a part of the universe, and consequently placed among a great number and variety of other bodies, upon which it may act, and by which it may be acted on, in many ways (or upon many accounts) each of which men are wont to fancy as a distinct power or quality in the body by which those actions, or in which those passions, are produced. For if we thus consider things, we shall not much wonder that a portion of matter that is indeed endowed but with a very few mechanical affections, as such a determinate texture and motion, but is placed among a multitude of other bodies that differ in those attributes from it and one another, should be capable of having a great number and variety of relations to those other bodies, and consequently should be thought to have many distinct inherent qualities by such as look upon those several relations or respects it may have to bodies without it, as real and distinct entities implanted in the body itself. When a curious watch is going, though the spring be that which puts all the parts into motion, yet we do not fancy (as an *Indian* or *Chinese* would perchance do) in this spring one faculty to move the index uniformly round the dial-plate, another to strike the hour, and perhaps a third to give an alarm, or shew the age of the moon or the tides; all the action of the spring (which is but a flexible piece of steel forcibly coiled together) being but an endeavour to dilate or unbind itself, and the rest being performed by the various respects it hath to the several bodies (that compose the watch) among which it is placed, and which they have one to another. We all know that the sun hath a power to harden clay, and soften wax, and melt butter, and thaw ice, and turn water into vapours, and make air expand itself in weather-glasses, and contribute to blanch linen, and make the white skin of the face swarthy, and mowed grass yellow, and ripen fruit, hatch the eggs of silk-worms, caterpillars, and the like in-

\* This memorable accident happened to a senator of *Berne*, who was cured by the experienced *Fabricius Hildanus*, that gives a long account of it to the learned *Horspius*, among whose observations it is extant (*lib. 2. observ. 35.*) who ascribes the indolence of the part, whilst uncompresssed, to some slimy juice (familiar enough to those tendinous parts) wherein the glassy fragment was as it were bedded.

† In those notes about occult qualities, where the deleterious faculty attributed to diamonds is considered.



fects, and perform I know not how many other things, divers of which seem contrary effects; and yet these are not distinct powers or faculties in the sun, but only the productions of its heat (which itself is but the brisk and confused local motion of the minute parts of a body) diversified by the differing textures of the body that it chances to work upon, and the condition of the other bodies that are concerned in the operation. And therefore, whether the sun in some cases have any influence at all distinct from its light and heat, we see that all those phaenomena we have thought fit to name, are producible by the heat of the common culinary fire duly applied and regulated. And so, to give an instance of another kind, when some years since, to try some experiments about the propagation of motion with bodies less capable of being battered by one another than those that have been formerly employed, I caused some solid balls of iron, skilfully hardened and exquisitely shaped and glazed, to be purposely made; each of these polished balls was a spherical looking-glass, which, placed in the midst of a room, would exhibit the images of the objects round about it in a very regular and pleasing perspective. It would contract the image and reflect the beams of the sun after a manner differing from flat and from convex looking-glasses. It would in a neat perspective lessen the image of him that looked upon it, and bent it, and it would shew that image as if it were behind the surface and within the solid substance of the sphere; and in some it had all those distinct, and some of them wonderful properties, which either ancient or modern writers of catoptricks have demonstrated to belong to spherical specula, as such: and yet the globe, furnished with all these properties and affections, was but the iron itself reduced by the artificer to a spherical figure; (for the glass that made it specular was not distinct from the superficial parts of the iron, reduced all of them to a physically equal distance from the center). And of specula, spherical enough as to sense, you may make store in a trice, by breaking a large drop of quick-silver into several little ones, each of which will serve for objects placed pretty near it, and the smaller of which (being the least depressed in the middle by their own weight, and consequently more perfectly globous) may, with a good microscope placed in a window, afford you no unpleasant prospect of the neighbouring objects; and yet to reduce a parcel of stagnant quick-silver, which will much emulate a flat looking-glass, into many of these little spherical specula, whose properties are so differing from those of plain ones, there intervenes nothing but a slight local motion, which, in the twinkling of an eye, changeth the figure of the self-same matter.

I HAVE said thus much, *Pyrophilus*, to remove the mistake, that every thing men are wont to call a quality must needs be a real and physical entity, because of the importance of the subject; and yet I have omitted some things that might have been pertinently added, partly because I may hereafter have opportunity to take them in, and partly because I would not any farther lengthen this excursion, which yet I must not conclude, till I have added this short advertisement.

THAT I have chosen to declare what I mean by qualities, rather by examples than definitions, partly because being immediately or reductively the objects of sense, men generally understand pretty well what one another mean when they are spoken of: as to say, that the taste of such a thing is saline or sour, or that such a sound is melodious, shrill, or jarring (especially if, when we speak of sensible qualities, we add some enumeration of particular subjects wherein they do the most eminently reside) will make a man as soon understood as if he should go about to give logical definitions of those qualities; and partly because the notions of things are not yet so well stated and agreed on, but that it is many times difficult to assign their true genus's. And *Aristotle* himself doth not only define accidents without setting down their genus, but when he comes to define qualities, he tells us, that quality is that by which a thing is said to be *qualis*; where



where I would have you take notice, both that in his definition he omits the genus, and that it is no such easy thing to give a very good definition of qualities, since he that is reputed the great master of logick, where he pretends to give us one, doth but upon the matter define the thing by the same thing: for it is supposed to be as little known what *qualis* is, as what *qualitas* is; and methinks he does just as if I should define whiteness to be that, for which a thing is called white, or virtue that, for which a man is said to be virtuous\*. Besides that, I much doubt whether his definition be not untrue as well as obscure: for, to the question, *Qualis res est?* answer may be returned out of some, if not all, of the other predicaments of accidents; which some of the modern logicians being aware of, they have endeavoured to salve the matter with certain cautions and limitations, which, however they may argue the devisers to be ingenious, do, for aught I can discern, leave us still to seek for a right and intelligible definition of quality in general; though to give such a one be probably a much easier task than to define many qualities that may be named in particular, as saltiness, sourness, green, blue, and many others, which, when we hear named, every man knows what is meant by them, though no man (that I know of) hath been able to give accurate definitions of them.

IV. AND if we should conceive that all the rest of the universe were annihilated, except any of these intire and undivided corpuscles (treated of in the 3d particular foregoing) it is hard to say what could be attributed to it, besides matter, motion (or rest) bulk, and shape. Whence by the way you may take notice that bulk, though usually taken in a comparative sense, is in our sense an absolute thing, since a body would have it, though there were no other in the world. But now there being actually in the universe great multitudes of corpuscles mingled among themselves, there arise in any distinct portion of matter, which a number of them make up, two new accidents or events: the one doth more relate to each particular corpuscle in reference to the (really or supposed) stable bodies about it, namely its posture (whether erected, inclined, or horizontal): and when two or more of such bodies are placed one by another, the manner of their being so placed, as one besides another, or one behind another, may be called their order; as I remember, *Aristotle* in his *Metaphysics*, lib. 1. cap. 4. recites this example out of the ancient Corpuscularians, that A and N differ in figure, and A N and N A in order, Z and N in situation: and indeed posture and order seem both of them reducible to situation. And when many corpuscles do so convene together as to compose any distinct body, as a stone or a metal, then from their other accidents (or modes) and from these two last mentioned, there doth emerge a certain disposition or contrivance of parts in the whole, which we may call the texture of it.

V. AND if we should conceive all the rest of the universe to be annihilated save one such body, suppose a metal or a stone, it were hard to shew that there is physically any thing more in it than matter, and the accidents we have already named. But now we

\* Since the writing of this the author found that some of the eminentest of the modern schoolmen themselves have been as well as he unsatisfied with the *Aristotelian* definition of quality: concerning which (not to mention *Revisus*, a learned Protestant annotator upon *Suarez*) *Arriaga* says (*disp.* 5. s. 2. sub. 1.) *Per hanc nihil explicatur; nam de hoc querimus, quid sit esse quale, dices habere qualitatem; bonus circulus: qualitas est id, quo quis fit qualis, & esse qualem est habere qualitatem.* And even the famous Jesuit *Suarez*, though he endeavours to excuse it, yet confesseth that it leaves the proper notion of quality as obscure to us as before: (*Quæ definitio, scilicet he, licet ea ratione essentialis videatur, quæ detur per habitudinem ad effectum formalem, quem omnis forma essentialiter respicit, tamen quæ ad nos spectat, æquæ obscura nobis manet propria ratio qualitatis.*) *Suarez* disputat. metaphys. 42. But *Hurtadus* (in his met. physical disputations) speaks more boldly, telling us roundly, that it is *non tam definitio, quàm inanis quædam nugatio*; which makes me the more wonder that a famous *Cartesian* (whom I forbear to name) should content himself to give us such an insignificant or at least superficial definition of quality.



are to consider, that there are *de facto* in the world certain sensible and rational beings that we call men; and the body of man having several external parts, as the eye, the ear, &c. each of a distinct and peculiar texture, whereby it is capable to receive impressions from the bodies about it, and upon that account it is called an organ of sense; we must consider, I say, that these sensories may be wrought upon by the figure, shape, motion, and texture of bodies without them after several ways, some of those external bodies being fitted to affect the eye, others the ear, others the nostrils, &c. And to these operations of the objects on the sensories, the mind of man, which upon the account of its union with the body, perceives them, giveth distinct names, calling the one light or colour, the other sound, the other odour, &c. And because also each organ of sense, as the eye, or the palate, may be itself differently affected by external objects, the mind likewise gives the objects of the same sense distinct appellations, calling one colour green, the other blue, and one taste sweet, and another bitter, &c. Whence men have been induced to frame a long catalogue of such things, as, for their relating to our senses, we call sensible qualities; and because we have been conversant with them before we had the use of reason, and the mind of man is prone to conceive almost every thing (nay, even privations, as blindness, death, &c.) under the notion of a true entity or substance, as itself is; we have been from our infancy apt to imagine that these sensible qualities are real beings in the objects they denominate, and have the faculty or power to work such and such things; as gravity hath a power to stop the motion of a bullet shot upwards, and carry that solid globe of matter toward the center of the earth; whereas indeed (according to what we have largely shewn above) there is in the body, to which these sensible qualities are attributed, nothing of real and physical but the size, shape, and motion or rest, of its component particles, together with that texture of the whole, which results from their being so contrived as they are; nor is it necessary they should have in them any thing more, like to the ideas they occasion in us, those ideas being either the effect of our prejudices or inconsiderateness, or else to be fetched from the relation that happens to be betwixt those primary accidents of the sensible object and the peculiar texture of the organ it affects: as when a pin being run into my finger causeth pain, there is no distinct quality in the pin answerable to what I am apt to fancy pain to be, but the pin in itself is only slender, stiff, and sharp, and by those qualities happens to make a solution of continuity in my organ of touching, upon which, by reason of the fabrick of the body, and the intimate union of the soul with it, there ariseth that troublesome kind of perception which we call pain, and I shall anon more particularly shew how much that depends upon the peculiar fabrick of the body.

VI. But here I foresee a difficulty, which being perhaps the chiefest that we shall meet with against the corpuscular hypothesis, it will deserve to be, before we proceed any farther, taken notice of. And it is this, that whereas we explicate colours, odours, and the like sensible qualities by a relation to our senses, it seems evident that they have an absolute being irrelative to us: for snow (for instance) would be white, and a glowing coal would be hot, though there were no man or any other animal in the world. And it is plain that bodies do not only by their qualities work upon our senses, but upon other, and those inanimate, bodies; as the coal will not only heat or burn a man's hand if he touch it, but would likewise heat wax (even so much as to melt it and make it flow) and thaw ice into water, although all the men and sensitive beings in the world were annihilated. To clear this difficulty, I have several things to represent: and,

1. I say not that there are no other accidents in bodies than colours, odours, and the like; for I have already taught that there are simpler and more primitive affections of



of matter, from which these secondary qualities, if I may so call them, do depend : and that the operations of bodies upon one another spring from the same, we shall see by and by.

2. NOR do I say that all qualities of bodies are directly sensible ; but I observe that when one body works upon another, the knowledge we have of their operation proceeds either from some sensible quality, or some more catholick affection of matter, as motion, rest, or texture, generated or destroyed in one of them ; for else it is hard to conceive how we shall come to discover what passes betwixt them.

3. WE must not look upon every distinct body that works upon our senses as a bare lump of matter of that bigness and outward shape that it appears of ; many of them having their parts curiously contrived, and most of them perhaps in motion too. Nor must we look upon the universe that surrounds us as upon a moveless and undistinguished heap of matter, but as upon a great engine, which having either no vacuity, or none that is considerable betwixt its parts (known to us) the actions of particular bodies upon one another must not be barely estimated, as if two portions of matter of their bulk and figure were placed in some imaginary space beyond the world, but as being situate in the world, constituted as it now is, and consequently as having in their action upon each other liable to be promoted or hindered or modified by the actions of other bodies besides them : as in a clock, a small force applied to move the index to the figure of XII will make the hammer strike often and forcibly against the bell, and will make a far greater commotion among the wheels and weights, than a far greater force would do, if the texture and contrivance of the clock did not abundantly contribute to the production of so great an effect. And in agitating water into froth, the whiteness would never be produced by that motion, were it not that the sun or other lucid body shining upon that aggregate of small bubbles enables them to reflect confusedly great store of little and as it were contiguous lucid images to the eye. And so the giving to a large metalline speculum a concave figure would never enable it to set wood on fire, and even to melt down metals readily, if the sun-beams, that in cloudless days do, as to sense, fill the air, were it not by the help of that concavity thrown together to a point. And to shew you by an eminent instance how various and how differing effects the same action of a natural agent may produce according to the several dispositions of the bodies it works upon, do but consider that in two eggs, the one prolifick, the other barren, the sense can perhaps distinguish before the incubation no difference at all ; and yet these bodies outwardly so like, do differ in the internal disposition of their parts, that if they be both exposed to the same degree of heat (whether of a hen or an artificial oven) that heat will change the one into a putrid and stinking substance, and the other into a chick, furnished with great variety of organical parts of very differing consistencies, and curious as well as differing textures.

4. I do not deny but that bodies may be said in a very favourable sense to have those qualities we call sensible, though there were no animals in the world : for a body in that case may differ from those bodies which now are quite devoid of quality, in its having such a disposition of its constituent corpuscles, that in case it were duly applied to the sensory of an animal, it would produce such a sensible quality which a body of another texture would not : as though if there were no animals there would be no such thing as pain, yet a pin may, upon the account of its figure, be fitted to cause pain, in case it were moved against a man's finger ; whereas a bullet, or other blunt body, moved against it with no greater force, will not cause any such perception of pain. And thus snow, though, if there were no lucid body nor organ of sight in the world, it would exhibit no colour at all (for I could not find it had any in places exactly darkened) yet it hath a greater disposition than a coal or foot, to reflect store of light outwards, when  
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the sun shines upon them all three. And so we say, that a lute is in tune whether it be exactly played upon or no, if the strings be all so duly stretched as that it would appear to be in tune, if it were played upon. But as if you should thrust a pin into a man's finger, both a while before and after his death, though the pin be as sharp at one time as at another, and maketh in both cases alike a solution of continuity; yet in the former case the action of the pin will produce pain, and not in the latter, because in this the pricked body wants the soul, and consequently the perceptive faculty: so if there were no sensitive beings those bodies that are now the objects of our senses would be but dispositively, if I may so speak, endowed with colours, tastes, and the like; and actually, but only with those more catholick affections of bodies, figure, motion, texture, &c.

To illustrate this yet a little farther: suppose a man should beat a drum at some distance from the mouth of a cave, conveniently situated to return the noise he makes; although men will presently conclude that that cave hath an echo, and will be apt to fancy upon that account some real property in the place to which the echo is said to belong; and although indeed the same noise made in many other of the neighbouring places would not be reflected to the ear, and consequently would manifest those places to have no echoes; yet, to speak physically of things, this peculiar quality or property we fancy in the cave is in it nothing else but the hollowness of its figure, whereby it is so disposed, as when the air beats against it, to reflect the motion towards the place whence that motion began; and that which passeth on this occasion is indeed but this, that the drumstick falling upon the drum makes a percussive motion of the air, and puts that fluid body into an undulating motion, and the airy waves thrusting on one another till they arrive at the hollow superficies of the cave have, by reason of its resistance and figure, their motion determined the contrary way; namely, backwards towards that part where the drum was when it was struck. So that in that which here happens there intervenes nothing but the figure of one body and the motion of another; though if a man's ear chance to be in the way of these motions of the air, forwards and backwards, it gives him a perception of them, which he calls sound. And because these perceptions which are supposed to proceed from the same percussive motion of the drum, and thereby of the air, are made at distinct times one after another, that hollow body from whence the last sound is conceived to come to the air, is imagined to have a peculiar faculty, upon whose account men are wont to say that such a place hath an echo.

5. AND whereas one body doth often seem to produce in another divers such qualities as we call sensible, which qualities therefore seem not to need any reference to our senses; I consider, that when one inanimate body works upon another, there is nothing really produced by the agent in the patient, save some local motion of its parts or some change of texture consequent upon that motion: and so, if the patient come to have any sensible quality that it had not before, it acquires it upon the same account upon which other bodies have it, and it is but a consequent to this mechanical change of texture, that, by means of its effects upon our organs of sense, we are induced to attribute this or that sensible quality to it. And in case a pin should chance by some inanimate body to be driven against a man's finger, that which the agent doth is but to put a sharp and slender body into such a kind of motion; and that which the pin doth is to pierce into a body that it meets with, not hard enough to resist its motion; and so that upon this there should ensue such a thing as pain, is but a consequent that superadds nothing of real to the pin that occasions that pain. So if a piece of transparent ice be, by the falling of some heavy and hard body upon it, broken into a gross powder that looks whitish, the falling body doth nothing to the ice but break it into very small fragments, lying confusedly upon one another; though by reason of the fabrick of the world and of our eyes there



doth in the day-time, upon this comminution, ensue such a kind of copious reflection of the incident light to our eyes as we call whiteness. And when the sun, by thawing this broken ice, destroys the whiteness of that portion of matter, and makes it become diaphanous, which it was not before, it doth no more than alter the texture of the component parts, by putting them into motion, and thereby into a new order; in which, by reason of the disposition of the pores intercepted betwixt them, they reflect but few of the incident beams of light, and transmit most of them. Thus, when with a burnisher you polish a rough piece of silver, that which is really done is but the depression of the little protuberant parts into one level with the rest of the superficies; though upon this mechanical change of the texture of the superficial parts, we men say, that it hath lost the quality of roughness, and acquired that of smoothness; because that, whereas before the little extancies by their figure resisted a little the motion of our fingers, and grated upon them a little, our fingers now meet with no such offensive resistance. It is true that the fire doth thaw ice, and also both make wax flow, and enable it to burn a man's hand; and yet this doth not necessarily argue in it any inherent quality of heat, distinct from the power it hath of putting the small parts of the wax into such a motion, as that their agitation surmounts their cohesion; which motion, together with their gravity, is enough to make them *pro tempore* constitute a fluid body; and aqua fortis, without any (sensible) heat, will make camphire cast on it assume the form of a liquor distinct from it; as I have tried that a strong fire will also make camphire fluid: not to add, that I know a liquor into which certain bodies being put, when both itself (as well as they) is actually cold (and consequently when you would not suspect it of an actual inherent heat) will not only speedily dissipate many of their parts into smoke, but leave the rest black and burnt almost like a coal. So that though we suppose the fire to do no more than variously and briskly to agitate the insensible parts of the wax, that may suffice to make us think the wax endowed with a quality of heat: because if such an agitation be greater than that of the spirit and other parts of our organs of touching, that is enough to produce in us that sensation we call heat; which is so much a relative to the sensory which apprehends it, that we see that the same lukewarm water, that is, whose corpuscles are moderately agitated by the fire, will appear hot to one of a man's hands, if that be very cold, and cold to the other, in case it be very hot, though both of them be the same man's hands. To be short, if we fancy any two of the bodies about us, as a stone, a metal, &c. to have nothing at all to do with any other body in the universe, it is not easy to conceive either how one can act upon the other, but by local motion (of the whole body or its corporeal effluvia); or how by motion it can do any more than put the parts of the other body into motion too, and thereby produce in them a change of situation and texture, or of some other of its mechanical affections: though this (passive) body being placed among other bodies in a world constituted as ours now is, and being brought to act upon the most curiously contrived sensories of animals, may upon both these accounts exhibit many differing sensible phenomena; which, however we look upon them as distinct qualities, are consequently but the effects of the often mentioned catholick affections of matter, and deducible from the size, shape, motion (or rest) posture, order, and the resulting texture of the insensible parts of bodies. And therefore though, for shortness of speech, I shall not scruple to make use of the word Qualities, since it is already so generally received, yet I would be understood to mean them in a sense suitable to the doctrine above delivered. As if I should say that roughness is apt to grate and offend the skin, I should mean, that a file or other body, by having upon its surface a multitude of little hard and exsultant parts, and of an angular or sharp figure, is qualified to work the mentioned effect: and so if I should say that heat melts metals, I should mean, that this fusion is effected by fire or some other body,



body, which, by the various and vehement motion of its insensible parts, does to us appear hot. And hence (by the way) I presume you will easily guess at what I think of the controversy so hotly disputed of late betwixt two parties of learned men, whereof the one would have all accidents to work only in virtue of the matter they reside in, and the other would have the matter to act only in virtue of its accidents: for considering that on the one side the qualities we here speak of do so depend upon matter, that they cannot so much as have a being but in and by it; and on the other side, if all matter were but quite devoid of motion (to name now no other accidents) I do not readily conceive how it would operate at all; I think it is safest to conclude that neither matter nor qualities apart, but both of them conjointly, do perform what we see done by bodies to one another, according to the doctrine of qualities just now delivered.

### *Of the Nature of a Form.*

VII. **WE** may now advance somewhat further, and consider, that men having taken notice that certain conspicuous accidents were to be found associated in some bodies, and other conventions of accidents in other bodies, they did for convenience, and for the more expeditious expression of their conceptions, agree to distinguish them into several sorts, which they call genders or species, according as they referred them, either upwards to a more comprehensive sort of bodies, or downward to a narrower species, or to individuals; as, observing many bodies to agree in being fusible, malleable, heavy, and the like, they gave to that sort of body the name of Metal, which is a genus in reference to gold, silver, lead, and but a species in reference to that sort of mixed bodies they call Fossilia: this superior genus comprehending both metals, stones, and divers other concretions, though itself be but a species in respect of mixed bodies. Now when any body is referred to any particular species (as of a metal, a stone, or the like) because men have for their convenience agreed to signify all the essentials requisite to constitute such a body by one name, most of the writers of physicks have been apt to think, that besides the common matter of all bodies, there is but one thing that discriminates it from other kinds, and makes it what it is, and this, for brevity's sake, they call a form: which, because all the qualities and other accidents of the body must depend on it, they also imagine to be a very substance, and indeed a kind of soul, which, united to the gross matter, composes with it a natural body, and acts in it by the several qualities to be found therein, which men are wont to ascribe to the creature so composed. But as to this affair I observe, that if (for instance) you ask a man what gold is; if he cannot shew you a piece of gold, and tell you this is gold, he will describe it to you as a body that is extremely ponderous, very malleable and ductile, fusible, and yet fixed in the fire, and of a yellowish colour; and if you offer to put off to him a piece of brass for a piece of gold, he will presently refuse it, and (if he understand metals) tell you, that though your brass be coloured like it, it is not so heavy nor so malleable, neither will it like gold resist the utmost brunt of the fire, or resist aqua fortis. And if you ask men, what they mean by a ruby, or nitre, or a pearl, they will still make you such answers, that you may clearly perceive, that whatever men talk in theory of substantial forms, yet that, upon whose account they really distinguish any one body from others, and refer it to this or that species of bodies, is nothing but an aggregate or convention of such accidents as most men do by a kind of agreement (for the thing is more arbitrary than we are aware of) think necessary or sufficient to make a portion of the universal matter belong to this or that determinate genus or species of natural bodies. And therefore



not only the generality of chymists, but divers philosophers, and, what is more, some schoolmen themselves, maintain it to be possible to transmute the ignobler metals into gold; which argues, that if a man could bring any parcel of matter to be yellow, and malleable and ponderous, and fixed in the fire, and upon the test, and indissoluble in aqua fortis, and in some to have a concurrence of all those accidents by which men try true gold from false, they would take it for true gold without scruple. And in this case the generality of mankind would leave the school-doctors to dispute, whether being a factitious body (as made by the chymist's art) it have the substantial form of gold, and would upon the account of the convention of the freshly mentioned accidents, let it pass current amongst them, notwithstanding most mens greater care not to be deceived in a matter of this nature than in any other. And indeed since to every determinate species of bodies there doth belong more than one quality, and for the most part a concurrence of many is so essential to that sort of bodies, that the want of any one of them is sufficient to exclude it from belonging to that species; there needs no more to discriminate sufficiently any one kind of bodies from all the bodies in the world that are not of that kind; as the chymists *luna fixa*, which they tell us wants not the weight, the malleableness, nor the fixedness, nor any other property of gold, except the yellowness (which makes them call it white gold) would by reason of that want of colour be easily known from true gold. And you will not wonder at this if you consider, that though spheres and parallelopipedons differ but in shape, yet this difference alone is the ground of so many others, that *Euclid*, and other geometricians have demonstrated I know not how many properties of the one, which do no way belong to the other; and \* *Aristotle* himself somewhere tells us that a sphere is composed of brass and roundness. And I suppose it would be thought a man's own fault if he could not distinguish a needle from a file, or a key from a pair of scissors, though these being all made of iron, and differing but in bigness and shape, are less remarkably diverse than natural bodies, the most part of which differ from each other in far more accidents than two. Nor need we think that qualities being but accidents, they cannot be essential to a natural body; for accident, as I formerly noted, is sometimes opposed to substance, and sometimes to essence. And though an accident cannot but be accidental to matter, as it is a substantial thing, yet it may be essential to this or that particular body: as in *Aristotle's* newly mentioned example, though roundness is but accidental to brass, yet it is essential to a brazen sphere; because, though the brass were devoid of roundness (as if it were cubical, or of any other figure) it would still be a corporeal substance, yet without that roundness it could not be a sphere. Wherefore since an aggregate or convention of qualities is enough to make the portion of matter it is found in what it is, and denominate it of this or that determinate sort of bodies; and since those qualities, as we have seen already, do themselves proceed from those more primary and catholic affections of matter, bulk, shape, motion, or rest, and the texture thence resulting, why may we not say that the form of a body being made up of those qualities united in one subject, doth likewise consist in such a convention of those newly named mechanical affections of matter as is necessary to constitute a body of that determinate kind. And so, though I shall for brevity's sake retain the word Form, yet I would be understood to mean by it, not a real substance distinct from matter, but only the matter itself of a natural body, considered with its peculiar manner of existence; which I think may not inconveniently be called either its specific or its denominating state, or its essential modification; or, if you would have me express it in one word, its stamp. For such a convention of accidents is sufficient to perform the offices that are necessarily required in what men call a form, since it makes the body such as it is, making it appertain to this

\* *Arist. Metaph. lib. 7. cap. 8.*



or that determinate species of bodies, and discriminating it from all other species of bodies whatsoever : As for instance, ponderousness, ductility, fixedness, yellowness, and some other qualities concurring in a portion of matter, do with it constitute gold, and making it belong to that species we call metals, and to that sort of metals we call gold, do both denominate and discriminate it from stones, salts, marcasites, and all other sorts of bodies that are not metals, and from silver, brass, copper, and all metals, except gold. And whereas it is said by some, that the form also of a body ought to be the principle of its operation, we shall hereafter consider in what sense that is to be admitted or rejected ; in the mean time it may suffice us, that even in the vulgar philosophy it is acknowledged, that natural things for the most part operate by their qualities, as snow dazzles the eyes by its whiteness, and water scattered into drops of rain falls from the clouds upon the account of its gravity. To which I shall add, that how great the power may be which a body may exercise by virtue of a single quality, may appear by the various and oftentimes prodigious effects which fire produces by its heat, when thereby its melts metals, calcines stones, destroys whole woods and cities, &c. And if several active qualities convene in one body (as that which in our hypothesis is meant by form, usually comprises several of them) what great things may be thereby performed, may be somewhat guessed at by the strange things we see done by some engines, which, being as engines, undoubtedly devoid of substantial forms, must do those strange things they are admired for, by virtue of those accidents, the shape, size, motion, and contrivance of their parts. Not to mention, that in our hypothesis, besides those operations that proceed from the essential modification of the matter, as the body (composed of matter and necessary accidents) is considered *per modum unius*, as one intire corporeal agent, it may in divers cases have other operations upon account of those particular corpuscles, which though they concur to compose it, and are, in reference to the whole, considered but as its parts, may yet retain their own particular nature, and divers of the peculiar qualities : as in a watch, besides those things which the watch performs as such, the several parts whereof it consists, as the spring, the wheels, the string, the pins, &c. may have each of them its peculiar bulk, shape, and other attributes, upon the account of one or more of which the wheel or spring, &c. may do other things than what it doth, as merely a constituent part of the watch. And so in the milk of a nurse, that hath some hours before taken a potion, though the corpuscles of the purging medicine appear not to sense distinct from the other parts of the milk, which in far greater numbers concur with them to constitute that white liquor ; yet these purgative particles, that seem to be but part of the matter whereof the milk consists, do yet so retain their own nature and qualities, that being sucked in with the rest by the infant, they quickly discriminate and discover themselves by purging him. But of this subject more hereafter.

### *Of Generation, Corruption, and Alteration.*

VIII. IT now remains, that we declare, what, according to the tenor of our hypothesis, is to be meant by generation, corruption, and alteration ; (three names that have very much puzzled and divided philosophers). In order hereunto we may consider,

1. THAT there are in the world great store of particles of matter, each of which is too small to be, whilst single, sensible ; and being intire or undivided, must needs both have its determinate shape, and be very solid. Inasmuch, that though it be mentally, and by divine Omnipotence divisible, yet by reason of its smallness and solidity nature doth scarce ever actually divide it ; and these may in this sense be called *minima* or *prima naturalia*.

2. THAT



2. THAT there are also multitudes of corpuscles which are made up of the coalition of several of the former *minima naturalia*; and whose bulk is so small and their adhesion so close and strict, that each of these little primitive concretions or clusters (if I may so call them) of particles is singly below the discernment of sense, and though not absolutely indivisible by nature into the *prima naturalia* that composed it, or perhaps into other little fragments, yet, for the reasons freshly intimated, they very rarely happen to be actually dissolved or broken, but remain intire in great variety of sensible bodies, and under various forms or disguises. As, not to repeat what we lately mentioned of the undestroyed purging corpuscles of milk, we see that even grosser and more compounded corpuscles may have such a permanent texture: for quicksilver, for instance, may be turned into a red powder for a fusible and malleable body, or a fugitive smoke, and disguised I know not how many other ways, and yet remain true and recoverable mercury. And these are, as it were, the seeds or immediate principles of many sorts of natural bodies, as earth, water, salt, &c. and those singly insensible, become capable, when united, to affect the sense: as I have tried, that if good camphire be kept a while in pure spirit of wine, it will thereby be reduced into such little parts as totally to disappear in the liquor, without making it look less clear than fair water; and yet, if into this mixture you pour a competent quantity of water, in a moment the scattered corpuscles of the camphire will, by reuniting themselves, become white, and consequently visible, as before their dispersion.

3. THAT as well each of the *minima naturalia*, as each of the primary clusters above mentioned having its own determinate bulk and shape, when these come to adhere to one another, it must always happen that the size, and often that the figure of the corpuscle composed by their juxtaposition and cohesion, will be changed; and not seldom too, the motion either of the one or the other, or both, will receive a new tendency, or be altered as to its velocity or otherwise: and the like will happen when the corpuscles that compose a cluster of particles are disjoined, or any thing of the little mass is broken off. And whether any thing of matter be added to a corpuscle or taken from it, in either case (as we just now intimated) the size of it must necessarily be altered, and for the most part the figure will be so too, whereby it will both acquire a congruity to the pores of some bodies (and perhaps some of our sensories) and become incongruous to those of others; and consequently be qualified, as I shall more fully shew you hereafter, to operate on divers occasions, much otherwise than it was fitted to do before.

4. THAT when many of these insensible corpuscles come to be associated into one visible body, if many or most of them be put into motion, from what cause soever the motion proceeds, that itself may produce great changes and new qualities in the body they compose; for not only motion may perform much even when it makes not any visible alteration in it, as air put into swift motion (as when it is blown out of bellows) acquires a new name, and is called Wind, and to the touch appears far colder than the same air not so formed into a stream; and iron, by being briskly rubbed against wood or other iron, hath its small parts so agitated as to appear hot to our sense: but this motion oftentimes makes visible alterations in the texture of the body into which it is received; for always the moved parts strive to communicate their motion, or somewhat of the degree of it, to some parts that were before either at rest or otherwise moved, and oftentimes the same moved parts do thereby either disjoin or break some of the corpuscles they hit against, and thereby change their bulk or shape, or both, and either drive some of them quite out of the body, and perhaps lodge themselves in their places, or else associate them anew with others. Whence it usually follows that the texture is for a while at least, and unless it be very stable and permanent for good and all, very



much altered, and especially in that the pores or little intervals intercepted betwixt the component particles will be changed as to bigness or figure, or both, and so will cease to be commensurate to the corpuscles that were fit for them before, and become commensurate to such corpuscles of other sizes and shapes, as till then were incongruous to them. Thus we see that water, by losing the wonted agitation of its parts, may acquire the firmness and brittleness we find in ice, and lose much of the transparency it had whilst it was a liquor. Thus also by very hard rubbing two pieces of resinous wood against one another, we may make them throw out divers of their looser parts into steams and visible smoke; and may, if the attrition be duly continued, make that commotion of the parts so change the texture of the whole, as afterwards to turn the superficial parts into a kind of coal. And thus milk, especially in hot weather, will by the intestine, though languid motions of its parts, be in a short time turned into a thinner sort of liquor than milk, and into cream, and this (last named) will, by being barely agitated in a churn, be turned in a short time into that unctuous and consistent body we call butter, and into thin, fluid, and sour butter-milk. And thus (to dispatch) by the bruising of fruit, the texture is commonly so changed, that, as we see particularly in apples, the bruised part soon comes to be of another nature than the sound part, the one differing from the other both in colour, taste, smell, and consistence. So that (as we have already inculcated) local motion hath, of all other affections of matter, the greatest interest in the altering and modifying of it; since it is not only the grand agent or efficient among second causes, but is also sometimes one of the principal things that constitutes the form of bodies. As when two sticks are set on fire by long and vehement attrition, local motion is not only that which kindles the wood, and so as an efficient produces the fire, but is that which principally concurs to give the produced stream of shining matter, the name and nature of flame: and so it concurs also to constitute all fluid bodies.

5. AND that since we have formerly seen that it is from the size, shape, and motion of the small parts of matter and the texture that results from the manner of their being disposed in any one body, that the colour, odour, taste, and other qualities of that body are to be derived, it will be easy for us to recollect that such changes cannot happen in a portion of matter without so much varying the nature of it, that we need not deride the antient atomists for attempting to deduce the generation and corruption of bodies from the famed *σύνκρισις ἢ διάκρισις*, the *convention* and *dissolution*, and the alterations of them, from the transposition of their (supposed) atoms. For though indeed nature is wont, in the changes she makes among things corporeal, to imply all the three ways, as well in alterations as generations and corruptions; yet if they only meant, as probably enough they did that of the three ways proposed, the first was wont to be the principal in the generation of bodies, the second in the corruption, and the third in their alterations; I shall not much oppose this doctrine: though I take the local motion or transposition of parts in the same portion of matter to bear a great stroke as well in reference to generation and corruption, as to alteration: as we see when milk, or flesh, or fruit, without any remarkable addition or loss of parts, turns into maggots, or other insects; and as we may more conspicuously observe in the precipitation of mercury, without addition, in the vitrification of metals, and other chymical experiments to be hereafter mentioned.

THESE things premised, it will not now be difficult to comprise in few words such a doctrine touching the generation, corruption, and alteration of bodies as is suitable to our hypothesis and the former discourse. For if in a parcel of matter there happen to be produced (it imports not much how) a concurrence of all those accidents (whether those only or more) that men by tacit agreement have thought necessary and sufficient to constitute



stitute any one determinate species of things corporeal, then we say that a body belonging to that species, as suppose a stone, or a metal, is generated or produced *de novo*: not that there is really any thing of substantial produced, but that those parts of matter that did indeed before pre-exist, but were either scattered and shared among other bodies, or at least otherwise disposed of, are now brought together and disposed of after the manner requisite to entitle the body that results from them to a new denomination, and make it appertain to such a determinate species of natural bodies, so that no new substance is in generation produced, but only that which was pre-existent obtains a new modification or manner of existence. Thus when the spring, and wheels, and string, and balance, and index, &c. necessary to a watch, which lay before scattered, some in one part, some in another of the artificer's shop, are first set together in the order requisite to make such an engine, to shew how the time passes, a watch is said to be made: not that any of the mentioned material parts is produced *de novo*, but that till then the divided matter was not so contrived and put together, as was requisite to constitute such a thing as we call a watch. And so when sand and ashes are well melted together and suffered to cool, there is generated by the colliquation that sort of concretion we call glass, though it be evident that its ingredients were both pre-existent, and do but by their association obtain a new manner of existing together. And so when, by the churning of cream, butter and butter-milk are generated, we find not any thing substantial produced *de novo* in either of them, but only that the serum and the fat corpuscles being put into local motion do, by their frequent occurrences, extricate themselves from each other, and associate themselves in the new manner requisite to constitute the bodies, whose names are given them.

AND as a body is said to be generated when it first appears clothed with all those qualities, upon whose account men have been pleased to call some bodies stones; others, metals; others, salts, &c. so when a body comes to lose all or any of those accidents that are essential and necessary to the constituting of such a body, it is then said to be corrupted or destroyed, and is no more a body of that kind, but loses its title to its former denomination. Not that any thing corporeal or substantial perishes in this change, but only that the essential modification of the matter is destroyed: and though the body be still a body (no natural agent being able to annihilate matter) yet it is no longer such a body as it was before, but perisheth in the capacity of a body of that kind. Thus, if a stone, falling upon a watch, break it to pieces, as, when the watch was made, there was no new substance produced, all the material parts (as the steel, brass, string, &c.) being pre-existent somewhere or other (as in iron and copper-mines, in the bellies of those animals of whose guts men use to make strings); so not the least part of the substance of the watch is lost, but only displaced and scattered; and yet that portion of matter ceases to be a watch as it was before. And so (to resume our late example) when cream is by churning turned into butter, and a serous liquor, the parts of the milk remain associated into those new bodies, but the white liquor perisheth in the capacity of milk. And so when ice comes to be thawed in exactly-closed vessels, though the corruption be produced only (for aught appears) by introducing a new motion and disposition into the parts of the frozen water, yet it thereupon ceases to be ice, however it be as much water, and consequently as much a body as before it was frozen or thawed. These and the like examples may teach us rightly to understand that common axiom of naturalists, *Corruptio unius est generatio alterius*; & à contrà: for since it is acknowledged on all hands that matter cannot be annihilated, and since it appears by what we have said above, that there are some properties, namely, size, shape, motion (or, in its absence, rest) that are inseparable from the actual parts of matter; and since also the coalition of any competent number of these parts is sufficient to constitute a natural body en-

dowed



dowed with divers sensible qualities, it can scarce be otherwise, but that the same agents that shatter the frame, or destroy the texture of one body, will, by shuffling them together, and disposing them after a new manner, bring them to constitute some new sort of bodies: as the same thing that by burning destroys wood, turns it into flame, soot, and ashes. Only I doubt whether the axiom do generally hold true, if it be meant, that every corruption must end in the generation of a body belonging to some particular species of things, unless we take powders and fluid bodies indefinitely for species of natural bodies; since it is plain there are multitudes of vegetables and other concretions, which, when they rot, do not, as some others do, turn into worms, but either into some slimy or watery substance, or else (which is the most usual) they crumble into a kind of dust or powder, which, though looked upon as being the earth into which rotten bodies are at length resolved, is very far from being of an elementary nature, but as yet a compounded body, retaining some if not many qualities, which often makes the dust of one sort of plant or animal differ much from that of another. And this will supply me with this argument *ad hominem*, viz. That since in those violent corruptions of bodies that are made by outward agents, shattering them into pieces, if the axiom hold true, the new bodies emergent upon the dissolution of the former must be really natural bodies (as indeed divers of the moderns hold them to be) and generated according to the course of nature; as when wood is destroyed by fire, and turned partly into flame, partly into soot, partly into coals, and partly into ashes, I hope we may be allowed to conclude, that those chymical productions, which so many would have to be but factitious bodies, are natural ones, and regularly generated. For it being the same agent, the fire that operates upon bodies, whether they be exposed to it in close glasses or in chimnies, I see no sufficient reason why the chymical oils and volatile salts, and other things, which Spagyrites obtain from mixed bodies, should not be accounted natural bodies, as well as the soot and ashes, and charcoal, that by the same fire are obtained from kindled wood.

BUT before we pass away from the mention of the corruption of bodies, I must take some notice of what is called their putrefaction. This is but a peculiar kind of corruption, wrought slowly (whereby it may be distinguished from destruction by fire and other nimble agents) in bodies: it happens to them for the most part by means of the air or some other ambient fluid, which, by penetrating into the pores of the body, and by its agitation in them, doth usually call out some of the more agile and less intangled parts of the body, and doth almost ever loosen and dislocate the parts in general, and thereby so change the texture, and perhaps too the figure, of the corpuscles that compose it, that the body thus changed acquires qualities unsuitable to its former nature, and for the most part offensive to our senses, especially of smelling and tasting: which last clause I therefore add, not only because the vulgar look not upon the change of an egg into a chick as a corruption, but as a perfection of the egg; but because also I think it not improbable, that if by such slow changes of bodies as make them lose their former nature, and might otherwise pass for putrefaction, many bodies should acquire better scents or tastes than before; or if nature, custom, or any other cause, should much alter the texture of our organs of tasting and smelling, it would not perhaps be so well agreed on what should be called putrefaction, as that imports an impairing alteration, but men would find some favourable notion for such changes. For I observe that medlars, though they acquire in length of time such a colour and softness as rotten apples and other putrefied fruits do, yet, because their taste is not then harsh as before, we call that ripeness in them, which otherwise we should call rottenness. And though, upon the death of a four-footed beast, we generally call that change which happens to the flesh or



blood putrefaction, yet we pass a more favourable judgment upon that which happens to the flesh and other softer parts of that animal (whether it be a kind of large rabbits or very small and hornless deer) of which in China and in the Levant they make muck; because, by the change that ensues the animal's death, the flesh acquires not an odious but a grateful smell. And we see that some men, whose appetites are gratified by rotten cheese, think it then not to have degenerated, but to have attained its best state, when having lost its former colour, smell, and taste, and, which is more, being in great part turned into those insects called mites, it is both in a philosophical sense corrupted, and in the estimate of the generality of men grown putrid. But because it very seldom happens that a body by generation acquires no other qualities than just those that are absolutely necessary to make it belong to the species that denominates it; therefore in most bodies there are divers other qualities that may be there, or may be missing, without essentially changing the subject: as water may be clear or muddy, odorous or stinking, and still remain water; and butter may be white or yellow, sweet or rancid, consistent or melted, and still be called butter. Now therefore, whenever a parcel of matter does acquire or lose a quality that is not essential to it, that acquisition or loss is distinctly called alteration (or by some, mutation): the acquiescence only of the qualities that are absolutely necessary to constitute its essential and specific difference, or the loss of any of those qualities, being such a change as must not be called mere alteration, but have the particular name of generation or corruption; both which, according to this doctrine, appear to be but several kinds of alteration taken in a large sense, though they are distinguished from it in a more strict and limited acceptance of that term.

AND here we have a fair occasion to take notice of the fruitfulness and extent of our mechanical hypothesis: for since, according to our doctrine, the world we live in is not a moveless or indigested mass of matter, but an *Autóματος*, or *self-moving engine*, wherein the greatest part of the common matter of all bodies is always (though not still the same parts of it) in motion, and wherein bodies are so close set by one another, that (unless in some very few and extraordinary and as it were preternatural cases) they have either no vacuities betwixt them, or only here and there interposed and very small ones: and since, according to us, the various manner of the coalition of several corpuscles into one visible body is enough to give them a peculiar texture, and thereby fit them to exhibit divers sensible qualities, and to become a body, sometimes of one denomination, and sometimes of another; it will very naturally follow, that from the various occurrences of those innumerable swarms of little bodies that are moved to and fro in the world, there will be many fitted to stick to one another, and so compose concretions; and many (though not in the self-same place) disjoined from one another and agitated apart; and multitudes also that will be driven to associate themselves, now with one body, and presently with another. And if we also consider on the one side, that the sizes of the small particles of matter may be very various, their figures almost innumerable; and that if a parcel of matter do but happen to stick to one body, it may chance to give it a new quality, and if it adhere to another, or hit against some of its parts, it may constitute a body of another kind; or if a parcel of matter be knocked off from another, it may barely by that leave it, and become itself of another nature than before: if, I say, we consider these things on the one side, and on the other side, that (to use Lucretius's comparison) all that innumerable multitude of words that are contained in all the languages of the world are made of the various combinations of some of the twenty-four letters of the alphabet, it will not be hard to conceive that there may be an incomprehensible variety of associations and textures of the minute parts of bodies, and consequently a vast multitude of portions of matter endowed with store enough of differing qualities



qualities to deserve distinct appellations ; though for want of heedfulness and fit words men have not yet taken so much notice of their less obvious varieties as to sort them as they deserve, and give them distinct and proper names. So that though I would not say that any thing can immediately be made of every thing, as a gold-ring of a wedge of gold, or oil or fire of water ; yet since bodies having but one common matter can be differenced but by accidents, which seem all of them to be the effects and consequents of local motion, I see not why it should be absurd to think that (at least among inanimate bodies) by the intervention of some very small addition or subtraction of matter (which yet in most cases will scarce be needed) and of an orderly series of alterations, disposing by degrees the matter to be transmuted, almost of any thing, may at length be made any thing : as, though out of a wedge of gold one cannot immediately make a ring, yet by either wire-drawing that wedge by degrees, or by melting it, and casting a little of it into a mould, that thing may easily be effected. And so though water cannot immediately be transmuted into oil, and much less into fire ; yet if you nourish certain plants with water alone (as I have done) till they have assimilated a great quantity of water into their own nature, you may, by committing this transmuted water (which you may distinguish and separate from that part of the vegetable you first put in) to distillation in convenient glasses, obtain, besides other things, a true oil, and a black combustible coal (and consequently fire) ; both of which may be so copious as to leave no just cause to suspect that they could be any thing near afforded by any little spirituous parts, which may be presumed to have been communicated by that part of the vegetable that is first put into the water, to that far greater part of it which was committed to distillation.

BUT, Pyrophilus, I perceive the difficulty and fruitfulness of my subject have made me so much more prolix than I intended, that it will not now be amiss to contract the summary of our hypothesis, and give you the main points of it with little or no illustration, and without particular proofs, in a few words. We teach then (but without peremptorily asserting it)

1. THAT the matter of all natural bodies is the same ; namely, a substance, extended and impenetrable.

2. THAT all bodies thus agreeing in the same common matter, their distinction is to be taken from those accidents that do diversify it.

3. THAT motion, not belonging to the essence of matter (which retains its whole nature when it is at rest) and not being originally producible by other accidents, as they are from it, may be looked upon as the first and chief mood or affection of matter.

4. THAT motion, variously determined, doth naturally divide the matter it belongs to into actual fragments of parts ; and this division, obvious experience (and more eminently chymical operations) manifest to have been made into parts exceedingly minute, and very often too minute to be singly perceivable by our senses.

5. WHENCE it must necessarily follow, that each of these minute parts or *minima naturalia* (as well as every particular body made up by the coalition of any number of them) must have its determinate bigness or size, and its own shape. And these three, namely, bulk, figure, and either motion or rest (there being no mean between these two) are the three primary and most catholic moods or affections of the insensible parts of matter, considered each of them apart.

6. THAT when divers of them are considered together, there will necessarily follow here below both a certain position of posture in reference to the horizon (as erected, inclining, or level) of each of them, and a certain order or placing before or behind, or besides one another ; as when in a company of soldiers, one stands upright, the



other stoops, the other lies along upon the ground, they have various postures; and their being placed besides one another in ranks, and behind one another in files, are varieties of their order: and when many of these small parts are brought to convene into one body from their primary affections, and their disposition or contrivance as to posture and order, there results that which by one comprehensive name we call the texture of that body. And indeed these several kinds of location (to borrow a scholastical term) attributed in this sixth number to the minute particles of bodies, are so near of kin that they seem all of them referable to (that one event of their convening) situation or position. And these are the affections that belong to a body, as it is considered in itself, without relation to sensitive beings or to other animal bodies.

7. THAT yet there being men in the world whose organs of sense are contrived in such differing ways, that one sensory is fitted to receive impressions from some, and another from other sorts of external objects or bodies without them (whether these act as intire bodies, or by emission of their corpuscles, or by propagating some motion to the sensory) the perceptions of these impressions are by men called by several names, as heat, colour, sound, odour; and are commonly imagined to proceed from certain distinct and peculiar qualities in the external objects which have some resemblance to the ideas their action upon the senses excites in the mind; though indeed all these sensible qualities, and the rest that are to be met with in the bodies without us, are but the effects or consequences of the above-mentioned primary affections of matter, whose operations are diversified according to the nature of the sensories or other bodies they work upon.

8. THAT when a portion of matter, either by the accession or recess of corpuscles, or by the transposition of those it consisted of before, or by any two, or all of these ways, happens to obtain a concurrence of all those qualities which men commonly agree to be necessary and sufficient to denominate the body which hath them, either a metal or a stone, or the like, and to rank it in any peculiar and determinate species of bodies, then a body of that denomination is said to be generated.

9. THIS convention of essential accidents being taken (not any of them apart, but all) together for the specifical difference that constitutes the body and discriminates it from all other sorts of bodies, is by one name, because considered as one collective thing, called its form (as beauty, which is made up of symmetry of parts and agreeableness of colours) which is consequently but a certain character (as I sometimes call it) or a peculiar state of matter, or, if I may so name it, an essential modification: a modification, because it is indeed but a determinate manner of existence of the matter, and yet an essential modification, because that though the concurrent qualities be but accidental to matter (which with others instead of them, would be matter still) yet they are essentially necessary to the particular body, which without those accidents would not be a body of that denomination, as a metal or a stone, but of some other.

10. Now a body being capable of many other qualities besides those whose convention is necessary to make up its form, the acquisition or loss of any such quality is by naturalists, in the more strict sense of that term, named alteration: as when oil comes to be frozen, or to change colour, or to grow rancid; but if all or any of the qualities that are reputed essential to such a body come to be lost or destroyed, that notable change is called corruption. As when oil being boiled takes fire, the oil is not said to be altered in the former sense, but corrupted or destroyed, and the emergent fire generated; and when it so happens that the body is slowly corrupted, and thereby also acquires qualities offensive to our senses, especially of smell and taste (as when flesh or fruit grows rotten) that kind of corruption is by a more particular name called putrefaction. But neither in this nor in any other kind of corruption is there any thing substantial destroyed (no such thing having been produced in generation, and  
matter



matter itself being on all hands acknowledged incorruptible) but only that special connexion of the parts or manner of their co-existence, upon whose account the matter (whilst it was in its former state) was, and was called a stone, or a metal, or did belong to any other determinate species of bodies.

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S U B S T A N T I A L F O R M S,

As it is wont to be taught by the PERIPATETICKS.

THE origin of forms, Pyrophilus, as it is thought the noblest, so, if I mistake not, it hath been found one of the most \* perplexed enquiries that belong to natural philosophy: and, I confess, it is one of the things that has invited me to look about for some more satisfactory account than the schools usually give of this matter, that I have observed that the wisest that have busied themselves in explicating forms according to the Peripatetick notions of them, have either knowingly confessed themselves unable to explain them, or unwittingly proved themselves to be so, by giving but unsatisfactory explications of them.

It will not, I presume, be expected that I, who now write but notes, should enumerate, much less examine, all the various opinions touching the origin and nature of forms; it being enough for our purpose, if, having already intimated in our hypothesis what, according to that, may be thought of this subject, we now briefly consider the general opinion of our modern Aristotelians and the schools concerning it; I say, the modern Aristotelians, because divers of the ancient, especially Greek commentators of Aristotle, seem to have understood their master's doctrine of forms much otherwise, and less incongruously, than his Latin followers, the schoolmen and others, have since done. Nor

\* *Formarum cognitio est rudis, confusa, nec nisi per  $\pi\epsilon\rho\iota\phi\alpha\sigma\iota\varsigma$ ; neque verum est, formæ substantialis speciem recipi in intellectum, non enim in sensu usquam fuit.* J. C. Scalig.

*Formæ substantiales sunt incognitæ nobis, quia insensibiles: ideo per qualitates, quæ sunt principia immediatæ transmutationis, exprimuntur.* Aquinas ad 1. de degenerat. & corrupt.

*In hac humanæ mentis caligine aquæ forma ignis ac magnetis nobis ignota est.* Sennertus.

do I expressly mention Aristotle himself among the champions of substantial forms, because though he seems in a place or two expressly enough to reckon forms among substances, yet elsewhere the examples he employs to set forth the forms of natural things by, being taken from the figures of artificial things (as of a statue, &c.) which are confessedly but accidents, and making very little use, if any, of substantial forms to explain the phenomena of nature, he seems to me upon the whole matter either to have been irresolved whether there were any such substances or no, or to speak ambiguously and obscurely enough of them, to make it questionable what his opinions of them were.

BUT the sum of the controversy betwixt us and the schools is this, whether or no the forms of natural things (the souls of men always excepted) be in generation educed, as they speak, out of the power of the matter, and whether these forms be true substantial entities, distinct from the other substantial principle of natural bodies, namely matter.

THE reasons that move me to embrace the negative are principally these three: first, that I see no necessity of admitting in natural things any such substantial forms; matter, and the accidents of matter, being sufficient to explicate as much of the phenomena of nature as we either do or are like to understand. The next, that I see not what use this puzzling doctrine of substantial forms is of in natural philosophy; the acute Scalliger, and those that have most busied themselves in the indagation of them, having freely acknowledged (as the more candid of the Peripateticks generally do) that the true knowledge of forms is too difficult and abstruse to be attained by them. And how like it is, that particular phenomena will be explained by a principle whose nature is confessedly ignored, I leave you to judge. But because to these considerations I often have had, and shall have here and there, occasion to say something in the body of these notes, I shall at present insist upon the third; which is, that I cannot conceive neither how the forms can be generated, as the Peripateticks would have it, nor how the things they ascribe to them are consistent with the principles of true philosophy, or even with what themselves otherwise teach.

THE manner how forms are educed out of the power of the matter, according to that part of the doctrine of forms wherein the schools generally enough agree, is a thing so inexplicable, that I wonder not it hath put acute men upon several hypotheses to make it out. And indeed the number of these is of late grown too great to be fit to be here recited, especially since I find them all so very unsatisfactory, that I cannot but think the acute sticklers for any of them are rather driven to embrace it by the palpable inconveniences of the ways they reject, than by any thing they find to satisfy them in that which they make choice of: and for my part I confess I find so much reason in what each party says against the explications of the rest, that I think they all confute well, and none does well establish.

BUT my present way of writing forbidding me to insist on many arguments against the doctrine wherein they most agree, I shall only urge that which I confess chiefly sticks with me, namely, that I find it not comprehensible.

I KNOW the modern schoolmen fly here to their wonted refuge of an obscure distinction, and tell us, that the power of matter in reference to forms is partly educive, as the agent can make the form out of it; and partly receptive, whereby it can receive the form so made. But since those that say this will not allow that the form of a generated body was not actually pre-existent in its matter, or indeed any-where else; it is hard to conceive how a substance can be educed out of another substance totally distinct in nature from it, without being before such education actually existent in it. And as for the receptive power of the matter, that but fitting it to receive or lodge a form when brought to be united with it, how can it be intelligibly made out to contribute



tribute to the production of a new substance of a quite differing nature from that matter, though it harbours it when produced? And it is plain that the human body hath a receptive power in reference to the human soul, which yet themselves confess both to be a substantial form, and not to be educed out of the power of matter. Indeed if they would admit the form of a natural body to be but a more fine and subtle part of the matter, as spirit of wine is of wine, which upon its recefs remains no longer wine, but phlegm or vinegar, then the educative power of matter might signify something: and so it might, if with us they would allow the form to be but a modification of the matter; for then it would import, that the matter may be so ordered or disposed by fit agents as to constitute a body of such a sort and denomination: and so (to resume that example) the form of a sphere may be said to lurk potentially in a piece of brass, inasmuch as that brass may by casting, turning, or otherwise, be so figured as to become a sphere. But this they will not admit, lest they should make forms to be but accidents, though it is, for aught I know, as little intelligible how what is educed out of any matter, without being either pre-existent, or being any part of the matter, can be a true substance, as how that roundness that makes a piece of brass become a sphere, can be a new substance in it. Nor can they admit the other way of educing a form out of matter as spirit is out of wine, because then not only matter will be corruptible against their grounds, but matter and form would not be two differing and substantial principles, but one and the same, though diversified by firmness, grossness, &c. which are but accidental differences. I know they speak much of the efficacy of the agent upon the matter in the generation of natural bodies, and tell us strange things of his manner of working. But not to spend time in examining those obscure niceties, I answer in short, that since the agent, be he what he will, is but a physical and finite agent, and since what way soever he works he can do nothing repugnant to the nature of things, the difficulty that sticks with me will still remain. For if the form produced in generation be, as they would have it, a substance that was not before to be found any where out of that portion of matter wherewith it constitutes the generated body; I say, that either it must be produced by refining or subtiliating some parts of the matter into form, or else it must be produced out of nothing, that is, created (for I see no third way how a substance can be produced *de novo*). If they allow the first, then will the form be indeed a substance, but not, as they hold it is, distinct from matter; since matter, however subtiliated, is matter still, as the finest spirit of wine is as truly a body as was the wine itself that yielded it, or as is the grosser phlegm from which it was extracted: besides that, the Peripateticks teach that the form is not made of any thing of the matter; nor indeed is it conceivable how a physical agent can turn a material into an immaterial substance, especially matter being, as they themselves confess, as well incorruptible as ingenerable. But if they will not allow, as indeed they do not, that the substantial form is made of any thing that is material, they must give me leave to believe that it is produced out of nothing, till they shew me how a substance can be produced otherwise that existed no-where before. And at this rate every natural body of a special denomination, as gold, marble, nitre, &c. must not be produced barely by generation, but partly by generation and partly by creation. And since it is confessed on all sides that no natural agent can produce the least atom of matter, it is strange they should in generation allow every physical agent the power of producing a form, which, according to them, is not only a substance but a far nobler one than matter; and thereby attribute to the meanest creatures that power of creating substances which the ancient naturalists thought too great to be ascribed to God himself, and which indeed is too great to be ascribed to any other than him. And therefore some schoolmen and philosophers have derived forms immediately from God; but  
this



is not only to desert Aristotle and the Peripatetick philosophy they would seem to maintain, but to put Omnipotence upon working I know not how many thousand miracles every hour to perform that (I mean the generation of bodies of new denominations) in a supernatural way, which seems the most familiar effect of nature in her ordinary course.

AND as the production of forms out of the power of matter is for these reasons incomprehensible to me; so those things which the Peripateticks ascribe to their substantial forms are some of them such as I confess I cannot reconcile my reason to: for they tell us positively that these forms are substances, and yet at the same time they teach that they depend upon matter both *in fieri* and *in esse*, as they speak; so that out of the matter that supports them they cannot so much as exist (whence they are usually called material forms) which is to make them substances in name, and but accidents in truth. For not to ask how (among physical things) one substance can be said to depend upon another *in fieri* that is not made of any part of it, the very notion of a substance is to be a self-subsisting entity, or that which needs no other created being to support it or to make it exist. Besides that, there being but two sorts of substances, material and immaterial, a substantial form must appertain to one of the two, and yet they ascribe things to it that make it very unfit to be referred to either. To all this I add, that these imaginary material forms do almost as much trouble the doctrine of corruption as that of generation: for if a form be a true substance really distinct from matter, it must, as I lately noted, be able to exist of itself without any other substance to support it; as those I reason with confess that the soul of man survives the body it did before death inform: whereas they will have it that in corruption the form is quite abolished and utterly perishes, as not being capable of existing, separated from the matter whereunto it was united. So that here again what they call a substance they make indeed an accident, and besides contradict their own vulgar doctrine, that natural things are upon their corruption resolved into the first matter; since, at this rate they should say, that such things are but partly resolved into the first matter, and partly either into nothing, or into forms, which, being as well immaterial as the souls of men, must, for aught appears, be also, like them, accounted immortal.

I SHOULD now examine those arguments that are wont to be employed by the schools to evince their substantial forms; but, besides that the nature and scope of my present work enjoins me brevity, I confess that, one or two excepted, the arguments I have found mentioned as the chief, are rather metaphysical or logical, than grounded upon the principles and phenomena of nature, and respect rather words than things; and therefore I, who have neither inclination nor leisure to wrangle about terms, shall content myself to propose, and very briefly answer, two or three of those that are thought the plausiblest.

FIRST then they thus argue, *omne compositum substantiale* (for it is hard to English well such uncouth terms) *requirit materiam & formam substantialem, ex quibus componatur. Omne corpus naturale est compositum substantiale: ergo, &c.* In this syllogism some do plausibly enough deny the consequence, but for brevity's sake I shall rather chuse to deny the minor, and desire the proposers to prove it. For I know not any thing in nature that is composed of matter, and a substance distinct from matter, except man, who alone is made up of an immaterial form and a human body: and if it be properly said to be *composita substantialia*, I shall, rather than wrangle with them, give them leave to find out some other name for other natural things.

BUT then they argue, in the next place, that if there were no substantial forms, all bodies would be but *entia per accidens*, as they speak; which is absurd. To which I answer, that in the notion that divers learned men have of an *ens per accidens*, namely, that



that it is that which consists of those things *quæ non ordinantur ad unum*, it may be said, that though we do not admit substantial forms, yet we need not admit natural bodies to be *entia per accidens*; because in them the several things that concur to constitute the body, as matter, shape, situation, and motion, *ordinantur per se & intrinsicè* to constitute one natural body. But if this answer satisfy not, I shall add, that for my part that which I am solicitous about is, that what nature hath made things to be in themselves, not what, logician or metaphysician will call them in the terms of his art; it being much fitter in my judgment to alter words, that they may better fit the nature of things, than to affix a wrong nature to things that they may be accommodated to forms or words that were probably devised, when the things themselves were not known or well understood, if at all thought on.

WHEREFORE I shall but add one argument more of this sort; and that is, that if there were no substantial forms, neither could there be any substantial definitions; but the consequent is absurd, and therefore so is the antecedent. To which I reply, that since the Peripateticks themselves confess the forms of bodies to be of themselves unknown; all that this argument seems to me to conclude is but this, that if we do not admit some things that are not *in rerum naturâ*, we cannot build our definitions upon them: nor indeed could we, if we should admit substantial forms, give substantial definitions of natural things, unless we could also define natural bodies by things that we know not; for such \* the substantial forms are (as we have seen already) confessed to be by the wisest Peripateticks, who pretend not to give the substantial definition of any natural *compositum*, except man. But it may suffice us to have, instead of substantial, essential definitions of things; I mean such as are taken from the essential differences of things, which constitute them in such a sort of natural bodies, and discriminate them from all those of any other sort.

THESE three arguments, Pyrophilus, for substantial forms, you may possibly, as well as I, find variously proposed, and perhaps with some light alterations multiplied in the writings of the Peripateticks and schoolmen; but all the arguments of this kind that I have met with, may, if I mistake not, be sufficiently solved by the answers we have given to these, or at least by the grounds upon which those answers are built; those seemingly various arguments agreeing in this, that either they respect rather words than things, or that they are grounded upon precarious suppositions; or lastly that they urge that as an absurdity, which, whether it be one or not in those that admit the Peripatetic philosophy, to me, that do as little acquiesce in many of their other principles as I do in their substantial forms, doth not appear any absurdity at all. And it is perhaps for fear that arguments of this sort should not much prevail with naturalists, that some of the modern assertors of the forms we question have thought it requisite to add some more physical arguments, which (though I have not found them all in the same writers, yet) being in all but few, I shall here briefly consider them.

FIRST then, among the physical arguments that are brought to prove substantial forms, I find that the most confidently insisted on, which is taken from the spontaneous return of heated water to coldness; which effects, say they, must necessarily be ascribed to the action of the substantial form whose office it is to preserve the body in its natural state, and when there is occasion to reduce it thereunto: and the argument indeed might be plausible, if we were sure that heated water would grow cold again (without the avolation of any parts more agitated than the rest) supposing it to be removed into some of the imaginary spaces beyond the world; but as the case is, I see no necessity of flying to a substantial form, the matter seeming to be easily explicable otherwise. The

\* *Neque tibi ullam esse formam nobis notam plenè & planè, nostramque scientiam esse ambrym in sole.* Scalig.  
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water we heat is surrounded with our air, or with some vessel, or other body contiguous to the air, and both the air and the water in these climates are most commonly less agitated than the juices in our hands, or other organs of touching, which makes us esteem and call those fluids cold. Now when the water is exposed to the fire, it is thereby put into a new agitation more vehement than that of the parts of our sensory, which you will easily grant, if you consider that when the heat is intense it makes the water boil and smoke, and oftentimes run over the vessel; but when the liquor is removed from the fire, this acquired agitation must needs by degrees be lost, either by the avolation of such fiery corpuscles as the Epicureans imagine to be got into heated water, or by the water's communicating the agitation of its parts to the contiguous air, or to the vessel that contains it, till it have lost its superfluous of motion, or by the ingress of those frigorifick atoms, wherewith (if any such be to be granted) the air in these climates is wont to abound, and so be reduced into its former temperature: which may as well be done without a substantial form, as if a ship swimming slowly down a river should by a sudden gust of wind, blowing the same way the stream runs, be driven on much faster than before, the vessel upon the ceasing of the wind may, without any such internal principle, return after a while to its former slowness of motion. So that in this phenomenon we need not have recourse to an internal principle, the temperature of the external air being sufficient to give an account of it. And if water be kept (as is usual in poor mens houses that want cellars) in the upper rooms of the house, in case the climate be hot, the water will, in spite of the form, continue far less cold than, according to the Peripateticks, its nature requires, all the summer long. And let me here represent to the champions of forms, that, according to their doctrine, the fluidity of the water must at least as much proceed from its form as the coldness; and yet this does so much depend upon the temperature of the air, that in Nova Zembla vast quantities of water are kept in the hard and solid form of ice all the year long by the sharp cold of the ambient air, notwithstanding all the pretended office and power of the substantial form to keep it fluid; which it will never be reduced to be, unless by such a thawing temperature of the air as would itself, for aught appears, make it flow again, although there were no substantial form *in rerum natura*.

THERE is another argument much urged of late by some learned men, the substance whereof is this; that matter being indifferent to one sort of accidents as well as to another, it is necessary there should be a substantial form to keep those accidents, which are said to constitute it, united to the matter they belong to, and preserve both them and the body in their natural state: for since it is confessed that matter hath no appetite to these accidents more than to any others, they demand how without a substantial form these accidents can be contained and preserved? To this I might represent that I am not so well satisfied with the notion wont to be taken for granted, not only by the vulgar, but by philosophers, of the natural state of bodies; as if it were undeniable that every natural body (for as to some I shall not now question it) has a certain state wherein nature endeavours to preserve it, and out of which it cannot be put, but by being put into a preternatural state. For the world being once constituted by the great Author of things as it now is, I look upon the phenomena of nature to be caused by the local motion of one part of matter hitting against another, and am not so fully convinced that there is such a thing as nature's designing to keep such a parcel of matter in such a state that is clothed with just such accidents, rather than with any other. But I look upon many bodies, especially fluid ones, as frequently changing their state, according as they happen to be more or less agitated or otherwise wrought upon by the sun, and other considerable agents in nature. As the air, water, and other fluids, if the temperature as to cold or heat, and rarefaction or condensation,

which



which they are in at the beginning of the spring here at London, be pitched upon as their natural state; then not only in the torrid and frozen zones they must have other and very differing natural states, but here itself they will almost all the summer and all the winter (as our weather-glasses inform us) be in a varying preternatural state, because they will be in those seasons either more hot and rarified, or more cold and condensed, than in the beginning of the spring. And in more stable and constant bodies, I take in many cases the natural state to be but either the most usual state, or that wherein that which produces a notable change in them finds them. As when a slender piece of silver that is most commonly flexible, and will stand bent every way, comes to be well hammered, I count that flexibility to be the natural state of that metal, because most commonly silver is found to be flexible, and because it was so before it was hammered; but the springiness it acquires by hammering is a state which properly is no more unnatural to the silver than the other, and would continue with the metal as long as it, if both pieces of silver, the one flexible the other springy, were let alone and kept from outward violence. And as the silver, to be deprived of its flexibility, needed the violent motion of the hammer, so to deprive it of its spring it needs the violent agitation of a heating fire. These things and much more I might here represent; but to come close to the objection, I answer, that the accidents spoken of are introduced into the matter by the agents or efficient causes, whatever they be that produce in it what (in the sense formerly explained) we call an essential (though not a substantial) form. And these accidents being once thus introduced into the matter, we need not seek for a new substantial principle to preserve them there, since by the general law or common course of nature the matter qualified by them must continue in the state such accidents have put it into, till by some agent or other it be forcibly put out of it, and so divested of those accidents: as in the formerly mentioned example borrowed from Aristotle, of a brazen sphere, when once the motion of tools impelled and guided by the artificer have turned a piece of brass into a sphere, there needs no new substance to preserve that round figure, since the brass must retain it till it be destroyed by the artificer himself, or some other agent able to overcome the resistance of the matter, to be put into another figure. And on this occasion let me confirm this *ad hominem*, by representing that there is not an inconsiderable party among the Peripateticks themselves who maintain, that in the elements the first qualities (as they call them) are instead of forms, and that the fire (for instance) hath no other form than heat and dryness, and the water than coldness and moisture. Now if these bodies, that are the vastest and the most important of the sublunary world, consist but of the universal matter and the few accidents; and if in these there needs no substantial form to keep the qualities of the matter united to it, and conjoined among themselves, and preserve them in that state as long as the law of nature requires; though besides the four qualities that are called first, the elements have divers others, as gravity and levity, firmness and fluidity, opacousness and transparency, &c. why should the favourers of this opinion deny that in other bodies, besides the elements, qualities may be preserved and kept united to the matter they belong to without the band or support of a substantial form? And as, when there is no competent destructive cause, the accidents of a body will by the law of nature remain such as they were; so if there be, it cannot with reason be pretended that the substantial form is able to preserve all those accidents of a body that are said to flow from it, and to be as it were under its care and tuition. For if, for instance, you expose a sphere or bullet of lead to a strong fire, it will quickly lose (not to mention its figure) both its coldness, its consistence, its malleableness, its colour (for it will appear of the colour of fire) its flexibility, and some other qualities; and all this in spite of the imaginary substantial form, which, according



to the Peripatetical principles, in this case must still remain in it without being able to help it. And though, upon the taking the lead from off the fire, it is wont to be educed to most of its former qualities (for it will not of itself recover its sphericity) yet that may well be ascribed partly to its peculiar texture, and partly to the coldness of the ambient air, according to what we lately discoursed touching heated and refrigerated water; which temperature of the air is an extrinsecal thing to the lead, and indeed it is but accidental that the lead upon refrigeration regains its former qualities: for in case the lead have been exposed long enough to a sufficiently intense fire, it will (as we have purposely tried) be turned into glass, and lose its colour, its opacity, its malleableness, and (former degree of) flexibleness, and acquire a reddishness, a degree of transparency, a brittleness, and some other qualities that it had not before: and let the supposed substantial form do what it can, even when the vessel is removed from the fire, to reduce or restore the body to its natural state and accidents, yet the former qualities will remain lost as long as these preternatural ones introduced by the fire continue in the matter; and neither the one will be restored, nor the other destroyed, till some sufficiently powerful extrinsecal agent effect the change. And on the other side I consider, that the fruit, when severed from the tree it grew on, is confessed to be no longer animated (at least the kernels or seeds excepted) by the vegetative soul or substantial form of the plant; yet in an orange or lemon (for instance) plucked from the tree, we see that the same colour, the same odour, the same taste, the same figure, the same consistence, and, for aught we know, the same other qualities, whether sensible or even occult, as are its antidotal and antiscorbutical virtues that must before be said to have flowed from the soul of the tree, will continue many months, perhaps some years, after the fruit has ceased to have any commerce with the tree (nay, though the tree whereon it grew be perhaps in the mean time hewn down or burnt, and though consequently its vegetative soul or form be destroyed) as when it grew thereon, and made up one plant with it. And we find that tamarinds, rhubarb, fenna, and many other simples, will, for divers years after they have been deprived of their former vegetative soul, retain their purgative and other specific properties.

I FIND it likewise urged that there can be no reason why whiteness should be separable from a wall and not from snow or milk, unless we have recourse to substantial forms. But in case men have agreed to call a thing by such a name, because it has such a particular quality that differences it from others, we need go no farther to find a reason why one quality is essential to one thing and not to another. As in our former example of a brass sphere, the figure is that for which we gave it that name; and therefore, though you may alter the figure of the matter, yet by that very alteration the body perishes in the capacity of a sphere, whereas its coldness may be exchanged for heat, without the making it the less a sphere, because it is not for any such quality, but for roundness, that a body is said to be a sphere. And so firmness is an inseparable quality of ice, though this or that particular figure be not, because that it is for want of fluidity that any thing that was immediately before a liquor is called ice; and congruously hereunto, though whiteness were inseparable from snow and milk, yet that would not necessarily infer that there must be a substantial form to make it so: for the firmness of the corpuscles that compose snow is as inseparable from it as the whiteness; and yet it is not pretended to be the effect of the substantial form of the water, but of the excess of the coldness of the air, which (to use vulgar, though perhaps inaccurate expressions) puts the water out of its natural state of fluidity and into a preternatural one of firmness and brittleness. And the reason why snow seldom loses its whiteness but with its nature, seems to be, that its component particles are so disposed, that the same heat of the ambient air that is fit to turn it into a transparent body is also fit to  
make



make it a fluid one, which when it is become, we no longer call it snow, but water; so that the water loses its whiteness, though the snow do not. But if there be a cause proper to make a convenient alteration of texture in the snow without melting or resolving it into water, it may then exchange its whiteness for yellowness without losing its right to be called snow: as I remember I have read in an eminent writer, that *de facto*, in the northern regions towards the pole, those parcels of snow that have lain very long on the ground degenerated in time into a yellowish colour, very differing from that pure whiteness to be observed in the neighbouring snow lately fallen.

BUT there yet remains an argument for substantial forms, which, though (perhaps because physical) it will not be overlooked or slightly answered by their opposers, will for the same reason deserve to be taken notice of here; and it is, that there seems to be a necessity of admitting substantial forms in bodies, that from thence we may derive all the various changes to which they are subject, and the differing effects they produce, the preservation and restitution of the state requisite to each particular body, as also the keeping of its several parts united into one *totum*. To the answering of this argument so many things will be found applicable both in the past and subsequent parts of these notes, that I shall at present but point the chief particulars on which the solution is grounded.

I CONSIDER then, first, that many and great alterations may happen to bodies, which seem manifestly to proceed from their peculiar texture and the action of outward agents upon them, and of which it cannot be shewn that they could happen otherwise, though there were no substantial forms in *rerum natura*: as we see, that tallow (for instance) being melted by the fire, loses its coldness, firmness, and its whiteness, and acquires heat, fluidity, and some transparency; all which being suffered to cool, it presently changes for the three first named qualities. And yet divers of these changes are plainly enough the effects partly of the fire, partly of the ambient air, and not of I know not what substantial form: and as it is both evident and remarkable, what great variety of changes in qualities, and productions in new ones, the fire (that is, a body consisting of insensible parts, that are variously and vehemently moved) doth effect by its heat, that is, by a modified local motion; I consider further, that various operations of a body may be derived from the peculiar texture of the whole, and the mechanical affections of the particular corpuscles or other parts that compose it, as we have often occasion to declare here and there in this treatise; and particularly by an instance, ere long to be further insisted on, namely, that though vitriol made of iron with a corrosive liquor be but a factitious body, made by a convenient apposition of the small parts of the saline menstruum to those of the metal, yet this vitriol will do most, if not all, of the same things, that vitriol made by nature in the bowels of the earth, and digged out thence, will perform: and each of these bodies may be endowed with variety of differing qualities, which I see not why they must flow in the native vitriol from a substantial form, since in the factitious vitriol the same qualities belong to a form that does plainly emerge from the coalition of metalline and saline corpuscles associated together and disposed of after a certain manner.

AND lastly, as to what is very confidently as well as plausibly pretended, That a substantial form is requisite to keep the parts of a body united, without which it would not be one body; I answer, That the contrivance of conveniently figured parts, and in some cases their juxta position, may, without the assistance of a substantial form, be sufficient for this matter. For not to repeat what I just now mentioned concerning vitriol made by art, whose parts are as well united and kept together as those of the native vitriol, I observe, that a pear grafted upon a thorn, or a plumb inoculated upon an apricot, will bear good fruit, and grow up with the stock, as though they both  
made

made but one tree, and were animated but by the same common form; whereas indeed both the stock and the inoculated or grafted plant have each of them its own form, as may appear by the differing leaves, and fruits, and seeds they bear. And that which makes to our present purpose is, that even vegetation and the distribution of aliments are in such cases well made, though the nourished parts of the total plant, if I may so call it, have not one common soul or form; which is yet more remarkable in the mistletoes that I have seen growing upon old hazle-trees, crab-trees, apple-trees, and other plants, in which the mistletoe often differs very widely from that kind of plant on which it grows and prospers. And for the durableness of the union betwixt bodies, that a substantial form is not requisite to procure it, I have been induced to think, by considering that silver and gold, being barely mingled by infusion, will have their minute parts more closely united than those of any plant or animal that we know of. And there is scarce any natural body wherein the form makes so strict, durable, and indissoluble an union of the parts it consists of, as that which in that fictitious concrete we call glass arises from the bare commition of the corpuscles of sand with those saline ones wherewith they are colligated by the violence of the fire: and the like may be said of the union of the proper accidents of glass with the matter of it, and betwixt one another.

To draw towards a conclusion: I know it is alledged as a main consideration on the behalf of substantial forms, that these being in natural bodies the true principles of their properties, and consequently of their operations, their natural philosophy must needs be very imperfect and defective, who will not take in such forms; but for my part I confess, that this very consideration does rather indispose than incline me to admit them. For if indeed there were in every natural body such a thing as a substantial form, from which all its properties and qualities immediately flow, since we see that the actions of bodies upon one another are for the most part (if not all) immediately performed by their qualities or accidents, it would scarce be possible to explicate very many of the explicable phenomena of nature without having recourse to them; and it would be strange if many of the abstruser phenomena were not explicable by them only. Whereas indeed almost all the rational accounts to be met with of difficult phenomena are given by such as either do not acknowledge or at least do not take notice of substantial forms. And it is evident by the clear solutions (untouched by many vulgar philosophers) we meet with of many phenomena in the Staticks and other parts of the Mechanicks, and especially in the Hydrostaticks and Pneumatics, how clearly many phenomena may be solved without employing a substantial form. And on the other side, I do not remember that either Aristotle himself (who perhaps scarce ever attempted it) or any of his followers, has given a solid and intelligible solution of any one phenomenon of nature by the help of substantial forms; which you need not think it strange I should say, since the greatest patrons of forms acknowledging their nature to be \* unknown to us, to explain any effect by a substantial form, must be to declare (as they speak) *ignotum per ignotius*, or at least *per æquè ignotum*. And indeed to explicate a phenomenon being to deduce it from something else in nature more known to us than the thing to be explained by it, how can the employing of incomprehensible (or at least uncomprehended) substantial forms help us to explain intelligibly this or that particular phenomenon? For to say, that such an effect proceeds not from this or that quality of the agent, but from its substantial form, is to take an easy way to resolve all difficulties in general, without rightly resolving any one in particular; and would make

\* *Nomina tu lapidis, qui quotidie tuis oculis observatur, formam, & Phyllida solus habeto.* Scal. contra Card.



a rare philosophy, if it were not far more easy than satisfactory : for if it be demanded why jet attracts straws, rhubarb purges choler, snow dazzles the eyes rather than grass, &c. to say, that these and the like effects are performed by the substantial forms of the respective bodies, is at best but to tell me what is the agent, not how the effect is wrought ; and seems to be but such a kind of general way of answering, as leaves the curious enquirer as much to seek for the causes and manner of particular things, as men commonly are for the particular causes of the several strange things performed by witchcraft, though they be told that it is some devil that does them all. Wherefore I do not think but that natural philosophy, without being for that the more defective, may well enough spare the doctrine of substantial forms as an useless theory ; not that men are arrived to be able to explicate all the phenomena of nature without them, but because whatever we cannot explicate without them, we cannot neither intelligibly explicate by them.

AND thus, Pyrophilus, I have offered you some of those many things that indisposed me to acquiesce in the received doctrine of substantial forms ; but in case any more piercing enquirer shall persuade himself that he understands it thoroughly, and can explicate it clearly, I shall congratulate him for such happy intellectuals, and be very ready to be informed by him. But since what the schools are wont to teach of the origin and attributes of substantial forms, is that which I confess I cannot yet comprehend ; and since I have some of the most eminent persons among the modern philosophers to join with me, though perhaps not for the same considerations, in the like confession, that it is not necessary the reason of my not finding this doctrine conceivable must be rather a defectiveness in my understanding, than the unconceivable nature of the thing itself ; I, who love not (in matters purely philosophical) to acquiesce in what I do not understand, nor to go about to explicate things to others by what appears to me itself inexplicable, shall, I hope, be excused, if, leaving those that contend for them the liberty of making what use they can of substantial forms, I do, till I be better satisfied, decline employing them myself, and endeavour to solve those phenomena I attempt to give an account of, without them ; as not scrupling to confess, that those that I cannot explicate, at least in a general way, by intelligible principles, I am not yet arrived to the distinct and particular knowledge of.

Now for our doctrine touching the origin of forms, it will not be difficult to collect it from what we formerly discoursed about qualities and forms together : for the form of a natural body being, according to us, but an essential modification, and as it were the stamp of its matter ; or such a convention of the bigness, shape, motion (or rest) situation and contexture (together with the thence resulting qualities) of the small parts that compose the body, as is necessary to constitute and denominate such a particular body ; and all these actions being producible in matter by local motion, it is agreeable to our hypothesis to say, that the first and universal, though not immediate cause of forms, is none other but God, who put matter into motion (which belongs not to its essence) and established the laws of motion amongst bodies, and also, according to my opinion, guided it in divers cases at the beginning of things ; and that, among second causes, the grand efficient of forms in local motion, which, by variously dividing, sequestering, transposing, and so connecting the parts of matter, produces in them those accidents and qualities, upon whose account the portion of matter they diversify, comes to belong to this or that determinate species of natural bodies, which yet is not so to be understood, as if motion were only an efficient cause in the generation of bodies, but very often (as in water, fire, &c.) it is also one of the chief accidents that concur to make up the form.

BUT in this last summary account of the origin of forms, I think myself obliged to declare to you a little more distinctly what I just now intimated to be my own opinion. And this I shall do by advertising you, that though I agree with our Epicureans in thinking it probable that the world is made up of an innumerable multitude of singly insensible corpuscles endowed with their own sizes, shapes, and motions; and though I agree with the Cartesians in believing (as I find that \* Anaxagoras did of old) that matter hath not its motion from itself, but originally from God; yet in this I differ both from Epicurus and Des Cartes, that whereas the former of them plainly denies that the world was made by any deity (for deities he owned) and the latter of them, for aught I can find in his writings, or those of some of his most eminent disciples, thought that God, having once put matter into motion, and established the laws of that motion, needed not more particularly interpose for the production of things corporeal, nor even of plants or animals, which, according to him, are but engines: I do not at all believe that either these Cartesian laws of motion, or the Epicurean casual concurrence of atoms, could bring mere matter into so orderly and well contrived a fabrick as this world; and therefore I think, that the wise Author of nature did not only put matter into motion, but, when he resolved to make the world, did so regulate and guide the motions of the small parts of the universal matter, as to reduce the greater systems of them into the order they were to continue in; and did more particularly contrive some portions of that matter into seminal rudiments or principles, lodged in convenient receptacles (and as it were wombs) and others into the bodies of plants and animals: one main part of whose contrivance did, as I apprehend, consist in this, that some of their organs were so framed, that, supposing the fabrick of the greater bodies of the universe, and the laws he had established in nature, some juicy and spirituous parts of these living creatures must be fit to be turned into prolific seeds, whereby they may have a power, by generating their like, to propagate their species. So that, according to my apprehension, it was at the beginning necessary that an intelligent and wise agent should contrive the universal matter into the world (and especially some portions of it into seminal organs and principles) and settle the laws according to which the motions and actions of its parts upon one another should be regulated; without which interposition of the world's architect, however, moving matter may, with some probability (for I see not in the notion any certainty) be conceived to be able, after numberless occurrences of its insensible parts, to cast itself into such grand conventions and convolutions as the Cartesians call *vortices*, and (as I remember) † Epicurus speaks of under the name of *πρῶταίσις, καὶ διήσεις*; yet I think it utterly improbable that brute and unguided, though moving matter, should ever convene into such admirable structures as the bodies of perfect animals. But the world being once framed, and the course of nature established, the naturalist (except in some few cases where God or incorporeal agents interpose) has recourse to the first cause but for its general and ordinary support and influence, whereby it preserves matter and motion from annihilation or desition; and in explicating particular phaenomena considers only the size, shape, motion (or want of it) texture, and the resulting qualities and attributes of the small particles of matter. And thus in this great automaton, the world (as in a watch or clock) the materials it consists of being left to themselves, could never at the first convene into so curious an engine: and yet when the skilful artist has once made and set it a going, the phaenomena it exhibits are to be accounted for by the number, bigness, proportion, shape,

\* Aristot'e, speaking of Anaxagoras in the first chapter of his last book of his *Physicks*, hath this passage: *Dicit (Anaxagoras) cum omnia simul essent, atque quiescerent tempore infinito, mentem movisse ac agere.*

† Epicurus in his epistle to Pythocles.



motion (or endeavour) rest, coaptation, and other mechanical affections of the spring, wheels, pillars, and other parts it is made up of: and those effects of such a watch that cannot this way be explicated, must, for aught I know, be confessed not to be sufficiently understood.

BUT to return thither, whence my duty to the author of nature obliged me to make this short digression:

THE hitherto proposed hypothesis touching the origination of forms hath, I hope, been rendered probable by divers particulars in the past discourses, and will be both exemplified and confirmed by some of the experiments that make the latter part of this present treatise, (especially the fifth and seventh of them) which containing experiments of the changing the form of a salt and a metal, do chiefly belong to the historical or experimental part of what we deliver touching the origin of forms: and indeed, besides the two kinds of experiments presently to be mentioned, we might here present you a third sort consisting partly of divers relations of metalline transmutations delivered upon their own credit by credible men that are not alchymists, and partly of some experiments (some made, some directed by us) of changing both bodies totally inflammable almost totally into water; and a good part even of distilled rain water without addition into earth; and distilled liquors readily and totally mingleable with water *pro parte* into a true oil that will not mix with it: this sort of experiments, I say, I might here annex if I thought fit in this place, either to lay any stress upon those that I cannot myself make out, or to transfer hither those experiments of changes amongst bodies not metalline that belong to another \*treatise: but over and above what the past notes and the experiments that are to follow them contain towards the making of what we teach concerning forms; we will here for further confirmation proceed to add two sorts of experiments (besides the third already mentioned): the one, wherein it appears that bodies of very differing natures being put together, like the wheels and other pieces of a watch, and by their connection acquiring a new texture, and so new qualities may, without having recourse to a substantial form, compose such a new concrete as may as well deserve to have a substantial form attributed to it by virtue of that new disposition of its parts as other bodies that are said to be endowed therewith; and the other, that a natural body being dissipated and as it were taken in pieces like a watch, may have its parts so associated as to constitute new bodies of natures very differing from its own and from each other, and yet these dissipated and scattered parts, by being re-collected and put together again like the pieces of a watch in the like order as before, may recompose (almost, if not more than almost) such another body as that they made up before they were taken asunder.

## I. Experiments and Thoughts about the Production and Reproduction of Forms.

IT was not at random that I spoke, when in the foregoing notes, about the origin of qualities, I intimated that it was very much by a kind of tacit agreement that men had distinguished the species of bodies, and that those distinctions were more arbitrary

\* The Sceptical Chymist.

than we are wont to be aware of; for I confess that I have not yet, either in *Aristotle's* or any other writer, met with any genuine and sufficient diagnostic and boundary for the discriminating and limiting the species of things; or to speak more plainly, I have not found that any naturalist has laid down a determinate number and sort of qualities or other attributes, which is sufficient and necessary to constitute all portions of matter endowed with them, distinct kinds of natural bodies: and therefore I observe that most commonly men look upon these as distinct species of bodies that have had the luck to have distinct names found out for them, though perhaps divers of them differ much less from one another than other bodies, which (because they have been huddled up under one name) have been looked upon as but one sort of bodies. But not to lay any weight on this intimation about names, I found that for want of a true characteristick or discriminating note, it hath been and is still both very uncertain as to divers bodies, whether they are of different species or of the same, and very difficult to give a sufficient reason why divers bodies wherein nature is assisted by art should not as well pass for distinct kinds of bodies as others that are generally reckoned to be so.

WHETHER (for instance) water and ice be not to be esteemed distinct kinds of bodies is so little evident that some that pretend to be very well versed in *Aristotle's* writings and opinions, affirm him to teach that water loses not its own nature by being turned into ice; and indeed I remember I have read a \* text of his that seems express enough to this purpose, and the thing itself is made plausible by the reducibleness of ice back again into water. And yet I remember *Galen* is affirmed to make these two distinct species of bodies, which doctrine is favoured by the differing qualities of ice and water: for not only the one is fluid and the other solid and even brittle; but ice is also commonly more or less opacous in comparison of water, being also lighter than it in species, since it swims upon it. To which may be added, that ice beaten with common salt will freeze other bodies when water mingled with salt will not: and on this occasion I would propose to be resolved, whether must wine, spirit of wine, vinegar, tartar, and vappa be specifically distinct bodies? And the like question I would ask concerning a hen's egg and the chick that is afterwards hatched out of it; as also concerning wood, ashes, soot, and likewise the eggs of silkworms, which are first small caterpillars, or (as some think them) but worms when they are newly hatched, and then aurelias, (or husked maggots), and then butterflies; which I have observed with pleasure to be the successive production of the prolific seed of silk-worms. And whether the answer to these queries be affirmative or negative, I doubt, the reason that will be given for either of the two will not hold in divers cases whereto I might apply it. And a more puzzling question it may be to some, whether a charcoal being thoroughly kindled do specifically differ from another charcoal? For according to those I argue with, the fire has penetrated it quite through; and therefore some of the recent Aristotelians are so convinced of its being transmuted, that all the satisfaction I could find from a very subtle modern schoolman to the objection, that if the glowing coal were plunged into water it would be a black coal again; was that, notwithstanding that reduction, the form of a charcoal had been once abolished by the fire, and was reproduced by God upon the regained disposition of the matter to receive it.

\* See *Lib. 1. de Gen. & Corr. t. 80. Idem corpus* (says he there) *quoniam continuum, aliis liquidum, aliis concretum videmus, non divisione aut compositione hoc passum, aut conversione, aut actu, sicuti Democritus asserit: nam neque transpositione, neque Natura demutatione (ἡ τὸ μεταβάλλον τὴν φύσιν) ex liquido concretum evadere solet.*



NOR is it very easy to determine whether clouds, and rain, and hail, and snow, be bodies specifically distinct from water and from each other; and the writers of meteors are wont to handle them as distinct. And if such slight differences as those, that discriminate these bodies, or that which distinguishes wind from exhalations, who's course makes it be sufficient to constitute differing kinds of bodies, it will be hard to give a satisfactory reason why other bodies that differ in more, or more considerable particulars, should not enjoy the same privilege: and I presume that snow differs less from rain than paper doth from rags, or glass made of wood-ashes does from wood. And indeed men having by tacit consent agreed to look upon paper, and glass, and soap, and sugar, and brass, and ink, and pewter, and gunpowder, and I know not how many others, to be distinct sorts of bodies; I see not why they may not be thought to have done it on as good grounds as those upon which divers other differing species of bodies have been constituted. Nor will it suffice to object that these bodies are factitious; for it is the present nature of bodies that ought to be considered in referring them to species, which way soever they came by that nature; for salt that is in many countries made by boiling sea-water in cauldrons and other vessels, is as well true sea-salt as that which is made in the *Isle of Man* (as navigators call it) without any co-operation of man, by the bare action of the sun upon those parts of the sea-water which chance to be left behind in hollow places after a high spring-tide. And silk-worms which will hatch by the heat of human bodies, and chickens that are hatched in *Egypt* by the heat of ovens or dunghills are no less true silk-worms or chickens than those that are hatched by the sun or by hens.

As for what may be objected, that we must distinguish betwixt factitious bodies and natural, I will not now stay to examine how far that distinction may be allowed; for it may suffice for our present purpose to represent, that whatever may be said of factitious bodies, where man does, by instruments of his own providing, only give figure, or also contexture to the sensible (not insensible) parts of the matter he works upon; as when a joiner makes a stool, or a statuary makes an image, or a turner a bowl: yet the case may be very differing in those other factitious productions wherein the insensible parts of matter are altered by natural agents who perform the greatest part of the work among themselves, though the artificer be an assistant, by putting them together after a due manner. And therefore I know not why all the productions of the fire made by chymists should be looked upon as not natural, but artificial bodies; since the fire, which is the grand agent in these changes, doth not, by being employed by the chymist, cease to be and to work as a natural agent: and since nature herself doth, by the help of the fire, sometimes afford us the like productions that the alchymist's art presents us: as in *Ætna*, *Vesuvius*, and other burning mountains, (some of whose productions I can shew you), stones are sometimes turned into lime, (and so an alkalizate salt is produced), and sometimes, if they be more disposed to be fluxed than calcined, brought to vitrification; metalline and mineral bodies are by the violence of the fire colligated into masses of very strange and compounded natures. Ashes and metalline flowers of divers kinds are scattered about the neighbouring places, and copious flowers of sulphur sublimed by the internal fire, have been several times found about the vents at which the fumes are discharged into the air: (as I have been assured by ingenious visitors of such places, whom I purposely inquired of touching these *flours*; for of these travellers more than one answered me they had themselves gathered, and had brought some very good). Not to add, that I have sometimes suspected, upon no absurd grounds, that divers of the minerals and other bodies we meet with in the lower parts of the earth, and think to have been formed and lodged there ever since the beginning of things, have been



since produced there by the help of subterranean fires or other heats, which may, either by their immediate action and exceedingly long application, very much alter some bodies by changing their texture; as when lead is turned into minium and tin into putty, by the operation of the fire in a few hours, or by elevating, in the form of exhalations or vapours, divers saline and sulphureous corpuscles or particles of unripe, or (to use a chymical term of art) embryonated minerals, and perhaps metals, which may very much alter the nature, and thereby vary the kind of other subterranean bodies which they pervade, and in which they often come to be incorporated; or else may, by convening among themselves, constitute particular concretions; as we see that the fumes of sulphur and those of mercury unite into that lovely red mass which in the shops they call vermilion, and which is so like to the mineral whence we usually obtain mercury; that the Latins give them both the same name, *Cinnabaris*, and in that are imitated by the French and Italians; in whose favour I shall add, that if we suppose this mineral to consist of a stony concretion penetrated by such mineral fumes as I have been speaking of, the appellation may be better excused than perhaps you imagined; since from *Cinnabaris nativa* not only I obtained a considerable quantity of good running mercury (which is that men are wont to seek for from it) but, to gratify my curiosity somewhat further, I tried an easy way that came into my mind, whereby the *caput mortuum* afforded me no despicable quantity of good combustible sulphur: but this upon-the-by, being not obliged to set down here the grounds of my paradoxical conjecture about the effects of subterranean fires and heats since I here lay no stress upon it, but return to what I was saying about *Ætna* and other Volcanos. Since then these productions of the fire being of nature's own making, cannot be denied to be natural bodies, I see not why the like productions of the fire should be thought unworthy that name, only because the fire that made the former was kindled by chance in a hill, and that which produced the latter was kindled by a man in a furnace. And if flower of sulphur, lime, glass, and colligated mixtures of metals and minerals are to be reckoned among natural bodies, it seems to be but reasonable, that, upon the same grounds, we should admit flower of antimony, lime, and glass, and pewter, and brass, and many other chymical concretes (if I may so call them) to be taken into the same number; and then it will be evident that, to distinguish the species of natural bodies, a concurrence of accidents will, without considering any substantial form, be sufficient.

BUT because I need not on this occasion have recourse to instances of a disputable nature, I will pitch, for the illustration of the mechanical production of forms, upon vitriol: for since nature herself without the help of art does oftentimes produce that concrete (as I have elsewhere shewn by experience) there is no reason why vitriol, produced by easy chymical operations should not be looked upon as a body of the same nature and kind: and in factitious vitriol, our knowing what ingredients we make use of, and how we put them together, enables us to judge very well how vitriol is produced. But because it is wont to be reckoned with saltpetre, sea-salt, and sal-gem; among true salts, I think it requisite to take notice, in the first place, that vitriol is not a mere salt, but that which *Paracelsus* somewhere, and after him divers other Spagyrist call a *magistery*, which in their sense (for there are that use it in another) commonly signifies a preparation, wherein the body to be prepared has not its principles separated; as in distillation, incineration, &c. but wherein the whole body is brought into another form by the addition of some salt or menstruum that is united *per minima* with it. And agreeably to this notion, we find that from common vitriol, whether native or factitious, may be obtained (by distillation and reduction) an acid saline spirit, and a metalline substance, as I elsewhere mention, that from blue vitriol copper may be (by more  
than



than one way) separated. And I the rather give this advertisement, because that as there is a vitriol of iron, which is usually green, and another of copper, which is wont to be blue, and also a white vitriol about which it is disputed what it holds (though that it holds some copper I have found); and yet of all these, are without scruple reputed true vitriols, notwithstanding that they differ so much in colour, and (as I have discovered) in several other qualities; so I see no reason why the other minerals, being reduced by their proper menstruums into salt like magisteries, may not pass for the vitriols of those metals, and consequently for natural bodies; which if granted, will add some confirmation to our doctrine, though its being granted is not necessary to make it out. For to confine ourselves to vitriol, it is known among chymists, that if upon the filings of *Mars* one put a convenient quantity of that acid distilled liquor, which is (abusively) wont to be called oil of vitriol, diluting the mixture with rain or with common water, it is easy by filtrating the solution, by evaporating the aqueous superfluity of it, and by leaving the rest for a competent while in a cellar (or other cold place) to crystallize; it is easy, I say, by this means to obtain a vitriol of iron; which agrees with the other vitriol of vitriol-stones or marchasites presented us by nature without the help of any other menstruum than the rain that falls upon them from the clouds, in I know not how many qualities, part obvious, and part of them occult: as (of the first sort) in colour, transparency, brittleness, easiness of fusion, styptical taste, reducibleness to a red powder by calcination, and other qualities more obvious to be taken notice of; to which may be annexed divers qualities of the second sort, (I mean the more abstruse ones), as the power to turn in a trice an infusion of galls made in ordinary water, (as also to turn a certain clear mineral solution, elsewhere mentioned) into an inky colour, to which in all probability we may add a faculty of causing vomits even in a small dose, when taken into the stomach of a man, and that remarkable property of being endowed with as exact and curious a shape or figure as those for which salts have been, by modern philosophers especially, so much admired. But that no scruple might arise from hence, that in the *vitriolum maris* wont to be made by chymists, the menstruum that is employed is the oil of common vitriol, which may be suspected to have retained the nature of the concrete whence it proceeded; and so this factitious vitriol may not be barely a new production, but partly a recorporification, as they speak, of the vitriolate corpuscles contained in the menstruum: to prevent this scruple, I say (which yet perhaps would not much trouble a considering chymist) I thought fit to employ a quite other menstruum that would not be suspected to have any thing of vitriol in it. And though aqua fortis and spirit of nitre, however they corrode *Mars*, are unfit for such a work; yet having pitched upon spirit of salt instead of oil of vitriol, and proceeding the same way, that has been already set down, it answered our expectation, and afforded us a good green vitriol. Nor will the great disposition I have observed in this our vitriol to resolve, by the moisture of the air into a liquor, make it essentially differing from other vitriols, since it has been observed, and particularly by *Guntberus Belichius* more than once, that even the common vitriol he used in *Germany* will also, though not so easily as other salts, run (as the chymists phrase it) *per deliquium*. And to make the experiment more complete, though we did not find either oil of vitriol or spirit of salt, good menstruums to make a blue venereal vitriol out of copper (however filed or thinly laminated) and though upon more trials than one, it appeared that aqua fortis and spirit of nitre, which we thought fit to substitute to the above-mentioned liquors, did indeed make a solution of copper, but so unctuous a one that it was very hard to bring any part of it to dryness without spoiling the colour and shape of the desired body: yet repeating the experiment with care and watchfulness, we this way obtained one of the loveliest



loveliest vitriols that hath perhaps been seen, and of which you yourself may be the judge by a parcel of it I keep by me for a rarity.

To apply now these experiments, especially that wherein spirit of salt is employed, to the purpose for which I have mentioned them, let us briefly consider these two things; the one, that our facitious vitriol is a body that, as well as the natural, is endowed with many qualities (manifest and occult) not only such as are common to it with other salts, as transparency, brittleness, solubleness in water, &c. but such as are properties peculiar to it, as greenness, easiness of fusion, stypticity of taste, a peculiar shape, a power to strike a black with infusion of galls, an emetick faculty, &c.

THE other thing we are to consider is, that though these qualities are in common vitriol believed to flow from the substantial form of the concrete; and may as justly as the qualities, whether manifest or occult, of other inanimate bodies, be employed as arguments to evince such a form: yet in our vitriol, made with spirit of salt, the same qualities and properties were produced by the associating and juxtaposition of the two ingredients, of which the vitriol was compounded; the mystery being no more but this, that the steel being dissolved in the spirit, the saline particles of the former, and the metalline ones of the latter having each their determinate shapes, did, by their association, compose divers corpuscles of a mixed or compounded nature, from the convention of many whereof there resulted a new body of such a texture, as qualified it to affect our sensories, and work upon other bodies, after such a manner as common vitriol is wont to do. And indeed in our case, not only it cannot be made appear, that there is any substantial form generated anew, but that there is not so much as an exquisite mixture, according to the common notion the schools have of such a mixture. For both the ingredients retain their nature (though perhaps somewhat altered) so that there is, as we were saying, but a juxtaposition of the metalline and saline corpuscles; only they are associated so, as by the manner of their coalition to acquire that new texture which denominates the magistery they compose, vitriol. For it is evident that the saline ingredient may either totally, or for much the greatest part be separated by distillation, the metalline remaining behind. Nay, some of the qualities we have been ascribing to our vitriol, do so much depend upon texture, that the very beams of the sun (converged) will, as I have purposely tried, very easily alter its colour, as well as spoil its transparency, turning it at first from green to white; and, if they be concentrated by a good burning glass, making it change that livery for a deep red.

*Doubts and Experiments touching the curious Figures of Salts.*

AND here let me take notice, that though the exact and curious figures, in which vitriol and other salts are wont to shoot, be made arguments of the presence, and great instances of the plattick skill of substantial-forms and seminal powers; yet, I confess, I am not so fully satisfied in this matter, as even the modern philosophers appear to be. It is not that I deny that *Plato's* excellent saying, *γινωσκети δ' εδωκ',* may be applied to these exquisite productions of nature. For though God has thought fit to make things corporeal after a much more facile and intelligible way, than by the intervention of substantial forms; and though the plattick power of seeds, which in plants and animals I willingly admit, seem not in our case to be needful; yet is the divine architect's geometry



geometry (if I may so call it) nevertheless to be acknowledged and admired. For having been pleased to make the primary and insensible corpuscles of salts and metals of such determinate, curious, and exact shapes, that as they happen to be associated together, they should naturally produce concretions; which, though differinglly figured, according to the respective natures of their ingredients, and the various manners of their conveining, should yet be all of them very curious, and seem elaborate in their kinds; how little I think is fit to be allowed, that the bodies of animals, which consist of so many curiously framed and wonderfully adapted organical parts, and whose structure is a thousand times more artificial than that of salts and stones, and other minerals, can be reasonably supposed to have been produced by chance, or without the guidance of an intelligent author of things, I have elsewhere largely declared. But I confess I look upon these figures we admire in salts, and in some kind of stones (which I have not been incurious to collect) as textures so simple and slight, in comparison of the bodies of animals, and oftentimes in comparison of some one organical part, that I think it cannot be in the least inferred, that because such slight figurations need not be ascribed to the plastick power of seeds, it is not necessary, that the stupendous and incomparably more elaborate fabrick and structure of animals themselves should be so. And this premised I shall add, that I have been inclined to the conjecture about the shapes of salts, that I lately proposed by these considerations.

FIRST, That by a bare association of metalline and saline corpuscles, a concrete, as finely figured as other vitriols may be produced, as we have lately seen.

SECONDLY, Because that the figures of these salts are not constantly in all respects the same, but may in divers manners be somewhat varied, as they happen to be made to shoot more hastily or more leisurely, and as they shoot in scander or fuller proportion of liquor. This may be easily observed by any that will but with a little attention consider the difference that may be found in vitriolate crystals or grains, when quantities of them were taken out of the great coolers, as they call them, wherein that salt, at the works where it is boiled, is wont to be set to shoot. And accordingly where the experienced mineralist *Agricola* describes the several ways of making vitriol in great quantities, he does not only more than once call the great grains or crystals, into which it coagulates, cubes; but speaking of the manner of their concretion about the cords or ropes, that are wont (in *Germany*) to be hanged from certain cross bars into the vitriolate water or solution for the vitriol to fasten itself to, he compares the concretions indifferently to cubes or clusters of grapes; *Ex his* (says he, speaking of the cross-bars) *pendent resses lapillis extente, ad quos humor spissus adhaerescens densatur in translucentes atramenti sutorii vel cubos, vel acinos, qui uvæ speciem gerunt* \*. I remember also, that having many years since a suspicion that the reason why alkalies, such as salt of tartar and pot-ashes, are wont to be obtained in the form of white powders or calces, might be the way wherein the water of the lixiviums, that contain them, is wont to be drawn off; I fancied that by leaving the saline corpuscles a competent quantity of water to swim in, and allowing them leisure for such a multitude of occurrences as might suffice to make them hit upon more congruous coalitions than is usual, I might obtain crystals of them, as well as of other salts: conjecturing this, I say, I caused some well purified alkalies dissolved in clear water to be slowly evaporated, till the top was covered with a thin ice-like crust; then taking care not to break that, lest they should (as in the ordinary way, where the water is all forced off) want a sufficient stock of liquor I kept them in a very gentle heat for a good while; and then breaking the above-mentioned ice-like cake, I

\* *Georg. Agricola de re metall. lib. 12. p. 462.*



had, as I wished, divers figured lumps of crystalline salt shot in the water, and transparent almost like white sugar-candy.

I LIKEWISE remember that having on several occasions distilled a certain quantity of oil of vitriol with a strong solution of sea-salt, till the remaining matter was left dry, that saline residue being dissolved in fair water, filtered, and gently evaporated, would shoot into crystals, sometimes of one figure, sometimes of another, according as the quantity or strength of the oil of vitriol and other substances determined. And yet these crystals, though sometimes they would shoot into prism-like figures, as roched petre; and sometimes into shapes more like to allom or vitriol; nay, though oftentimes the same *caput mortuum* dissolved would in the same glass shoot into crystals, whereof some would be of one shape, some of another; yet would these differing grains or crystals appear for the most part more exquisitely figured, than oftentimes vitriol does. From spirit of urine and spirit of nitre, when I have suffered them to remain long together before coagulation, and freed the mixture from the superfluous moisture very slowly, I have sometimes obtained fine long crystals (some of which I can shew you) so shaped, that most beholders would take them for crystals of salt-petre. And I have likewise tried, that whereas silver is wont to shoot into plates exceeding thin, almost like those of *Moscovia* glass, when I have dissolved a pretty quantity of it in aqua fortis or spirit of nitre, and suffered it to shoot very leisurely, I have obtained lunar crystals, (several of which I have yet by me), whose figure, though so pretty as to have given some wonder even to an excellent geometrician, is differing enough from that of the thin plates formerly mentioned; each crystal being composed of many small and finely shaped solids, that stick so congruously to one another, as to have one surface that appeared plain enough, common to them all.

THIRDLY, That insensible corpuscles of different, but all of them exquisite shapes, and endowed with plain, as well as smooth sides, will constitute bodies variously, but all very finely figured; I have made use of several ways to manifest. And first, though harts horn, blood, and urine, being resolved, and (as the chymists speak) analyzed by distillation, may well be supposed to have their substantial forms (if they had any) destroyed by the action of the fire; yet in regard the saline particles they contain, are endowed with such figures as we have been speaking of, when in the liquor, that abounds with either of these volatile salts, the dissolved particles do leisurely shoot into crystals, I have divers times observed in these many masses (some bigger and some less) whose surfaces had plains, some of figures, as to sense, exactly geometrical, and others very curious and pleasant. And of these finely shaped crystals of various sizes, I have pretty store by me. And because (as it may be probably gathered from the event) the saline corpuscles of stillatitious acid liquors, and those of many of the bodies they are fitted to dissolve, have such kind of figures as we have been speaking of, when the solutions of these bodies, upon the recess of the superfluous moisture, shoot into crystals; these, though they will some times be differing enough, according the particular natures of the dissolved bodies and the menstruum, yet either the crystals themselves, or their surfaces, or both, will oftentimes have fine and exquisite figures; as I have tried by a menstruum, wherewith I was able to dissolve some gems, as also with a solution of coral made with spirit of verdigrease, to omit other examples. And for the same reason when I tried, whether the particles of silver, dissolved in aqua fortis, would not, without coagulating with the salts, convene, upon the account of their own shapes, into little concretions of smooth and flat surfaces, I found that having (to afford the metalline corpuscles scope to move in) diluted one part of the solution with a great many parts of distilled rain-water, for common water will often-times make such solutions become white or turbid) a plate of copper being suspended in the liquor, and suffered to lie



lie quiet there a while, (for it need not be long) there would settle all about it swarms of little metalline and undiaphanous bodies, shining in the water like the scales of small fishes, but formed into little plates extremely thin, with surfaces not only flat, but exceeding glossy: and among those, divers of the larger were prettily figured at the edges. And as for gold, its corpuscles are sufficiently disposed to convene with those of fit or congruous salts into concretions of determinate shapes, as I have found in the crystals I obtained from gold dissolved in aqua regis, and after having been suffered to lose its superfluous moisture, kept in a cold place; and not only so, but also when, by a more powerful menstruum, I had subdivided the body of gold into such minute particles, that they were sublimable, (for that I can assure you is possible;) these volatile particles of gold, with the salts, wherewith they were elevated, afforded me (sometimes) store of crystals, which, though not all of them near of the same bigness, resembled one another in their shape, which was regular enough, and a very pretty one. But of this more elsewhere.

§ I REMEMBER I have long since taken pleasure to dissolve two or more of those saline bodies, whose shapes we know already, in fair water, that by a very gentle evaporation I might obtain concretions whose shapes should be, though curious, yet differing from the figure of either of the ingredients. But we must not expect that in all cases the salts dissolved together should be totally compounded: for oftentimes they are of such different natures, that one will shoot much sooner than another, and then it frequently happens, that a good proportion of that will be first crystallized in its own shape; as is conspicuously to be observed in the refining of that impure petre, (which from the country, that affords it, the purifiers call *Barbary nitre*), from the common salt it abounds with: and (also) as *Agricola* observes\*, that in some cases, where a vitriolate matter is mingled with that which yields allom, those two kinds of salts will shoot separately in the same large vessel; (which the trials I have made with the compounded solutions of those two salts do not discountenance.) Now in such cases all that can be expected, or need be desired, is, that the remaining part of the mixture, or some portion of it, afford crystals or grains of compounded solid figures. Though the *Venetian borax*, wont to be sold in shops, be known to be a factitious body, compounded of several salts, that I shall not now stay to enumerate; and though when we buy it, we usually find it to consist of lumps and grains misshapen enough, yet when I dissolved some of it in a good quantity of fair water, and made it coagulate very leisurely, I had crystals, upon whose surfaces I could perceive very exquisite, and, as to sense, regular and geometrical figures. And one thing I must not here by any means pretermitt, which is, that though the *caput mortuum* of common aqua fortis consists of bodies of very differing natures (for such are nitre and vitriol), and has been exposed to a great violence of the fire; yet I have sometimes admired the curiousness of those figures, that might be obtained barely by frequent solutions and coagulations of the saline particles of this *caput mortuum* in fair water. But because the glasses wherein my concretions were made, were too little to afford great crystals, and they ought to shoot very slowly; I chose rather to shew the curious some large crystals, which I took out of the laboratory of an ingenious person, who, without minding the figures, had upon my recommendation made great quantity of that salt in large vessels for a medicine; (it being the *Panacea duplicata*, so famous in *Holstein*). For divers of these crystals have not only triangles, hexagons, and rhomboides, and other figures exquisitely cut on their smooth and specular surfaces, and others, bodies of prismatical shapes: but some of them are no less accurately figured than the finest nitre or vitriol I remember myself to have observed, and

\* *G. Agricola de Re Metallica*, lib. 12.

some also terminate in bodies almost like pyramids, consisting of divers triangles, that meet in one vertical point, and are no less admirably shaped, than the fairer sort of *Cornish* diamonds, that have been brought me for rarities. Besides the producing of salts of new shapes by compounding of saline bodies, I have found it to be practicable not only in some gross, or, as they speak, corporal salts, such as sea-salt, salt-petre, but also in some natural and some chymical salts dissolved together, and which perhaps you will think more considerable in saline spirits made by distillation; not that all of them are fit for this purpose, but that I have found divers of those that work upon one another with ebullition, to be so. For in that consist the saline corpuscles, come to be associated to one another, and thereby, or by their newly acquired figure, whilst their coalition lasts, to lose much of their former volatility: so that upon evaporation of the superfluous liquor, they will not fly as otherwise they might, but coagulate into finely shaped crystals, as I have tried, among other saline liquors, with spirit of urine, and spirit of nitre, and with oil of vitriol, and spirit of fermented urine, with spirit of sheep's blood, and spirit of salt, and also with the spirits of salt and urine; which last experiment I the rather mention, because it shews by the difference of the crystals, afforded by those two liquors from the crystals resulting from one of them, namely, the spirit of urine (or, if you please, the volatile salt wherewith it abounds), coagulated with a fit dose of oil of vitriol, how much those compound emergent figures depend upon the more simple figures of the saline corpuscles, that happen to convene into those new concretes. For the spirit of urine satiated with spirit of salt, and both very gently, and not too far evaporated, often afforded me crystals that differed exceedingly in shape from those which I obtained from the same spirit of urine, satiated either with oil of vitriol, or with spirit of nitre. For (to add that upon the by) that salt, compounded of the two spirits of urine and of common salt, is wont to be very prettily figured, consisting of one long beam as it were, whence on both sides issue out far shorter crystals, sometimes perpendicular to that, and parallel to one another like the teeth in a comb, and sometimes so inclining as to make the whole appear almost like a feather; which is the more remarkable, because I have (many years ago) observed that common sal-armoniack that is made of urine and common salt, both crude, with a proportion of foot, will, if warily dissolved and coagulated, shoot into crystals of the like shape. How far the unknown figure of a salt may possibly (for I fear it will not easily) be guessed at by that of the figure which it makes with some other salt, whose figure is already known, I leave to geometricians to consider; having, I fear, insisted too long on this subject already. But yet I must add one particular more, which will as well illustrate and confirm much of what has been said above touching the organization of vitriol, as shew that the shape of vitriol depends upon the texture of the bodies whereof it is composed.

FOURTHLY, then, when I considered that (as I formerly noted) vitriol being but a magistery, made by the concoction of the corpuscles of a dissolved metal with those of the menstruum, the magisteries of other metals might, without inconvenience, be added, as other vitriolate concretes, to the green, the blue, and white vitriol, that are without scruple referred to the same species: and when I considered that oil of vitriol was not a fit menstruum to dissolve divers of the metals, nor even all those that it will corrode; and that the like unsuitness also is to be found in common spirit of salt; I pitched upon aqua fortis, or spirit of nitre, as that menstruum which was likeliest to afford variety of vitriols. And accordingly I found, that besides the lovely vitriol of copper formerly mentioned, that liquor would with quicksilver afford one sort of crystals, with silver another, and with lead a third; all which crystals of vitriol, as they differed from each other in other qualities (upon which score you will find this experiment else-

where



where mentioned), so they did very manifestly and considerably differ in shape; the crystals of silver shooting in exceeding thin plates, and those of lead and quicksilver obtaining figures, though differing enough from each other, yet of a far greater depth and thickness, and less remote from the figure of common vitriol or sea-salt: and yet all these vitriols, especial that of crude lead, when it was happily made, had shapes curious and elaborate, as well as those we admire in common vitriol or sea-salt.

If then these curious shapes, which are believed to be of the admirablest effects, and of the strongest proofs of substantial forms, may be the results of texture; and if art can produce vitriol itself as well as nature, why may we not think that in ordinary phenomena, that have much less of wonder, recourse is wont to be had to substantial forms without any necessity? (matter, and a convention of accidents being able to serve the turn without them;) and why should we wilfully exclude those productions of the fire, wherein the chymist is but a servant of nature, from the number of natural bodies? And indeed since there is no certain diagnostick agreed on whereby to discriminate natural and factitious bodies, and to constitute the species of both; I see not why we may not draw arguments from the qualities and operations of several of those that are called factitious, to shew how much may be ascribed to, and performed by the mechanical characterization, or stamp of matter; of which we have a noble instance in gun-powder, wherein, by a bare comminution and blending the ingredients, nitre, charcoal, and brimstone, which have only a new, and that an exceeding slight contexture, each retaining its own nature in the mixture, so that there is no colour afforded to the pretence of a substantial form; there is produced a new body, whose operations are more powerful and prodigious than those of almost any body of nature's own compounding. And though glass be but an artificial concrete, yet, besides that it is a very noble and useful one, nature herself has produced very few, if enough to make up a number more lasting and more unalterable. And indeed divers of those factitious bodies that chymistry is able to afford us, are endowed with more various and more noble qualities than many of those that are unquestionably natural. And if we admit these productions into the number of natural bodies, they will afford us a multitude of instances to shew that bodies may acquire many and noble qualities, barely by having mechanical affections introduced by outward agents into the matter, or destroyed there. As though glass be such a noble body as we have lately taken notice of, yet since its fusibility, transparency, and brittleness, that are its only constituent attributes, we can in less than an hour (or perhaps half that time) turn an opacous body into transparent glass without the addition of any other visible body, by a change of texture made in the same matter, and by another change of texture, made without addition, as formerly, we can in a trice reduce glass into, or obtain from it, a body not glassy, but opacous, and otherwise of a very different nature, as it had been before. And here let me add what may not a little conduce to our present design, that even those that embrace *Aristotle's* principles, do unawares confess that a slight change of texture, without the introduction of a substantial form, may not only make a specific difference betwixt bodies, but so vast a one, that they shall have differing geniuses, and may (as the chymists speak) belong to different kingdoms. For coral, to pass by all other plants of that kind that may be mentioned to the same purpose, whilst it grows in the bottom of the sea, is a real plant, and several times (which suffices for my present scope) hath been there found by an acquaintance of mine, as well as by other enquirers, soft and tender like another plant: nay, I elsewhere\* bring very good and recent authority to prove that it is oftentimes found very succulent, and does propagate its species as well as other shrubs; and yet coral being gathered and

\* In the *Essays* about things supposed to be spontaneously generated.



removed into the air, by the recess of its soul, no new lapidifick form being so much as pretended to, turns into a concretion, that is by many eminent writers, and others reckoned among lapideous ones: as indeed coral does not burn like wood, nor obey distillation like it; and not only its calx is very differing from the ashes of vegetables, and is totally soluble in divers acid liquors, and even spirit of vinegar, but the uncalcined coral itself will be easily corroded by good vinegar, after the same manner as I have seen *lapis stellaris*, and other unquestionably mineral stones dissolved, some by that liquor, and some by the spirit of it. A much stranger thing may be seen in the East-India island of *Sombrero*, not very far from *Sumatra*, if we may believe our countryman Sir *James Lancaster*, who relates it as an eye-witness, for which reason, and for the strangeness of the thing, I shall add the story in his own words. Here † says he, speaking of the coast of *Sombrero* we found upon the sand by the sea side a small twig growing up to a young tree; and offering to pluck up the same, it sprunk down into the ground, and sinketh unless you hold very hard. And being plucked up, a great worm is the root of it: and look how the tree groweth in greatness, the worm diminisheth. Now as soon as the worm is wholly turned into the tree, it rooteth in the ground, and so groweth to be great. This transformation was one of the greatest wonders I saw in all my travels. This tree being plucked up a little, the leaves stripped off, and the peel, by that time it was dry, turned into a hard stone, much like to white coral. So that (concludes he) this worm was twice transformed into different natures: of these we gathered and brought home many. The industrious *Piso*, in his excellent history of *Brasil* vouches a multitude of witnesses (not having opportunity to be one himself) for the ordinary transformation of a sort of animals (not much unlike grasshoppers) into vegetables, at a certain season of the \* year.

BUT since I set down this relation of Sir *John Lancaster*, I have met with another, whose strangeness may much countenance it, in a small tract newly published by a Jesuit, *P. Michael Boym*, whom a good critic much commended to me. For this author doth, as an eye-witness affirm, that which is little less to my present purpose: ‡ *Je vis, &c. i. e. I saw in a small fresh-water and shallow lake of the island Hainan (which belongs to China) crabs or craw-fishes, which, as soon as they were drawn out of the water, did in a moment lose both life and motion, and became petrified, though nothing appeared to be changed either in the external or internal figure of their bodies.* What he further adds of these fishes is but of their virtues in physick, which not concerning our subject, I shall, *Pyrophilus*, willingly pretermitt it; and even as to our countryman's relation, hoping by means of an ingenious correspondent in the *East Indies*, to receive a further information about the strange plant he mentions, I shall at present urge only what has been taken notice of concerning coral, to countenance the observation for whose sake these narratives have been alledged. And so likewise as to what I was saying of glass and gunpowder, our receiving of those, and the generality of factitious bodies into the catalogue of natural bodies, is not (which I formerly also intimated) necessary to my present argument: whereto it is sufficient that vitriol is granted on all hands to be a natural body, though it be also producible by art. And also to the argument it affords us we might add that memorable experiment delivered by *Helmont*, of turning oil of vitriol into allom, by the odour (as he calls it) of mercury, if however it be not despicable, we had found it fit to be relied on. But reserving an account of that for another place, we shall substitute the instance presented us by our author about the production of salt-petre: for

† *Purchas Pilgr.* Part the first, p. 152.

\* The passage, which is long, I do not here transcribe, having had occasion to do it elsewhere. It is extant, *lib. 5, cap. 21.* and at the close of his narrative he subjoins, *Non est, quod quisquam de veritate dubitet, cum infinitis testibus habeat Brasiliæ, &c.*

‡ *Flora Sinenfis, ou Traite des Fleurs, &c.* under the Title *Lozimeques*.



if, having dissolved pot-ashes in fair water, you coagulate the filtrated solution into a white salt, and on that pour spirit of nitre till they will not hiss any longer; there will shoot when the superfluous water is evaporated, crystals, that proclaim their nitrous nature by their prismatical (or at least prism-like) shape, their easy fusion, their accension, and deflagration, and other qualities partly mentioned by our author, and partly discoverable by a little curiosity in making trials.

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## II. *Experimental Attempts about the Redintegration of Bodies.*

THE former of those two arguments, *Pyrophilus*, by which I proposed to confirm the origin of forms, was, as you may remember, grounded upon the manner by which such a convention of accidents, as deserves to pass for a form, may be produced: and that having been hitherto profecuted, it now remains that we proceed to the second argument, drawn not (as the former) from the first production, but from the reproduction of a physical body. And though both these arguments are valid, yet if this latter could, in spite of the difficulties intervening in making of the experiments that belongs to it, be as clearly made out as the former, you would, I suppose, like it much the better of the two. For if we could reproduce a body which has been deprived of its substantial form, you would, I presume, think it highly probable, if not more than probable, that (to borrow our author's own expression) that which is commonly called the form of a concrete, which gives it its being and denomination, and from whence all its qualities are in the vulgar philosophy, by I know not what inexplicable ways, supposed to flow, may be in some bodies but a characterization or modification of the matter they consist of; whose parts by being so and so disposed in relation to each other, constitute such a determinate kind of body, endowed with such and such properties: whereas if the same parts were otherwise disposed, they would constitute other bodies of very differing natures from that of the concrete, whose parts they formerly were, and which may again result or be produced after its dissipation, and seeming destruction, by the re-union of the same component particles, associated according to their former disposition.

BUT though it were not possible to make an adequate redintegration of a chymically analyzed body, because some of the dissipated parts will either escape through the junctures of the vessels (though diligently closed), or if they be very subtle, will fly away upon the disjoining of the vessels, or will irrecoverably stick to the inside of them: yet I see not why such a reproduction, as is very possible to be effected, may not suffice to manifest what we intend to make out by it. For even in such experiments it appears, that when the form of a natural body is abolished, and its parts violently scattered by the bare re-union of some parts after the former manner, the very same matter the destroyed was before made of, may, without addition of other bodies, be brought again to constitute a body of the like nature with the former, though not of equal bulk. And indeed, the experiment recorded by our author about the reproduction of salt-petre, as it is the best and successfulest I have ever been able to make upon bodies, that require a strong heat to dissipate them; so I hope it will suffice to give you those thoughts about this matter that the author designed in alledging it; and therefore, though having premised thus much, I shall proceed to acquaint you with the success of some attempts.

he intimates (in that essay) his intention of making for the redintegration of some bodies; yet doing it out of some historical notes, I find among my loose papers, that which I at present pretend to, is but partly to shew you the difficulty of such attempts, which since our author's essay was communicated, have been represented (I fear by conjecture only) as very easy to be accurately enough done; and partly because our author does not without reason intimate the usefulness of redintegrations, in case they can be effected; and does not causelessly intimate that such attempts, though they should not perfectly succeed, may increase the number of noble and active bodies, and consequently the inventory of mankind's good.

Upon such considerations we attempted the dissipation and re-union of the parts of common amber; and though chymists for fear of breaking their vessels, are wont when they commit it to distillation, to add to it a *caput mortuum* (as they speak) of sand, brick, &c. (in whose room we sometimes chuse to substitute beaten glass;) which hinders them to judge of and employ the remanence of amber after the distillation is finished; yet we supposed and found that if the retort were not too much filled, and if the fire were slowly and warily enough administered, the addition of any other body would be needless. Wherefore having put into a glass retort four or five ounces of amber, and administered a gentle and gradual heat, we observed the amber to melt and bubble (which we therefore mention because ingenious men have lately questioned whether it can be melted), and having ended the operation and severed the vessels, we found that there was come over in the form partly of oil, partly of spirit and phlegm, and partly of volatile salt, near half the weight of the concrete: and having broken the retort, we found in the bottom of it a cake of coal-black matter, than whose upper surface I scarce remember to have seen in my whole life any thing more exquisitely polished; inasmuch that, notwithstanding the colour, as long as I kept it, it was fit to serve for a looking-glass: and this smooth mass being broken (for it was exceeding brittle), the large fragments of it appeared adorned with an excellent lustre. All those parts of the amber being put together into a glass body, with a blind head luted to it, were placed in sand to be incorporated by a gentle heat: but whilst I slept aside to receive a visit, the fire having been increased without my knowledge, the fumes ascended so copiously, that they lifted up the vessel out of the sand, whereupon falling against the side of the furnace, it broke at the top, but being seasonably called, we saved all but the fumes; and the remaining matter looks not unlike tar, and with the least heat may be poured out like a liquor, sticking, even when it is cold to the fingers. Yet this opening body doth not easily communicate so much as a tincture to spirit of wine, which therefore seems somewhat strange, because another time presuming that this would be a good way to obtain a solution of some of the resinous parts of amber, we did, by pouring spirit of wine, that (though rectified) was not of the very best, upon the re-united parts of amber, lightly digested into a mass, easily obtain a clear yellow solution, very differing from the tincture of amber, and abounding (as I found by trial) in the dissolved substance of the amber: but in oil of turpentine we have in a short time dissolved it into a blood-red balsam, which may be of good use (at least) to chirurgeons. And having again made the former experiment with more wariness than before, we had the like success in our distillation, but the re-united parts of the amber being set to digest in a large bolt-head, the liquor, that was drawn off, did in a few hours, from its own *caput mortuum*, extract a blood-red tincture, or else made a solution of some part of it, whereby it obtained a very deep red; but having been by intervening accidents, hindered from finishing the experiment, we missed the satisfaction of knowing to what it may be brought at last.

AND



AND as for what our author tells us of this design to attempt the redintegration of vitriol, turpentine, and some other concretes, wherein it seemed not unpracticable, he found in it more difficulty than every one would expect. For the bodies on which such experiments are likeliest to succeed, seem to be allum, sea-salt, and vitriol. And as for allum, he found it a troublesome work to take (as a Spagyrist would speak) the principles of it asunder, in regard that it is inconvenient to distil it with a *caput mortuum*, (as chymists call any fixed additament) lest that should hinder the desired redintegration of the dissipated parts. And when he distilled it by itself without any such additament, he found that with a moderate heat the allum would scarce part with any thing but its phlegm; and if he urged it with a strong fire, he found it would so swell, as to endanger the breaking of the retort, or threaten the boiling over into the receiver. (Yet having once been able very warily to abstract as much phlegm and spirit as I conveniently could, from a parcel of roch allum, and having poured it back upon that pulverized *caput mortuum*, and left the vessel long in a quiet place, I found that the corpufcles of the liquor having had time after a multitude of occurrences to accommodate and re-unite themselves to the more fixed parts of the concrete, did, by that association (or dissolution) recompose, at the top of the powder, many crystalline grains of finely figured salt, which increasing with time, made me hope that at the length the whole, or the greatest part would be reduced into allum, which yet a mischance that robbed me of the glass, hindered me to see. So likewise of sea-salt, if it be distilled, as it is usual, with thrice its weight of burned clay, or beaten brick, it will prove inconvenient in reference to its redintegration: and if it be distilled alone, it is apt to be fluxed by the heat of the fire, and whilst it remains in fusion will scarce yield any spirit at all. And as for vitriol, though the redintegration of it might seem to be less hopeful than that of the other salts, in regard that it consists not only of a saline, but of a metalline body, whence it may be supposed to be of a more intricate and elaborate texture; yet because there needs no *caput mortuum* in the distillation of it, we did, to pursue our author's intimated designs, make two or three attempts upon it, and seemed to miss of our aim, rather upon the account of accidental hindrances, than of any insuperable difficulty in the thing itself. For once we with a strong fire, drew off from a parcel of common blue vitriol, the phlegm and spirit, and some quantity of the heavy oil, (as chymists abusively call it): these liquors, as they came over without separation, we divided into several parts, and the remaining very red *caput mortuum* into as many. One of these parcels of liquor we poured over night upon its correspondent portion of the newly mentioned red powder: but having left it in a window, and the night proving very bitter, in the morning I found the glass cracked in many places by the violence of the frost, and the liquor seemed to have been soaked up by the powder, and to have very much swelled it. This mixture then I took out, and placing it in an open-mouthed glass in a window, I found after a while, divers grains of pure vitriol upon the other matter, and some little swellings, not unlike those we shall presently have occasion to speak of. I took likewise a much larger parcel of the fore-mentioned liquor, and its correspondent proportion of *caput mortuum*; and having leisurely mixed them in a large glass basin, I obtained divers phenomena, that belong not to this place, but may be met with where they will more properly fall in. In this basin (which I laid in the window, and kept from agitation) I perceived, after a while, the liquor to acquire a bluish tincture, and after ten or twelve weeks I found the mixture dry (for it seems it was too much exposed to the air); but the surface of it adorned in divers places, with grains of vitriol very curiously figured.

And

AND besides these, there were store of protuberances, which consisted of abundance of small vitriolate particles, which seemed in the way to a coalition; for having let the basin alone for four or five months longer, the matter appeared crusted over, partly with very elevated saline protuberances, partly with lesser parcels, and partly also with considerable broad cakes of vitriol, some of above half an inch in breadth, and proportionably long: and indeed the whole surface was so oddly diversified, that I cannot count the trouble these trials have put me to, mispent. Another time, in a more slender and narrow-mouthed glass, I poured back upon the *caput mortuum* of vitriol the liquors I had by violence of the fire forced from it; so that the liquid part did swim a pretty height above the red calx, and remained a while limpid and colourless: but the vessel having stood for some time unstopped in a window, the liquor after a while acquired by degrees a very deep vitriolate colour, and not long after there appeared at the bottom, and on the top of the calx, many fair and exquisitely figured grains of vitriol, which covered the surface of the calx, and the longer the vessel continued in the window, the deeper did this change, made upon the upper part of the powder, seem to penetrate: so that I began to hope that in process of time, almost (if not more than almost) the whole mixture would be reduced to perfect vitriol. But an accident robbed me of my glass before I could see the utmost of the event.

And on this occasion I must not premit an odd experiment I lately made, though I dare not undertake to make it again. I elsewhere relate how I digested for divers weeks a quantity of powdered antimony, with a greater weight by half of oil of vitriol; and how having at length committed this mixture to distillation, and thereby obtained, besides a little liquor, a pretty quantity of combustible antimonial, or antimonio-vitriolate sulphur; there remained in the bottom of the retort a somewhat light and very friable *caput mortuum*, all the upper part of which was at least as white as common wood-ashes, and the rest looked like a cinder. And now I must tell you what became of this *caput mortuum*, whereof I there make no further mention. We could not well foresee what could be made of it, but very probable it was that it would afford us some new discovery by being exposed to the fire, in regard of the copious sulphur whereof it seemed to have been deprived; provided it were urged in close vessels, where nothing could be lost. Whereupon committing it to a naked fire in a small glass retort, well coated and accommodated with a receiver, we kept it there many hours, and at length severing the vessels, we found (which need not be wondered at) no antimonial quicksilver, and much less of sulphur sublimed than we expected: wherefore greedily hastening to the *caput mortuum*, we found it fixed into a mass, covered with a thin cake of glass, whose fragments being held against the light, were not all coloured, as antimonial glass is wont to be, but were as colourless as common white glass. The lump above mentioned being broken, was found, somewhat to our wonder, to be perfect black antimony, adorned with long shining streaks, as common antimony is wont to be: only this antimony seemed to have been a little refined by the sequestration of its unnecessary sulphur; which ingredient seems by this experiment, as well as by some other observations of ours, to be more copious in some particular parcels of that mineral, than is absolutely requisite to the constitution of antimony. Though in our case it may be suspected that the reduction of part of the mass to a colourless glass was an effect of the absence of so much of the sulphur, and might in part make the remaining mass some amends for it. What we further did with this new or reproduced concrete, is not proper to be here told you: only for your satisfaction we have kept a lump of it, that you may with us take notice of what some Philosophers would call the mindfulness of nature; which, when a body was deprived of a not inconsiderable portion of its chief ingredient, and had all its other



parts dissipated and shuffled, and discoloured so as not to be knowable, was able to rally those scattered and disguised parts, and marshal or dispose them into a body of the former consistence, colour, &c. though (which is not here to be overlooked) the texture of antimony, by reason of the copious shining *stiria* that enoble the darker body, be much more elaborate, and therefore more uneasy to be restored than that of many other concretes.

BUT among all my trials about the redintegration of bodies, that which seemed to succeed best was made upon turpentine: for having taken some ounces of this, very pure and good, and put it into a glass retort, I distilled so long with a very gentle fire, till I had separated it into a good quantity of very clear liquor, and a *caput mortuum* very dry and brittle; then breaking the retort, I powdered the *caput mortuum*, which when it was taken out was exceeding sleek, and transparent enough, and very red, but being powdered, appeared of a pure yellow colour. This powder I carefully mixed with the liquor that had been distilled from it, which immediately dissolved part of it into a deep red balsam; but by further digestion, in a large glass exquisitely stopp'd, that colour began to grow fainter, though the remaining part of the powder (except a very little, proportionable to so much of the liquor, as may be supposed to have been wasted by evaporation and transusion out of one vessel into another) be perfectly dissolved, and so well re-united to the more fugitive parts of the concrete, that there is scarce any that by the smell, or taste, or consistence, would take it for other than good and laudable turpentine.



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C O N S I D E R A T I O N S  
A N D  
E X P E R I M E N T S  
TOUCHING THE ORIGIN OF  
QUALITIES and FORMS.  
THE HISTORICAL PART.

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S E C T I O N I.

*Containing the Observations.*

**I**N the foregoing notes I have endeavoured, with as much clearness as the difficulty of the subject and the brevity I was confined to permitted, to give a scheme or summary of the principles of the corpuscularian philosophy, as I apprehended them, by way of a short introduction to it, at least as far as I judged necessary for the better understanding of what is contained in our notes and experiments, concerning the productions and changes of particular qualities. But though I hope I have not so affected brevity as to fall into obscurity, yet, since these principles are built upon the phaenomena of nature, and devised in order to the explication of them, I know not what I can do more proper to recommend them than to subjoin some such natural phaenomena as either induce me to take up such notions, or which I was directed to find out by the notions I had embraced. And since I appeal to the testimony of nature to verify the doctrine I have been proposing, about the origin and production of qualities (for that of forms will require a distinct discourse); I think it very proper to set down some observations of what nature does, without being over-ruled by the power and skill of man, as well as some experiments wherein nature is guided, and as it were, mastered by art, that so she may be made to attest the truth of our doctrine, as well when she discloses herself freely, and, if I may so speak, of her own accord, as when she is, as it were, cited to make her depositions by the industry of man. The observations will be but  
the



the more suitable to our design for being common and familiar as to the phenomena, though perhaps new enough as to the application to our purpose. And as for the experiments, because those that belong more immediately to this or that particular quality, may be met with in the notes that treat of it, I thought it not amiss that the experiments should be both few in number, and yet so pregnant that every one of them should afford such differing phenomena as may make it applicable to more than one quality :

I.

THE observation I shall begin with shall be fetched from what happens in the hatching of an egg. For as familiar and obvious a thing as it is, (especially after what the learned *Fabricius ab Aquapendente*, and a recenter anatomist, have delivered about them) that there is a great change made in the substance of the egg when it is by incubation turned into a chick ; yet, as far as I know, this change hath not been taken notice of for the same purpose to which I am about to apply it.

I CONSIDER then that in a prolific egg (for instance that of a hen) as well the liquor of the yolk as that of the white is a substance, as to sense, similar. For upon the same account that anatomists and physicians call several parts of the human body, as bones, membranes, &c. similar, that is such, as that every sensible part of it hath the same nature or denomination with the whole, as every splinter of bone is bone, as every shred of skin is skin.

AND though I find by distilling the yolks and whites they seem to be dissimilar bodies, in regard that the white of an egg (for example) will afford substances of a very differing nature, as phlegm, salt, oil, and earth ; yet (not now to examine whether, or how far these may be esteemed productions of the fire that are rather obtained from the white of the egg, than were præ-existent in it ; not to mention this, I say) it doth not appear by distillation that the white of an egg is other than a similar body in the sense above delivered. For it would be hard to prove that one part of the white of an egg will not be made to yield the same differing substances by distillation that any other part does ; and bones themselves, and other hard parts of a human body that are confessedly similar may, by distillation, be made to afford salt, and phlegm, and spirit, and oil, and earth, as well as the white of an egg.

THIS being thus settled in the first place, we may in the next consider that by beating the white of an egg well with a whisk, you may reduce it from a somewhat tenacious, into a fluid body, though this production of a liquor be, as we elsewhere noted, effected by a divulsion, agitation, &c. of the parts ; that is, in a word, by a mechanical change of the texture of the body.

IN the third place I consider that, according to the exactest observations of modern anatomists, which our own observations do not contradict, the rudiments of the chick, lodged in the cicatrícula or white speck upon the coat of the yolk, is nourished till it have obtained to be a great chick, only by the white of the egg ; the yolk being by the providence of nature reserved as a more strong and solid aliment till the chick have absorbed the white, and be thereby grown great and strong enough to digest the yolk ; and in effect you may see the chick furnished not only with all the necessary, but divers other parts, as head, wings, legs, and beak, and claws, whilst the yolk seems yet, as it were, untouched. But whether this observation about the entrenches of the yolk be presently true, is not much material to our present purpose, nor would I be thought to build much upon it ; since the yolk itself, especially at that time, is wont to be fluid

enough, and to be a liquor perhaps no less so than the white was, and that is enough for my present purpose.

For in the last place I consider that the nutritive liquor of an egg, which is in itself a body so very soft, that by a little agitation it may be made fluid, and is readily enough dissolvable in common cold water; this very substance, I say, being brooded on by the hen, will within two or three weeks be transmuted into a chick furnished with organical parts, as eyes, ears, wings, legs, &c. of a very differing fabrick, and with a good number of similar ones, as bones, cartilages, ligaments, tendons, membranes, &c. which differ very much in texture from one another; besides the liquors, as blood, chyle, gall, &c. contained in the solid parts: so that here we have out of the white of an egg, which is a substance similar, insipid, soft (not to call it fluid) diaphanous, colourless, and readily dissoluble in cold water; out of this substance, I say, we have, by the new and various contrivement of the small parts it consisted of, an animal, some of whose parts are not transparent, but opacous; some of them red, as the blood; some yellow or greenish, as the gall; some white, as the brain; some fluid, as blood, and other juices; some consistent, as the bones, flesh, and other stable parts of the body; some solid and frangible, as the bones; others tough and flexible, as the ligaments; others soft and loosely coherent, as the marrow; some without springs, as many of the parts; some with springs, as the feathers; some apt to mingle readily with cold water, as the blood, the gall; some not to be so dissolved in it, as the bones, the claws, and the feathers; some well tasted, as the flesh and blood; some very ill tasted, as the gall (for that I have purposely and particularly observed); in a word, we have here produced out of such an uniform matter as the white of an egg,

FIRST, new kind of qualities, as (besides opacity) colours (whereof a single feather will sometimes afford us variety) odours, tastes, and heat in the heart and blood of the chick, hardness, smoothness, roughness, &c.

SECONDLY, divers other qualities that are wont to be distinguished from sensible ones, as fluidity (in the blood and aqueous humour of the eye) consistency in the gristles, flesh, &c. hardness, flexibility, springiness, toughness, unfitness to be dissolved in cold water, and several others. To which may probably be added,

THIRDLY, some occult properties, as physicians observe, that some birds, as young swallows, young magpies, afford specificck, or at least noble medicines in the falling-sickness, hysterical fits, and divers other distempers.

FOURTHLY, I very well foresee it may be objected that the chick with all its parts is not a mechanically contrived engine, but fashioned out of matter by the soul of the bird lodged chiefly in the cicatrix, which by its plastick power fashions the obsequious matter and becomes the architect of its own mansion. But not here to examine whether any animal except man be other than a curious engine, I answer, that this objection invalidates not what I intend to prove from the alledged example. For let the plastick principle be what it will, yet still, being a physical agent, it must act after a physical manner; and having no other matter to work upon but the white of the egg, it can work upon that matter but as physical agents, and consequently can but divide the matter into minute parts of several sizes and shapes, and by local motion variously context them according to the exigency of the animal to be produced, though from so many various textures of the produced parts there must naturally emerge such differences, of colours, tastes, and consistences, and other qualities as we have been taking notice of. That which we are here to consider is not what is the agent, or efficient in these productions, but what is done to the matter to effect them. And though some birds, by an inbred skill, do very artificially build their curious nests, yet cannot nature teach



teaches them, enable them to do any more than select the materials of their nests, and by local motion divide, transport, and connect them after a certain manner. And when man himself, who is undoubtedly an intelligent agent, is to frame a building or an engine, he may indeed, by the help of reason and art, contrive his materials curiously and skilfully; but still all he can do is but to move, divide, transpose, and context the several parts into which he is able to reduce the matter assigned him.

Nor need we imagine that the soul of that hen which having first produced the egg, does after a while sit on it, hath any particular efficiency in hatching of a chick, for the egg will be well hatched by another hen though that which laid it be dead; and which is more, we are assured by the testimony of very good authors as well as of recent travellers, that in some places, especially in *Egypt*, there needs no bird at all to the production of a chick out of an egg, since they hatch multitudes of eggs by the regulated heat of ovens or dunghills: and indeed, that there is a motion or agitation of the parts of the egg by the external heat whereby it is hatched, is evident of itself, and not (as far as I know) denied by any; and that also the white substance is assumed, and contexted or contrived into the body of the chick and its several parts, is manifest to sense; especially if one hath the curiosity to observe the progress of the chick's formation and increment. But as it is evident that these two things, the substance of the white, and the local motion wherein the external heat necessary to incubation puts its parts, do eminently concur to the production of the chick, so that the former power (whatever that be) doth any more than guide these motions, and thereby associate the fitted particles of matter after the manner requisite to constitute a chick, is that which I think will not easily be evinced. And I might, to what I said of the egg, add several things touching the generation of viviparous animals, which the learned *Fabricius ab Aquapendente*, as well as some of the ancient philosophers, would have to be generated from an imperfect kind of eggs: but I take the eggs of birds to be much fitter to instance in, because they are things that we have more at command, and where-with we can conveniently make more trials and observations; and especially because imperfect eggs the matter to be transmuted is more closely locked up, and being kept from any visible supply of matter, confined to be wrought upon by the external heat: and by its own vital principle within.

## II.

**WATER** being generally esteemed an elementary body, and being at least far more homogeneous than bodies here below are wont to be, it may make very much for our present purpose to shew that water itself, that is fluid, tasteless, inodorous, diaphanous, colourless, volatile, &c. may, by a differing texture of its parts, be brought to constitute bodies of attributes very distant from these. This I thought might be done by nourishing vegetables with simple water. For in case I could do so, all or the greatest part of that which would accrue to the vegetable thus nourished, would appear to have been materially but water with what exotic quality soever it may afterwards, when transmuted, be endowed.

The ingenious *Helmont*, indeed, mentions an experiment somewhat of this nature, though not to the same purpose, which he made by planting a branch of willow into a pot full of earth, and observing the increase of weight he obtained after divers years, though he fed the plant but with rain-water. And some learned modern naturalists have conjectured at the easy transmutableness of water by what happens in gardens and orchards, where the same showers or rain, after a long drought, makes a great number of differing plants to flourish. But though these things be worthy of their authors, yet

I thought



I thought they would not be so fit for my purpose, because it may be speciously enough objected that the rain-water does not make these plants thrive and flourish, by immediately affording them the aliments they assimilate into their own substance, but by proving a vehicle that dissolves the saline and other alimental substances of the earth, and dilutes both them and the nutritive juice which in a part of the plant itself it may find too much thickened by the drought or heat of the ambient air; and by this means it contributes to the nourishment of the plant, though itself afterwards exhaled into vapours. And indeed experience shews us, that several plants which thrive not well without rain-water, are not yet nourished by it alone; since when corn in the field and fruit-trees in orchards have consumed the saline and sulphureous juices of the earth, they will not prosper there, how much rain soever falls upon the land, till the ground, by dung or otherwise, be supplied again with such assimilable juices. Wherefore I rather chose to attempt the making of plants grow in phials filled with water, not only to prevent the fore-mentioned objection, and also to make the experiment less tedious, but that I might have the pleasure of seeing the progress of nature in the transmutation of water; and my observations of this kind, as novelties unmentioned by any other writer, I shewed divers ingenious friends, who having better opportunities than I of staying in one place, have attempted the like and made successful trials, which I suppose will not be concealed from the publick. Of my observations about things of this kind I can at present find but few among my *Adversaria*, but in them I find enough for my present turn. For they and my memory inform me that vinca pervinca, raphanus aquaticus, spearmint, and even ranunculus itself, did grow and prosper very well in phials filled with fair water, by whose necks the leaves were supported, and the plant kept from sinking; some of these were only cuttings without roots, divers of them were left in the water all the autumn and great part of the winter, and at the end of *January* were taken out verdant and with fair roots, which they had shot in the water. And besides, I find that particularly a branch or sprig of raphanus aquaticus was kept full nine months, and during that time withered not the whole winter, and was taken out of the water with many fibrous roots, and some green buds, and an increase of weight; and that a stump of ranunculus did so prosper in the water, that in a month's time it had attained to a pretty deal more than double the weight it had when it was put in. And the next note which I find concerning these plants informs me, that the above-mentioned crow's-foot, being taken out again at six months after it was put in, weighed a drachm and a half wanting a grain and a half, that is, somewhat above thrice as much as it did at first. This last circumstance (of the increase of weight) I therefore thought fit particularly to make trial of, and set down upon this account among others, that having doubted the roots and leaves that seemed produced out of the water, might really be so by an oblongation and an expansion of the plants (as I have purposely tried, that an onion weighed and laid up in the spring, though after some weeks keeping in the air it shot blades, whereof one was five inches long, instead of incorporating the air or terrestrial effluvia with itself, and consequently thereby growing heavier, had lost nine grains of its former weight); it might by this circumstance appear that there may be a real assimilation and transmutation of water into the substance of the vegetable, as I elsewhere also shew by other proofs. For this being made out, from thence I infer that the same corpuscles which, convening together after one manner, compose that fluid, inodorous, colourless, and insipid body of water, being contexted after other manners, may constitute differing concretes which may have firmness, opacity, odours, smells, tastes, colours, and several other manifest qualities, and that too very different from one another. And besides all this, these distinct portions of transmuted water may have many other



qualities, without excepting those that are wont to be called specifick or occult; witness the several medicinal virtues attributed by authors to spearmint, and to periwinkle, to majorane, and to raphanus aquaticus. And as for ranunculus, that plant being reckoned among poisonous ones, and among those that raise blisters, it will be easily granted that it hath, as other poisons, an occult, deleterial faculty: and indeed it somewhat deserves our wonder that so insipid and innocent a thing as fair water should be capable of being turned into a substance of such a piercing and caustic nature, as by contact to raise blisters on a human body. And yet perhaps that is no less strange which we elsewhere relate, that a plant, consisting chiefly of transmuted water, did by distillation afford us a true oil that would not mingle with water, and consequently was easily convertible into fire. But whether or no this experiment, or any such like, prove that almost all things may be made of all things, not immediately, but by intervention of successive changes and dispositions, is a question to which we elsewhere say something, but are not willing in this place to say any thing. And if it be here objected that the solid substance that accrues to a plant rooted in water proceeds not at all from the water itself, but from the nitrous, fat, and earthy substances that may be presumed to abound even in common water; not here to repeat what I elsewhere say about this objection, I shall at present reply, that though, as to divers plants that flourish after rain, I am apt to think, as I intimated above, that they may in part be nourished as well by the saline and earthy substances, to which the rain usually proves a vehicle, as by the rain itself; yet as to what the objection holds forth about the plants that grow not in the ground, but in glasses filled with water, it should not be barely said, but proved; which he will not perhaps think easy to be done that considers how vast a quantity of fair water is requisite to be exhaled away to obtain as much as one ounce of dry residue, whether saline or earthy.

## III.

THAT a plant growing in the earth doth by the faculties of its vegetative soul attract the juices of the earth that are within its reach, and selecting those parts that are congruous to its nature, refuse the rest, is the general opinion of philosophers and physicians; and therefore many naturalists are not wont much to marvel when they see a tree bear a fruit that is sour or bitter, because they presume that nature hath in the root of the tree culled out such parts of the alimental juice of the earth as being made to convene into one fruit, are fit to make it of such a quality. But it is worth observing, for our present purpose, what happens both in ordinary graftings, and especially in that kind of incision (taking the word in a large sense) which is commonly called inoculation. For though we may presume that the root of a white thorn (for instance) may electively attract its aliment from the earth and choose that which is fittest to produce the ignoble fruit that is proper for that plant; yet we cannot reasonably suppose that it should in its attraction of aliment have any design of providing an appropriate nutriment for a pear: and yet the known experience of gardeners, and our own observations, manifest that the cyons of a pear-tree will take very well upon a white-thorn stock, and bring forth a well-tasted fruit very differing in many qualities from that of the white-thorn. I have also learned from those that are expert, that though apples and pears, being but vulgar fruit, are seldom propagated but by grafting, yet they may be propagated likewise by inoculation (which seems to be but a kind of grafting with a bud). Now in the inoculations that are made upon fruit-trees it is very observable, and may much countenance what we are endeavouring to prove, that a little vegetable bud (that is no seed properly, so called) not so big oftentimes as a pea, should be able so to transmute



ture all the sap that arrives at it, that though this sap be already in the root, and in its passage upwards determined by nature's intention, as men are wont to speak, to the production of the fruit that is natural to the stock, yet this sap should by so small a vegetable substance as a bud (whether by the help of some peculiar kind of strainer, or by the operation of some powerful ferment lodged in it, or by both these, or some other cause) be so far changed and over-ruled as to constitute a fruit quite otherwise qualified than that which is the genuine production of the tree, and which is actually produced by those other portions of the like sap which happened to nourish the prolific buds that are the genuine offspring of the stock; so that the same sap that in one part of a branch constitutes (for instance) a cluster of haws, in another part of the same branch may constitute a pear. And that which is further remarkable to our present purpose, is, that not only the fruits made of the same sap do often differ from one another in shape, bigness, colour, odour, taste, and other obvious qualities as well as occult ones; but that though the sap itself be (oftentimes) a waterish and almost insipid liquor, that appears to sense homogeneous enough, and even by distillation affords very little besides phlegm; yet this sap is not only convertible by buds of several natures into differing fruits, but in one and the same fruit the transmuted sap shall, by differing textures, be made to exhibit very differing, and sometimes contrary qualities. As when (for instance) a peach-bud does not only change the sap that comes to it into a fruit very differing from that which the stock naturally produceth, but in the skin of the peach it must be red, in the kernel white, and in other parts, of other colours; the flesh of it must be fragrant, the stone inodorous, the flesh soft and yielding, the stone very hard and brittle, the meat pleasantly tasted, the kernel bitter; not to mention that peach-blossoms, though produced also by the bud, are of a colour and texture very differing from that of the fruit, and are ennobled with an occult quality which the fruit hath not, I mean a purgative virtue: so that from inoculations we may learn that a phlegmatic liquor that seems homogeneous enough, and but very slenderly provided with other manifest qualities than common water, may, by being variously contexted by the buds of trees, be transmuted into bodies endowed with new and various considerable scents, colours, tastes, solidity, medicinal virtues, and divers other qualities manifest and occult.

If it be here said that these qualities are the productions of the plastic power residing in prolific buds, which indeed (to me) seem to be but very minute boughs; I shall return the same answer that I did to the like objection when it was proposed in the first observation.

HITHERTO I have only argued from vulgar inoculations, but there may be others, as well more considerable, as less ordinary: and I remember I have seen a tree, whereof, though the stock was of one sort of good fruit, there were three more and differing kinds of stone-fruit that had been made to take by inoculation, and two of those inoculated boughs had actually fruit on them; and the third, though it had as yet no fruit, because the season for that sort of plants to bear was not yet come, yet the shoot was so flourishing, that we concluded that the blossoms would in due time be succeeded by fruit. And since I have been speaking of the differing qualities of the parts of the same fruit, I am content to add two things: the one that *Garcias ab Horto*, a classic author (and physician to the *Indian* viceroy) affirms\* with some solemnity (as wondering that a learned man should write otherwise) that though the fruit we call *castia fistula* be very commonly used both here and in the *Indies* as a purging medicine, yet the seeds

\* *Aromar. Hist. lib. 1. cap. 29. de Castia solutiwa.*



of this solutive cassia are astringent. The other, that of late years there have been often brought into *England* from the *Caribbee* islands certain kernels of a fruit, which those that have seen it grow, liken to a white pear-plumb; these are so strongly purgative and also emetic, that the ingenious Mr. *Lygon* \* tells us that five of them wrought with him a dozen times upwards, and above twenty downwards; and yet the same author assures us (which is likewise here a received tradition among them that are curious of this fruit) that in the kernel, in the parting of it into halves (as when our hazle-nuts in *England* part in the middle longways) you shall find a thin film, which looks of a faint carnation (which colour is easily enough discerned, the rest of the kernel being perfectly white) and that, taking out the film, you may eat the nut safely without feeling any operation at all, and it is as sweet as a Jordan almond. [A learned man that practised physic in *America*, being inquired of by me concerning the truth of this relation, answered, that though he had divers times given those nuts as cathartic remedies, yet he had not that curiosity to take out the films, finding it the universal belief that the purgative faculty consisted therein]. And I remember that the famous † *Monardes* doth somewhat countenance this tradition, where, speaking of another purging fruit that also comes from *America* (from *Cartagena* and *Nombre de Dios*) he takes notice that these purging beans (which are like ours, but smaller) have a thin skin that divides them through the middle, which must (together with the external rind) be cast away, else they will work so violently, both upwards and downwards, as to bring the taker into hazard of his life: whereas he commends these beans, rightly prepared, not only as a pleasant medicine that doth, without trouble, purge both choler, phlegm, and gross humours; for which it is celebrated among the *Indians*.

To these stories of our countrymen and *Monardes*, I shall subjoin another which I find related by that great Rambler about the world, *Vincent le Blanc*, who giving us an account of a publick garden which he visited in *Africa*, in the territories of the Lord of *Casima*, not far from the borders of *Nubia*, which he represents as the curiouslest garden he saw in all the East, he mentions this among other rarities: 'There were (says he) other sorts of fruit which I never saw but there, and one among the rest, leaved like a sycamore, with fruit like the golden apple, but no gall more bitter, and within five kernels as big as almonds, the juice whereof is sweet as sugar; betwixt the shell and the nut there grows a thick skin of a carnation colour, which, taken before they be thoroughly ripe, they preserve with date vinegar, and make an excellent sweetmeat, which they present to the king as a great curiosity.'

## IV.

THE fourth and last observation I shall at present mention is afforded me by the consideration of rotten cheese. For if we take notice of the difference betwixt two parts of the same cheese, whereof the one continues sound by preserving its texture, and the other hath suffered that impairing alteration of texture we call rottenness; we may often see a manifest and notable change in the several portions of a body that was before similar: for the rotten part will differ from the sound in its colour, which will be sometimes livid, but most commonly betwixt green and blue; and its odour, which will be both strong and offensive; and its taste, which will be very piquant, and to some men much more pleasant than before, but to most men odious; and in divers other qualities, as

\* *Lygon's History of Barbados*, pag. 67, 68.

† See *Nicholaus Monardes*, under the Title, *Fabæ purgatrices*.

particularly its consistence, it will be much less solid and more friable than before; and if with a good microscope we look upon the moulded parts of many cheefes, we shall quickly discover therein some swarms of little animals (the mites) furnished with variety of parts of differing sizes, shapes, textures, &c. and defery a yet greater diversity, both as to manifest qualities (nor probably is it inferior as to occult ones) betwixt the mouldy part of the cheefe and the untainted, than the unassisted eye could otherwise have discovered.

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*Advertisements about the ensuing Section II.*

THE author would not have the reader think that the following experiments are the sole ones, that he could have set down to the same purpose with them. For they are not the only that he had actually laid aside for this occasion, till judging the ensuing ones sufficient for his present scope, he thought it fitter to reserve others for those notes about the production of particular qualities, to which they seemed properly to belong. Perhaps also it will be requisite for me (because some readers may think the omission a little strange) to excuse my having left divers particulars unmentioned in more than one of the ensuing experiments. And I confess that I might easily enough both have taken notice of more circumstances in them, and made far more reflexions on them, if I would have expatiated on the several experiments according to the directions delivered in other \* papers. But though there, where it was my design to give employment to the curiosity and diligence of as many votaries to nature, as (for want of better instructions) had a mind to be so set on work, it was fit the proposed method should be suitable; yet here, where I deliver experiments not so much as part of natural history, as instances to confirm the hypotheses and discourses they are annexed to, it seemed needless and improper (if not impertinent) to set down circumstances, cautions, inferences, hints, applications, and other particulars that had no tendency to the scope for which the experiments were alledged.

And as for the kind of experiments here made choice of, I have the less scrupled to pitch upon chymical experiments rather than others on this occasion; not only because of those advantages which I have ascribed to such experiments in the latter part of the † preface to my *Specimens*, but because I have been encouraged by the success of the attempt made in those discourses. For as new as it was, when I made it four or five years ago; and as unusual a thing as it could seem to divers Atomists and Cartesians, that I should take upon me to confirm and illustrate the notions of the Particularian philosophy (if I may so call it) by the help of an art, which many were pleased to think cultivated but by illiterate operators or whimsical fanatics in philosophy, and useful only to make medicines or disguise metals: yet these endeavours of ours met with much less opposition than new attempts are most commonly fain to struggle with. And in so short a time I have had the happiness to engage both divers chymists to learn and relish the notions of the corpuscular philosophy, and divers eminent embracers of that to en-

\* Containing some advices and directions for the writing of an experimental natural history.

† The preface here mentioned is that prefixed to the tract intitled, — *Some Specimens of an attempt to make Chymical Experiments useful to illustrate the notions of the Corpuscular Philosophy.*



deavour to illustrate and promote the new philosophy, by addiſting themſelves to the experiments, and peruſing the books of chymiſts. And I acknowledge it is not unwelcome to me to have been (in ſome little meaſure) inſtrumental to make the corpuscularian philoſophy, aſſiſted by chymiſtry, preferred to that which has ſo long obtained in the ſchools. For (not here to conſider, which I elſewhere do, how great an advantage that philoſophy hath of this, by having an advantage of it in point of clearneſs) though divers learned and worthy men that knew no better principles, have, in cultivating the Peripatetick ones, abundantly exerciſed and diſplayed their own wit; yet I fear they have very little, if at all, improved their reader's intellect, or enriched it with any true or uſeful knowledge of nature; but have rather taught him to admire their ſubtlety, than underſtand hers. For to aſcribe all particular phaenomena that ſeem any thing difficult (for abundance are not thought ſo that are ſo) to ſubſtantial forms, and but nominally underſtood qualities, is ſo general and eaſy a way of reſolving difficulties, that it allows naturaliſts, without diſparagement, to be very careleſs and lazy, if it do not make them ſo; as in effect we may ſee, that in about two thouſand years ſince *Ariſtotle's* time the adorers of his phyſicks, at leaſt by virtue of his peculiar principles, ſeem to have done little more than wrangle, without clearing up (that I know of) any mystery of nature, or producing any uſeful or noble experiment: whereas the cultivators of the Particularian philoſophy, being obliged by the nature of their hypotheſis and their way of reaſoning, to give the particular accounts and explications of particular phaenomena of nature, are alſo obliged, not only to know the general laws and courſe of nature, but to inquire into the particular ſtructure of the bodies they are converſant with as that wherein, for the moſt part, their power of acting and diſpoſition to be acted on does depend. And in order to this, ſuch inquirers muſt take notice of abundance of minute circumſtances; and to avoid miſtaking the cauſes of ſome of them, muſt often make and vary experiments; by which means nature comes to be much more diligently and induſtriouſly ſtudied, and innumerable particulars are diſcovered and obſerved which in the lazy Ariſtotelian way of philoſophizing would not be heeded. But to return to that decade of inſtances to which theſe adverſements are premixed; I hope I need not make an apology for making choice rather of chymical experiments than others, in the ſecond and concluding ſection of the hiſtorical part of the preſent treatiſe. But though I prefer that kind of inſtances, yet I would not be thought to overvalue them in their kind, or to deny that ſome artiſts may (for aught I know) be found, to whoſe chymical arcana theſe experiments may be little better than trifles. Nor perhaps are theſe the conſiderableſt that I myſelf could eaſily have communicated (though theſe themſelves would not be now divulged, if I would have been ruled by the diſſuaſions of ſuch as would have nothing of chymical made common which they think conſiderable). But things of greater value in themſelves, and of noble uſe in phyſick, may be leſs fit for our preſent purpoſe (which is not to impart medicinal or alchymical proceſſes, but illuſtrate philoſophical notions) than ſuch experiments as theſe; which, beſides that they contain variety of phaenomena, do not (for the moſt part) require either much time, or much charge, or much ſkill.

## SECTION II.

## Containing the Experiments.

## EXPERIMENT I.

**T**AKE good and clear oil of vitriol, and cast into it a convenient quantity of good camphire grossly beaten; let it float there a while, and, without the help of external heat, it will insensibly be resolved into a liquor, which, from time to time, as it comes to be produced, you may, by shaking the glass, mingle with the oil of vitriol; whereunto you may by this means impart first a fine yellow and then a colour which though it be not a true red will be of kin to it, and so very deep as to make the very mixture almost quite opacous. When all the camphire is perfectly dissolved by incorporating with the menstruum, if you hit upon good ingredients, and upon a right proportion (for a slight mistake in either of them may make this part of the experiment miscarry) you may probably obtain such a mixture as I have more than once had; namely such a one, as not only to me, whose sense of smelling is none of the dullest, but also to others, that knew not of the experiment, seemed not at all to have an odour of the camphire. But if into this liquor you pour a due quantity of fair water, you will see (perhaps not without delight) that, in a trice the liquor will become pale almost as at the first; and the camphire that lay concealed in the pores of the menstruum, will immediately disclose itself, and immerse in its own nature and pristine form of white floating and combustible camphire, which will fill not the phial only, but the neighbouring part of the air with its strong and diffusive odour.

Now the phenomena of this experiment may, besides the uses we elsewhere make of it, afford us several particulars pertinent to our present purpose.

I. For (first) we see a lighter and consistent body brought by a comminution into particles of a certain figure, to be kept swimming and mixed with a liquor on which it floated before, and which is by great odds heavier than itself: so that as by the solution of gold in *aqua regis* it appears that the ponderouflest of bodies, if it be reduced to parts minute enough, may be kept from sinking in a liquor much lighter than itself; so this experiment of ours manifests, what I know not whether hitherto men have proved, that the corpuscles of lighter bodies may be kept from emerging to the top of a much heavier liquor. Which instance being added to that of the gold, may teach us that when bodies are reduced to very minute parts, we must as well consider their particular texture, as the received rules of the hydrostaticks, in determining whether they will sink, or float, or swim.

II. This experiment also shews that several colours, and even a very deep one, may soon be produced by a white body and a clear liquor, and that without the intervention of fire or any external heat.

III. And that yet this colour may almost in the twinkling of an eye be destroyed, and, as it were, annihilated; and the latent whiteness, as many would call it, may be as suddenly restored by the addition of nothing but fair water, which has no colour of its own; upon whose account it might be furnished to be contrary to the prevailing colour,



lour, or to heighten the other into a predominancy; nor does the water take into itself either the colour it destroyed, or that it restores. For

IV. *THE* more than semi-opacity of the solution of camphire and oil of vitriol does presently vanish; and that menstruum with the water make up (as soon as the camphorate corpuscles come to be afloat) one transparent and colourless liquor.

V. *AND* it is worth noting, that upon the mixture of a liquor which makes the fluid much lighter (for so water is in respect of vitriol) a body is made to emerge that did not so when the fluid was much heavier. This experiment may serve to countenance what we elsewhere argue against the schools touching the controversy about mistion. For whereas though some of them dissent, yet most of them maintain that the elements always lose their forms in the mixed bodies they constitute; and though if they had dexterously proposed their opinion and limited their assertions to some cases, perhaps the doctrine might be tolerated; yet since they are wont to propose it crudely and universally, I cannot but take notice how little it is favoured by this experiment; wherein even a mixed body (for such is camphire) doth in a further mistion retain its form and nature, and may be immediately so divorced from the body to which it was united, as to turn in a trice to the manifest exercise of its former qualities. And this experiment being the easiest instance I have devised of the preservation of a body when it seems to be destroyed, and of the recovery of a body to its former conditions, I desire it may be taken notice of, as an instance I shall after have occasion to have recourse to and make use of.

VI. *BUT* the notablest thing in the experiment is, that odours should depend so much upon texture; that one of the subtlest and strongest-scented drugs, that the East itself, or indeed the world afford us, should so soon quite lose its odour by being mixed with a body that has scarce, if at all, any sensible odour of its own; and this while the camphorate corpuscles survive undestroyed in a liquor, from whence one would think that less subtle and fugitive bodies than they should easily exhale.

VII. *NOR* is it much less considerable that so strong and piercing a scent as that of camphire should be in a moment produced in a mixture wherein none of it could be perceived before, by such a liquor as water, that is quite devoid of any odour of its own; which so easy and sudden restauration of the camphire to its native scent, as well as other qualities, by so languid a liquor as common water, doth likewise argue that the union or texture of the two ingredients, the camphire and the oil of vitriol, was but very slight, upon which, nevertheless, a great alteration in point of qualities depended. And to confirm, that divers of the preceding phenomena depend upon the particular texture of the liquors employed to exhibit them; I shall add, that if instead of oil of vitriol you cast the concrete into well-dephlegmed spirit of nitre, you will obtain no red nor dark, but a transparent and colourless solution. And when to the above mentioned red mixture I put, instead of fair water, about two or three parts of duly rectified spirit of wine, there would ensue no such changes as those formerly recited; but the spirit of wine that dissolved the concrete, when it was by itself, without losing its diaphaneity or acquiring any colour, did, when it dissolved the mixture, dissolve it with its new adventitious colour, looking like a gross red wine somewhat turbid, or not yet well freed from its lees; so that this colour appeared to reside in the mixture as such, since neither of the two ingredients dissolved in or mingled with the spirit of wine would have afforded that colour, or indeed any other. But if to this liquor that looked like troubled wine we poured a large proportion of fair water, the redness would immediately vanish, and the whole would, as to sense, become white throughout: I say as to sense, because the whiteness did not indeed appertain properly to the whole mixture, but to a huge multitude

tude of little corpuscles of the revived concrete, whereof some or other, which at first swam confusedly to and fro, left no sensible portion of the liquor unfurnished with some of them; whereas when the camphirate corpuscles had leisure to emerge, as they soon did, they floated in the form of a white powder or froth at the top of the liquor, leaving all the rest as clear and colourless as the common water.

BUT we have not yet mentioned all the use we designed to make of our mixture; for by prosecuting the experiment a little further, we made it afford us some new phenomena.

VIII. For having kept the mixture in a moderately warm place (which circumstance had perhaps no influence on the success) and having distilled it out of a glass retort, the event answered our expectation, and the liquor that came over had a scent, which, though very strong, was quite differing both from that of the mixture, and that of the camphire; and in the remaining body, though the liquor and the camphire it consisted of were either both transparent, or the one transparent as a liquor, and the other white, as transparent colourless bodies are wont to be made by confusion; yet the remaining mass, which amounted to a good part of the mixture, was not only opacous, but as black as coal, in some places looking just like polished jet; which is the more considerable, because, that though vegetable substances, that are not fluid, are wont to acquire a blackness from the fire, yet neither do liquors, that have already been distilled, obtain that colour upon redistillation; neither have we, upon trial purposely made, found that camphire, exposed to fire in a retort, fired with a receiver (which was the case of the present experiment) would at all acquire a jetty colour, but would either totally ascend white, or afford *floræ* and a *caput mortuum* (as a vulgar chymist would call the remains) of the same colour, both in respect of one another, and in respect of the camphire.

IX. AND our experiment afforded this notable phenomenon, that though oil of vitriol be a distilled liquor, and though camphire be so very fugitive a substance, that being left in the air it will of itself fly all away, and therefore physicians and druggists prescribe the keeping it in linceds or millium, or other convenient bodies, to hinder its avolation; yet, by our experiment, its fugacity is so restrained, that not only the *caput mortuum*, newly mentioned, endured a good fire in the retort, before it was reduced to that pitchy substance we were lately mentioning; but having taken some of that substance out of the retort, and ordered it by a careful workman to be kept in a closely covered crucible during some time in the fire, when it was brought me back, after the pot had been kept red-hot above half an hour, there remained a good quantity of the matter, brittle, without any smell of camphire, and as black as ordinary charcoal: so much do the fixidity and volatility of bodies depend upon texture.

## EXPERIMENT II.

Among those experiments of mine, *Pyrophilus*, which tend to manifest, that new qualities may be produced in bodies, as the effects of new textures; I remember, some years ago, I writ for a friend a whole set of trials that I had made about the changes I could produce in metals and minerals by the intervention of sublimate. But though the whole tract wherein they are recited might be pertinent enough to our present subject, yet reserving other passages of it for other places (especially for our notes upon those particular qualities, which they are most proper to illustrate) it may at this time suffice me to send you a transcript of what that account contains relating to copper and silver, the one a mean and fugitive, and the other a noble and fixed metal. For those changes in



colour, consistence, fusibleness, and other qualities, which you will meet with in these experiments, will afford us divers phænomena, to shew what great changes may be made, even in bodies scarce corruptible, by one or more of those three catholic ways of nature's working according to the corpuscular principles; namely, the access, the recess, and the transposition of the minute particles of matter.

As for my method of changing the texture of copper, I confess it hath oftentimes seemed strange to me, that chymists, plainly seeing the notable effect that sublimate distilled from antimony has upon that mineral, by opening it and volatilizing it (as we see it do in the making of what they are pleased to call *Mercurius vita*) should not have the curiosity to try, whether or no sublimate might not likewise produce, if not the same, yet a considerable change in other mineral bodies; there appearing no reason, or at least there having been none given, that I know of, why the referatating operation (if I may so speak) of sublimate should be confined to antimony. Upon these considerations, we were invited to endeavour to supply the neglect we had observed in chymists of improving the experiment of *Butyrum Antimonii*: and though an indisposition in point of health, which befel us before we had made any great progress in our inquiries, made us so shy of fumes of sublimate and minerals, that we neither did make all our trials so accurately, nor prosecute them so far as we would have done, had we been to deal with more innocent materials; yet we suppose it will not be unwelcome to you to receive from us a naked, but faithful narrative of our proceedings; being apt to think that you will therein find inducements to carry on this experiment further than we have done, and to compleat what we have but begun.

FIRST then, we took half a pound of copper plates, of about an inch broad, and the thickness of a grain of wheat (which we after found was too great) and of an arbitrary length; then casting a pound of grossly beaten Venetian sublimate into the bottom of a somewhat deep glass retort, we cast in the copper plates upon it, that the fumes of the sublimate might, in their ascension, be compelled to act upon the incumbent metal; and then placing this retort as deep as we well could in a sand furnace, and adapting to it a small receiver, we administered a gradual fire seven or eight hours, and at length for a while increased the heat, as much as we well could do in such a furnace. The success of this operation was as follows.

1. THERE came little or no liquor at all over into the receiver, but the neck and upper part of the retort were candied on the inside, by reason of the copious sublimate adhering to them, which sublimate weighed about ten ounces: in the retort we found about two ounces and a quarter of running mercury, which had been suffered to revive by the acid salts, which corroding the copper, forsook the quicksilver, whereto they had been in the sublimate united.

2. UPON the increase of the fire there was plainly heard a noise, made by the melting matter in the retort, not unlike that of a boiling pot, or of vitriol, when, being committed to a calcining fire, it is first brought to flow. And this noise we found to be a more constant circumstance of this experiment, than the revivification of part of the mercury contained in the sublimate: for upon another trial made with the former proportion of copper plates and sublimate, we observed, during a very long while, such a noise as hath been already mentioned; but the operation being finished, we scarce found so much as a few grains of running mercury either in the retort or receiver.

3. WE found the metalline lump in the bottom of the retort to have been increased in weight somewhat more than (though not half an ounce above) two ounces; some of the copper plates, lying at the bottom of the mass, retained yet their figure and malleableness, which we ascribe to their not having been thin enough to be sufficiently wrought upon

upon by the sublimate: the others, which were much the greater number, had wholly lost their metalline form, and were melted into a very brittle lump, which I can compare to nothing more fitly, than a lump of good benjamin. For this mass, though ponderous, was no less brittle, and being broken, appeared of divers colours, which seemed to be almost transparent, and in some places it was red, in others of a high and pleasant amber colour, and in other parts of it colours more darkish and mixt might be discerned.

4. But this strange mass being broken into smaller lumps, and laid upon a sheet of white paper in a window, was by the next morning, where-ever the air came at it, all covered with a lovely greenish blue, or rather bluish green, almost like that of the best verdigreese: and the longer it lay in the air, the more of the internal parts of the fragments did pass into the same colour; but the white paper, which in some places they staid, seemed dyed of a green colour inclining unto yellow. And here we had occasion to take notice of the insinuating subtilty of the air; for having put some pieces of this cupreous gum (if I may so call it) into a little box, to shut out the air, which we have found it possible to exclude by other means, we found that notwithstanding our care, those included fragments were, as well as the rest already mentioned, covered with the powder as it were of *viride aeris*.

5. We must not on this occasion omit to tell you, that having the last year made some trials in reference to this experiment, we observed in one of them, that some little copper-plates, from which sublimate had been drawn off, retained their pristine shape and metalline nature, but were whitened over like silver, and continued so for divers months (though we cannot precisely tell you how long, having at length accidentally lost them). And to try whether this whiteness were only superficial, we purposely broke some of these flexible plates, and found that this silver colour had penetrated them throughout, and was more glorious in the very body of the metal than on its surface; which made us suspect that the sublimate by us employed had been adulterated with arsenick (where-with the sophisticators of metals are wont to make blanchers for copper, but not to mention that the malleableness continued, which arsenick is wont to destroy) we discovered not by trial, that the sublimate was other than sincere.

6. In this metalline gum the body of the copper appeared so changed and opened, that we were invited to look upon such a change as no ignoble experiment, considering the difficulty which the best artists tells us there is, and which those that have attempted it have found; I say, not to unlock the sulphur of Venus, but to effect less changes in its texture, than was hereby made. For this gum, cast upon a quick coal and a little blown, will partly melt and flow like rosin, and partly flame and burn like a sulphur, and with a flame so lasting, if it be rekindled as often as it leaves off burning, that we observed it, not without some wonder; and so inflammable is this opened copper, that being held to the flame of a candle or a piece of lighted paper, it would almost in a moment take fire, and send forth a flame like common sulphur, but only that it seemed to us to incline much more to a greenish colour, than the bluer flame of brimstone is wont to do.

To these phaenomena of our experiment, as it was made with copper, my notes enable me to subjoin some others, exhibited, when we made it with sublimate and silver.

THERE were taken of the purest sort of coined silver we could get, half a score of thin plates, on which was cast double the weight of sublimate in a small and strongly-coated retort. This matter being sublimed in a naked fire, we found (having broken the vessel) that the sublimate was almost totally ascended to the top and neck of the retort; in the later of which appeared in many places some revived mercury; in the bottom of the  
retort



retort we found a little fluxed lump of matter, which it was scarce possible to separate from the glass; but having with much ado divorced them, we found this mass to be brittle, of a pale yellowish colour, of near about the weight of the metal on which the sublimate had been cast. And in the thicker part of this lump there appeared, when it was broken, some part of the silver plates, which, though brittle, seemed not to have been perfectly dissolved. This resin of silver did, like that of copper, but more slowly, imbibe the moisture of the air, and within about twenty-four hours was covered with a somewhat greenish dust, concerning which we durst not determine, whether it proceeded from that mixture of copper, which is generally to be met with in coined silver, or from the compounded metal: for the more curious sort of painters do, as they inform us, by corroding coined silver with the fretting steams of saline bodies, or with corrosive bodies themselves, turn it into a fine kind of azure, as we may elsewhere have opportunity more particularly to declare. I shall now only add, that some small fragments of our resin being cast upon red-hot coals, did there waste themselves in a flame, not very differing in colour from that of the former mentioned resin of copper, but much more durable than would have been easily expected from so small a quantity of matter.

THIS is all the account I can give you of our first trial; but suspecting that the copper wont to be mixed as an alloy with our coined silver might have too much influence on the recited event, coming afterwards into a place where we could procure refined silver, we took an ounce of that, and having laminated it, we cast it upon twice its weight of beaten sublimate, which being driven away from it with a somewhat strong fire, we took out of the bottom of the glass retort a lump of matter, which in some places, where it lay next the glass, was as it were silvered over very finely, but so very thinly, that the thickness of the silver scarce equalled that of fine white paper; the rest of the metal (except a little that lay undissolved almost in the middle of the mass, because, as we supposed, the plates had not been beaten till they were sufficiently and equally thin) having been, by the saline part of the sublimate that stuck to it, colligated into a mass that looked not at all like silver, or so much as any other metal or mineral.

AND it is remarkable, that though silver be a fixed metal, and accounted indestructible, yet it should by so slight an operation, and by but about a quarter of its weight of additament (as appeared by weighing the whole lump) be so strangely disguised, and have its qualities so altered.

FOR (first) though an eminent whiteness be accounted the colour which belongs to pure silver, and though beaten sublimate be also eminently white; yet the mass we are speaking of was partly of a lemon or amber colour, or a deep amethystine colour, and partly of so dark an one, as it seemed black: and it was pretty, that sometimes in a fragment, that seemed to be one continued and entire piece, the upper part would be of a light yellow, which abruptly ending, the lower was of a colour so obscure, as scarce to challenge any name distinct from black.

NEXT, Whereas silver is one of the most opacous bodies in nature, and sublimate a white one, the produced mass was in great part transparent, though not like glass, yet like good amber.

THIRDLY, The texture of the silver was exceedingly altered; for our mass, instead of being malleable and flexible, as that metal is very much, appeared, if you went about to cut it with a knife, like horn, yet otherwise easily apt to crack and break, though not at all to bend.

FOURTHLY, Whereas silver will endure ignition for a good while before it be brought to fusion, our mixture will easily melt, not only upon quick coals, but in the flame



of a candle; but this resin or gum (if I may so call it) of our fixed metal did not, like that we formerly described of copper, tinge the flame of a candle, or produce with the glowing coals, on which it is laid, either a green or bluish colour.

AND, *Pyrophilus*, to discover, how much these operations of the sublimate upon copper and silver depend upon the particular textures of these bodies, I took two parcels of gold, the one common gold thinly laminated, and the other very well refined, and having cast each of these in a distinct urinal upon no less than thrice its weight of grossly beaten sublimate, I caused this last named substance to be in a sand-furnace elevated from the gold, but found not that either of the two parcels of that metal was manifestly altered thereby: whether in case the gold had been reduced to very minute particles, some kind of change (perhaps, if any, differing enough from those lately recited to have been made in the copper and the silver) might have been made in it, I am not so absolutely certain; but I am confident, that by what I reserve to tell you hereafter of sublimate's operation upon some other minerals, especially tin, it will appear, that that operation depends very much upon the particular texture of the body from whence that sublimate is elevated.

BEFORE I dismiss this subject, *Pyrophilus*, I must not conceal from you, that in the papers whence these experiments made with sublimate have been transcribed, I annexed to the whole discourse a few advertisements, whereof the first was, that I was reduced in those experiments to imploy, for want of a better, a sand-furnace, wherein I could not give so strong a fire as I desired; which circumstance may have had some influence upon the recited phenomena: and among other advertisements there being one, that will not be impertinent to my present design, and may possibly afford a not unsuccessful hint, I shall subjoin it in the words wherein I find it delivered.

THE next thing of which I am to advertise you, is this, that this experiment may probably be further improved by imploying about it various and new kinds of sublimate; and that several other things may be sublimed up together either with crude mercury or with common sublimate, he that considers the way of making vulgar sublimate, will not, I suppose, deny. To give you only one instance, I shall inform you, that, having caused about equal parts of common sublimate and sal armoniack to be well powdered and incorporated, by subliming the mixture in strong and large urinals placed in a sand-furnace, we obtained a new kind of sublimate, differing from the former, which we manifested *ad oculum*, by dissolving a little of it and a little of common sublimate severally in fair water: for dropping a little resolved salt of tartar upon the solution of common sublimate, it immediately turned of an orange-tawny colour; but dropping the same liquor upon the solution of the armoniack sublimate, if I may so call it, it presently turned into a liquor, in whiteness resembling milk. And having from four ounces of copper-plates drawn six ounces of this new sublimate after the already often recited manner, we had indeed in the bottom of the retort a cupreous resin, not much unlike that made by copper and common sublimate; and this resin did, like the other, in the moist air soon begin to degenerate into a kind of verdigreese. But that which was singular in this operation, was, that not only some of the sublimate had carried up to a good height enough of the copper to be manifestly coloured by it of a fine bluish green, but into the receiver there was passed near an ounce of liquor, that smelt almost like spirit of sal armoniack, and was tinged like the sublimate, so that we supposed the body of the *Venus* to have been better wrought upon by this, than by the former sublimate. And yet I judged not this way to be the most effectual way of improving common sublimate, being apt to think, upon grounds not now to be mentioned, that it may by convenient liquors be so far enriched and advanced, as to be made capable of opening the compact body



body of gold itself, and of producing in it such changes (which yet perhaps will enrich but men's understandings) as chymists are wont very fruitlessly to attempt to make in that almost indestructible metal. But of this, having now given you a hint, I dare here say no more.

## EXPERIMENT III.

THERE is, *Pyrophilus*, another experiment, which many will find more easy to be put in practice, and which yet may, as to silver, be made a kind of succedaneum to the former, and consequently may serve to shew how the like qualities in bodies may be effected by differing ways, provided a like change of texture be produced by them. Of this I shall give you an example in that preparation of silver that some chymists have called *Luna Cornea*, which I shall not scruple to mention particularly, and apply to my present purpose; because, though the name of *Luna Cornea* be already to be met with in the writings of some alchymists, yet the thing itself, being not used in physick, is not wont to be known by those that learn chymistry in order to physick; and the way that I use in making it is differing from that of alchymists, being purposely designed to shew some notable phenomena, not to be met with in their way of proceeding.

We take then refined silver, and having beaten it into thin plates, and dissolved it in about twice its weight of good aqua fortis, we filtrate it carefully to obtain a clear solution, (which sometimes we evaporate further, till it shoot into crystals, which we afterwards dry upon brown paper with a moderate heat).

Upon the abovementioned solution we drop good spirit of salt, till we find that it will no more curdle the liquor it falls into (which will not happen so soon as you will be apt at first to imagine); then we put the whole mixture in a glass funnel lined with cap-paper, and letting the moisture drain through, we dry with a gentle heat the substance, that remains in the filtre, first washing it (if need be) from the loosely adhering salts, by letting fair water run through it several times, whilst it yet continues in the filtre. This substance being well dried, we put it into a glass phial, which being put upon quick coals first covered with ashes, and then freed from them, we melt the contained substance into a mass, which, being kept a while in fusion, gives us the *Luna Cornea* we are now to consider.

If to make this factitious concrete, we first reduce the silver into crystals, and afterwards proceed with spirit of salt, as we have just now taught you to do with the solution, we have the exceedingly opacous, malleable, and hardly fusible body of silver, by the convenient interposition of some saline particles, not amounting to the third part of the weight of the metal, reduced into crystals, that both shoot in a peculiar and determinate figure, differing from those of other metals, and also are diaphanous and brittle, and by great odds more easily fusible than silver itself, besides other qualities, wherein having elsewhere taken notice, that these crystals differ both from silver and from aqua fortis, we shall not now insist on them, but pass to the qualities that do more properly belong to the change of the solution of silver into *Luna Cornea*.

FIRST then we may observe, that though spirit of salt be an highly acid liquor, and though acid liquors and alkalies are wont to have quite contrary operations, the one precipitating what the other would dissolve, and dissolving what the other would precipitate; yet in our case, as neither oil of tartar *per deliquium*, nor spirit of salt will dissolve silver, to both the one and the other will precipitate it; which I desire may be taken notice of against the doctrine of the vulgar chymists, and as a proof that the precipitation of bodies



depends not upon acid or alkalizate liquors as such, but upon the texture of the bodies, that happen to be confounded.

2. We may here observe that whiteness and opacity may be immediately produced by liquors, both of them diaphanous and colourless.

3. THAT, on the other side, a white powder, though its minute parts appear not transparent, like those of beaten glass, resin, &c. which, by comminution, are made to seem white, may yet, by a gentle heat, be presently reduced into a mass indifferently transparent, and not at all white, but of a fair yellow.

4. We may observe too, that though silver require so strong a fire to melt it, and may be long kept red-hot, without being brought to fusion; yet by the association of some saline particles conveniently mingled with it, it may be made so fusible, as to be easily and quickly melted, either in a thin phial, or at the flame of a candle, where it will flow almost like wax.

5. It may also be noted, that though the lunar solutions and the spirit of salt would, either of them apart, have readily dissolved in water; yet when they are mingled, they do, for the most part, coagulate into a substance that will lie undissolved in water, and is scarce, if at all, soluble either in aqua fortis or in spirit of salt.

6. AND remarkable it is, that the body of silver being very flexible and malleable, (especially if the metal be, as ours was, refined) it should yet, by the addition of so small a proportion of salt (a body rigid and brittle) as is associated to it in our experiment, be made of a texture so differing from what either of its ingredients was before, being wholly unlike either a salt or a metal, and very like in texture to a piece of horn. And to satisfy my self how much the toughness of this metalline horn depended upon the texture of the *compositum* resulting from the respective textures of the several ingredients, I precipitated a solution of silver with the distilled saline liquor commonly called oil of vitriol, instead of spirit of salt; and having washed the precipitate with common water, I found, agreeably to my conjecture, that this precipitate, being fluxed in a moderate heat afforded a mass, that looked like enough to the concrete we have been discoursing of, but had not its toughness, being brittle enough to be easily broken in pieces. But the two considerablest phenomena of our experiment do yet remain unmentioned.

FOR, 7thly, it is odd, that whereas a solution of silver is, as we have often occasion to note, the bitterest liquor we have ever met with, and the spirit of salt far sourer than either the sharpest vinegar, or even the spirit of it, these two so strongly and offensively tasted liquors should be so easily and speedily, without any other thing to correct them, be reduced into an insipid substance (at least so far insipid, that I have licked it several times with my tongue, without finding it otherwise, though perhaps with much rolling it to and fro in the mouth, it may at length afford some unpleasant taste, but exceedingly different from that of either of the liquors that composed it): and this, though the salts, that made both the silver and the precipitating spirit so strongly tasted, remain associated with the silver.

8. AND lastly, it is very strange, that though the saline corpuscles that give the efficacy both to good aqua fortis and the like spirit of salt, be not only so volatile that they will easily be distilled with a moderate fire, but so fugitive, that they will in part fly away of themselves in the cold air (as our noses can witness to our trouble, when the phials, that contain such liquors, are unstopped); yet by virtue of the new texture they acquire by associating themselves with the corpuscles of the silver and with one another, these minute particles of salt lose so much of their former lightness, and acquire such a degree of fixedness, that they will endure melting with the metal they adhere to, rather than suffer themselves to be driven away from it. Nor do I remember, that when I melted this mass in

a thin



a thin phial, I could perceive any sensible evaporation of the matter; nay, having afterwards put a parcel of it upon a quick coal, though that were blown to intend the heat, yet it suffered fusion, and so ran off from the coal, without appearing, when it was taken up again, to be other than *Luna Cornea*, as it was before.

## EXPERIMENT IV.

I AM NOW, *Pyrophilus*, about to do a thing contrary enough both to my custom and inclination; that is, to discourse upon the phenomena of an experiment, which I do not teach you to make. But since I cannot as yet, without some breach of promise, plainly disclose to you what I must now conceal, your equity assures me of your pardon. And as because the qualities of the salt I am to speak of are very remarkable, and pertinent to my present purpose, I am unwilling to pass them by unmentioned; so I hope, that, notwithstanding their being strange, I may be allowed to discourse upon them to you, who, I presume, know me too well to suspect I would impose upon you in matters of fact, and to whom I am willing (if you desire it) to shew the anomalous salt itself, and ocular proofs of the chief properties I ascribe to it.

I SHALL not then scruple to tell you, that discoursing one day with a very ingenious traveller and chymist, who had had extraordinary opportunities to acquire secrets, of a certain odd salt I had thought upon and made, which was of so differing a kind from other salts, that though I did not yet know what feats I should be able to do with it, yet I was confident it must have noble and unusual operations: this gentleman, to requite my frankness, told me, that I had lighted on a greater jewel than perhaps I was aware of; and that if I would follow his advice, by adding something that he named to me, and prosecuting the preparation a little further, I should obtain a salt exceedingly noble. I thanked him, as I had cause, for his advice, and when I had opportunity, followed it. And though I found the way of making this salt so nice and intricate a thing, that if I would, I could scarce easily describe it, so as to enable most men to practise it; yet having once made it, I found, that besides some of the things I had been told it would perform, I could do divers other things with it, which I had good cause to believe the gentleman of whom I was speaking did not think of; and I doubt not but I should have done much more with it, if I had not unfortunately lost it soon after I had prepared it.

SEVERAL of the phenomena I tried to produce with it, which are not so proper for this place, are reserved for another: but here I shall mention a few that best fit my present purpose.

FIRST then, though the several ingredients that composed this salt, were all of them such as vulgar chymists must, according to their principles, look upon as purely saline, and were each of them far more salt than brine, or more sour than the strongest vinegar, or more strongly tasted than either of those two liquors; yet the compound, made up of only such bodies, is so far from being eminently salt, or sour, or insipid, that a stranger being asked what taste it had, would not scruple to judge it rather sweet, than of any other taste; though its sweetness be of a peculiar kind, as there is a difference even among bodies sweet by nature; the sweetness of sugar being diverse from that of honey, and both of them differing from that of the sweet vitriol of lead. And this is the only instance I remember I have hitherto met with of salts, that, without the mixture of insipid bodies, compose a substance really sweet. I say, really sweet, because chymists oftentimes term the calces of metals, and other bodies dulcified, if they be freed from all corrosive salts and sharpness of taste, sweet, though they have nothing at all of positive  
sweet-

sweetness in them; and by that licence of speaking do often enough impose upon the unskilful.

ANOTHER thing considerable in our anomalous salt is, that though its odour be not either strong or offensive (both which that of volatile salts is wont to be); yet if it be a little urged with heat, so as to be forced to evaporate hastily and copiously, I have known some that have been used to the powerful stink of aqua fortis, distilled urine, and even spirit of sal armoniack itself, that have complained of this smell as more strong, and upon that account more unsupportable than these themselves; and yet when these fumes settle again into a salt, their odour will again prove mild and inoffensive, if not pleasant.

THIRDLY, Whereas all the volatile and acid and lixivate salts, that we know of, are of so determinate and specified a nature, if I may so speak, that there is no one sort of the three but may be destroyed by some one or other of the other two salts, if not by both; as spirit of urine, which is a volatile salt, being mingled with spirit of salt or aqua fortis, or almost any other strong and acid spirit, will make a great ebullition, and lose its peculiar taste, and several of its other qualities; and, on the other side, salt of tartar and other alkalies (that is, salts produced by incineration of mixed bodies) will be destroyed with ebullition by aqua fortis, spirit of salt, or almost any other strong spirit of that family: and spirit of salt, aqua fortis, &c. will be (as they speak) destroyed both by animal volatile salts, and by the fixed salts of vegetables; that is, will make an effervescence with either sort of salts, and compose with them a new liquor or salt, differing from either of the ingredients, and, as to taste, smell, odour, and divers other qualities, more languid and degenerate: whereas, I say, each of these three families of salts may be easily destroyed by the other two, our anomalous salt seems to be above the being thus wrought upon by any of all the three, and is the only body I know (which is no small privilege, or rather prerogative): for I did not find that a solution of it made with as little water as I could, which is the way whereby we usually make it fluid, would make any ebullition either with oil of tartar *per deliquium*, or spirit of sal armoniack, or strong spirit of salt, or even oil of vitriol, but would calmly and silently mix with these differing liquors, and continue as long as I had patience to look upon them, without being precipitated by them. But this is not the only way I employed, to examine whether our salt belonged to any of the three above-mentioned comprehensive families of salts. For I found that the strongest solution of it would turn syrup of violets either red, as acid spirits do, or green, as both fixed and volatile salts will do. Nor would our solution turn a clear one of sublimate made in common water either white, as spirit of urine, sal armoniack, or others of the same family, or into an orange tawny, like salt of tartar, and other alkalies; but left the solution of sublimate transparent, without giving it any of these colours, mingling itself very kindly with it, as it had done with the four lately mentioned liquors. And to satisfy my self a little further, I not only tried that an undiscoloured mixture of syrup of violets and our solution would immediately be turned red by two or three drops of spirit of salt, or green by as much oil of tartar; but to prosecute the experiment, I let fall a drop or two of a mixture made of our anomalous solution, and spirit of salt well shaken together, upon some syrup of violets, which was thereby immediately turned red, and a little of the same anomalous solution, being shaken together with oil of tartar *per deliquium*, turned another parcel of the same syrup of violets into a delightful green; which, happening as I expected, seemed to argue that our solution, though as to sense it were exquisitely mingled in the several mixtures to which I had put it, did, as it left them their undestroyed respective natures, retain its own: and yet this salt is so far from being a languid or an insignificant thing, that aqua fortis, and oil of vitriol themselves, as operative



operative and as furious liquors as they are, are unable in divers cases to make such solutions, and perform such other things as our calm but powerful menstruum can, though but slowly effect.

FOURTHLY, Though this salt be a volatile one, and requires no strong heat to make it sublime into finely figured crystals without a remanence at the bottom; yet, being dissolved in liquors, you may make the solution, if need be, to boil, without making any of the salt sublime up, before the liquor be totally or almost totally drawn off; whereas the volatile salt of urine, blood, hartshorn, &c. are wont to ascend before almost any part of the liquor they are dissolved in, which is in many cases very inconvenient.

And though this be a volatile salt, yet I remember not that I have observed any fixed salt (without excepting salt of tartar itself) that runs near so soon *per deliquium* as this will do; but by abstraction of the adventitious moisture, it is easily restored to its former saline form, and yet differs from salt of tartar, not only in fixedness and taste, and divers other qualities, but also in this, that whereas salt of tartar requires a vehement fire to flux it, a gentler heat than one would easily imagine will melt our salt into a limpid liquor.

And whereas spirit of wine will dissolve some bodies, as sanderick, mastic, gum-lack, &c. and water, on the other side, dissolves many that spirit of wine cannot, and oils will dissolve some, for which neither of the other liquors are good solvents; our salt will readily dissolve both in fair water, in the highest rectified spirit of wine (and that so little, as not to weigh more than the salt) and in chymical oils themselves, with which it will associate itself very strictly, and perhaps more too than I have yet found any other consistent salt to do.

## EXPERIMENT V.

THE experiment I am, *Pyrophilus*, now about to deliver, though I have not yet had opportunity to perfect what I designed, when some notions that I have about fire and salt suggested it to me, is yet such as may far more clearly than almost any of the experiments commonly known to chymists serve to shew us how near to a real transmutation those changes may prove, that may be effected even in inanimate, and, which is more, scarce corruptible bodies, by the recess of some particles, and the access of some others, and the new texture of the residue. The experiment I have made several ways, but one of the latest and best I have used is this: Take one part of good sea-salt well dried and powdered and put to it double its weight of good aqua fortis or spirit of nitre; then having kept it (if you have time) for some while in a previous digestion, distil it over with a slow fire in a retort or a low body, till the remaining matter be quite dry, and no more: for this substance that will remain in the bottom of the glass is the thing that is sought for.

THIS operation being performable in a moderate fire, and the bodies themselves being almost of an incorruptible nature, one would scarce think that so slight a matter should produce any change in them; but yet I found, as I expected, these notable mutations of qualities effected by so unpromising a way.

For in the first place we may take notice that the liquor that came over was no longer an aqua fortis or spirit of nitre, but an aqua regis that was able to dissolve gold, which aqua fortis will not meddle with, and will not dissolve silver as it would have done before, but will rather, as I have purposely tried, precipitate it out of aqua fortis, if that menstruum

menstruum have already dissolved it. But this change belonging not so properly to the substance itself I was about to consider, I shall not here insist on it.

2. THEN the taste of this substance comes by this operation to be very much altered; for it hath not that strong saltiness that it had before, but tastes far milder; and though it relish of both, affects the palate much more like saltpetre than like common salt.

3. NEXT, whereas this last-named body is of very difficult fusion, our factitious salt imitates salt-petre in being very fusible; and it will, like nitre, soon melt by being held in the flame of a candle.

4. BUT to proceed to a more considerable phenomenon, it is known that sea-salt is a body that doth very much resist the fire; when once, by being brought to fusion, it hath been forced to let go that windy substance that makes unbeaten salt crackle in the fire, and so by blowing it accidentally increase it. It is also known that acid spirits, as those of salt, vitriol, nitre, vinegar, &c. are not only not inflammable themselves, but hinderers of inflammation in other bodies; and yet my conjecture leading me to expect that by this operation I should be able to produce out of two unflammable bodies, a third that would be easily inflammable, I found upon trial not only that small lumps of this substance cast upon quick and well-blown coals, though they did not give so blue a flame as nitre, did yet, like it, burn away with a copious and vehement flame. And for further trial, having melted a pretty quantity of this transmuted sea-salt in a crucible, by casting upon it little fragments of well-kindled charcoal, it would, like nitre, presently be kindled, and afford a flame so vehement and so dazzling that one that had better eyes than I, and knew not what it was, complained that he was not able to support the splendor of it. Nor were all its inflammable parts consumed at one deflagration, for by casting in more fragments of well-kindled coal the matter would fall a puffing, and flame afresh for several times consecutively according to the quantity that had been put into the crucible.

5. BUT this itself was not the chief discovery I designed by this experiment. For I pretended hereby to devise a way of turning an acid salt into an alkali, which seems to be one of the greatest and difficultest changes, that is rationally to be attempted among durable and inanimate bodies. For it is not unknown to such chymists as are any thing inquisitive and heedful, how vast a difference there is between acid salts, and those that are made by the combustion of bodies, and are sometimes called fixed, sometimes alkalizate. For whereas strong lixiviums (which are but strong solutions of alkalies) will readily enough dissolve common sulphur and divers other bodies abounding with sulphur, even those highly acid liquors aqua fortis and aqua regis, though so corrosive that one will dissolve silver and the other gold itself, will let brimstone lie in them undissolved I know not how long; though some say that in process of time there may be some tincture drawn by the menstruum from it, which I have not seen tried, and though it were true, would yet sufficiently argue a great disparity betwixt those acid spirits and strong alkalizate solutions, which will speedily dissolve the very mass of common sulphur. Besides it is observed by the inquisitive chymists, nor does my experience contradict it, that the bodies that are dissolved by an acid menstruum may be precipitated by an alkalizate; and on the contrary, solutions made by the latter may be precipitated by the former. Moreover, as litharge dissolved in spirit of vinegar will be precipitated by the oil of tartar *per deliquium*, or the solution of its salt; and, on the contrary, sulphur or antimony dissolved in such a solution will be precipitated out of it by the spirit of vinegar or even common vinegar: moreover acids and alkalizates do also differ exceedingly in taste, and in this greater disparity, that the one is volatile and the other fixed, besides other particulars not necessary here to be insisted on. And indeed if that



were true which is taught in the schools, that there is a natural enmity as well as disparity betwixt some bodies, as between oily and waterish ones, the chymists may very speciously teach (as some of them do) that there is a strange contrariety betwixt acid and alkalizate salts; as when there is made an affusion of oil of tartar upon aqua regis or aqua fortis, to precipitate gold out of the one and silver out of the other, their mutual hostility seems manifestly to shew itself not only by the noise, and heat, and fume that are immediately excited by their conflict, but by this most of all, that afterwards the two contending bodies will appear to have mutually destroyed one another, both the four spirit and the fixed salt having each lost its former nature in the scuffle and degenerated with its adversary into a certain third substance that wants several of the properties both of the four spirit and the alkali. Now to apply all this to the occasion on which I mentioned it, how distant and contrary soever the more inquisitive of the latter chemists take acid and fixed salts to be, yet I scarce doubted but that by our experiment I should from acid salts obtain an alkali; and accordingly having, by casting in several bits of well-kindled coal, excited in the melted mass of our transmuted salt as many deflagrations as I could, and then giving it a pretty strong fire to drive away the rest of the more fugitive parts, I judged that the remaining mass would be (like the fixed nitre I have elsewhere mentioned) of an alkalizate nature; and accordingly having taken it out, I found it to taste, not like sea-salt, but fiery enough upon the tongue, and to have a lixivate relish. I found too that it would turn syrup of violets into a greenish colour, that it would precipitate a limpid solution of sublimate made in fair water into an orange-tawny powder. I found that it would, like other fixed salts, produce an ebullition with acid spirits, and even with spirit of salt itself, and concoagulate with them. Nor are these themselves all the ways I took to manifest the alkalizate nature of our transmuted sea-salt.

I DID indeed consider at first that it might be suspected that this new alkalizateness might proceed from the ashes of the injected coals, the ashes of vegetables generally containing in them more or less of a fixed salt. But when I considered too that a pound of charcoal burned to ashes is wont to yield so very little salt that the injected fragments of coal (though they had been, which they were not) quite burned out in this operation, would scarce have afforded two or three grains of salt (perhaps not half so much) I saw no reason at all to believe that in the whole mass I had obtained, (and which was all that was left me of the sea-salt I had first employed); it was nothing but so inconsiderable a proportion of ashes that exhibited all the phenomena of an alkali.

AND for further confirmation both of this and what I said a little before, I shall add, that to satisfy myself yet more, I poured upon a pretty quantity of this lixivate salt a due proportion of aqua fortis till the hissing and ebullition ceased, and then leaving the fluid mixture for a good while to coagulate (which it did very slowly) I found it at length to shoot into saline crystals; which, though they were not of the figure of nitre, did yet, by their inflammability and their bigness, sufficiently argue that there had been a conjunction made betwixt the nitrous spirit and a considerable proportion of alkali.

I CONSIDERED also that it might be suspected that in our experiment it was the nitrous corpuscles of the aqua fortis, that, lodging themselves in the little rooms deserted by the saline corpuscles of the sea-salt that passed over into the receiver, had afforded this alkali; as common salt-petre, being handled after such a manner, would leave in the crucible a fixed or alkalizate salt. But to this, I answer, that as the sea-salt which was not driven over by so mild a distillation and seemed much a greater part than that which had passed over, was far from being of an alkalizate nature; so the nitrous corpuscles that are presumed to have staid behind were, whilst they composed the spirit of nitre, of an highly volatile and acid nature, and consequently of a nature directly opposite to that

of alkalies. And if by the addition of any other substance that were no more alkalizate than sea-salt, an alkali could be obtained out of spirit of nitre of aqua fortis, the producibleness of an alkali out of bodies of another nature might be rightly thence inferred; so that however it appears, that by the intervention of our experiment two substances that were formerly acid are turned into one that is manifestly of an alkalizate nature; which is that we would here evince.

Perhaps it may, *Pyrophilus*, be worth while to subjoin, that to prosecute the experiment by inverting it, we drew two parts of strong spirit of salt from one of purified nitre, but did not observe the remaining body to be any thing near so considerably changed as the sea-salt, from which we had drawn the spirit of nitre; since though the spirit of salt that came over did (as we expected) bring over so many of the corpuscles of the nitre, that being heated, it would readily enough dissolve foliated gold; yet the salt that remained in the retort, being put upon quick coals, did flash away with a vehement and halituous flame, very like that of common nitre.

## EXPERIMENT VI.

I come now, *Pyrophilus*, to an experiment, which, though in some things it be of kin to that which I have already taught you concerning the changing of sea-salt by aqua fortis, will yet afford us divers other instances to shew, how upon the change of texture in bodies there may arise divers new qualities; especially of that sort, which, because they are chiefly produced by chymistry, and are wont to be considered by chymists, if not by them only, may in some sense be called chymical.

THE body, which, partly whilst we were preparing it, and partly when we had prepared it, afforded us these various phenomena, either is the same that *Glauber* means by his *sal mirabilis*, or at least seems to be very like it; and whether it be the same or no, its various and uncommon properties make it very fit to have a place allowed it in this treatise; though of the many trials I made with it, I can at present find no more among my loose papers than that following part of it that I wrote some years ago to an ingenious friend, who I know will not be displeased, if, to save myself some time and the trouble of examining my memory, I annex the following transcript of it.

[To give you a more particular account of what I writ to you from *Oxford* of my trials about *Glauber's* salt, though I dare not say that I have made the self-same thing which he calls his *sal mirabilis*, because he has described it so darkly and ambiguously, that it is not easy to know with any certainty what he means; yet whether or no I have not made salt, that, as far as I have yet tried it, agrees well enough with what he delivers of his, and therefore is like to prove either his *sal mirabilis*, or almost as good a one, I shall leave you to judge by this short narrative.

THE strange things that the industrious *Glauber's* writings have invited men to expect from his *sal mirabilis*, in case he be indeed possessed of such a thing, and the enquiries of divers eminent men who would fain learn of me what I thought of its reality and nature, invited me the next opportunity I got, to take into my hands his *Pars altera miraculi mundi*, whose title you know promises a description of this *sal artis mirificum*, as he is pleased to call it. But I confess I did not read it near all over, because a great part of it is but a transcription of several entire chapters out of *Paracelsus*, and I perceived that much of the rest did, according to the custom of chymical writings, more concern the author than the subjects: wherefore, looking upon his process of making his *sal mirabilis*, I soon perceived he had no mind to make it common, since he only bids us,

upon



upon two parts of common salt dissolved in water, to pour *A*, without telling us what that *A* is. Wherefore, reading on in the same process, and finding that he tells us that with *B* (which he likewise explains not at all, nor determines the quantity of it) one may make an aqua fortis, it presently called into my mind that some years before having had occasion to make many trials, mentioned in other tracts of mine, with oil of vitriol and salt-petre, I did, among other things, make a red spirit of nitre by the help only of oil of vitriol; remembering this, I say, I resorted to one of my *Carneades's* dialogues \*, and reviewing that experiment, as I have set it down, I concluded, that though I had not dissolved the salt-petre in water, as *Glauber* doth his common salt, yet, since on the other side I made use of external fire, it was probable I might this way also get a nitrous spirit, though not so strong. And though by calling the liquor that must make an aqua fortis *B*, whereas he had called that which is to make his spirit of salt and *sal mirabilis*, *A*, he seemed plainly to make them different things; yet relying on the experiment I had made, and putting to a solution of nitre as much of the oil of vitriol as I had taken last, though that be double the quantity he prescribes for the making of his *sal mirabilis*, I obtained out of a low glass body and head in sand an indifferent good *spiritus nitri* that even before rectification would readily enough dissolve silver, though it were diluted with as much of the common water wherein salt-petre had been dissolved, as amounted at least to double or treble the weight of the nitrous parts. The remaining matter being kept in the fire till it was dry, afforded us a salt easily reducible (by solution in fair water and coagulation) into crystalline grains, of a nature very differing both from crude nitre and from fixed nitre, and from oil of vitriol. For it coagulated into pretty big and well-shaped grains, which, you know, fixed nitre and other alkalizate salts are not wont to do; and these grains were not, like the crystals of salt-petre itself, long and hexaëdric, but of another figure, not easy nor necessary to be here described.

BESIDES this vitriolate nitre (if I may so call it) would not easily, if at all, flow in the air as fixed nitre is wont to do: moreover it was easily enough fusible by heat, whereas fixed nitre doth usually exact a vehement fire for its fusion; and though crude salt-petre also melts easily, yet to satisfy you, how differing a substance this of ours was from that, we cast quick coals into the crucible, without being at all able to kindle it. Nay, and when for further trial we threw in some sulphur also, though it did flame away itself, yet it did not seem to kindle the salt that was hot enough to kindle it; much less did it flash, as sulphur is wont on such occasions to make salt-petre do. Add to all this, that a parcel of this white substance, being without brimstone made to flow for a while in a crucible with a bit of charcoal for it to work upon, grew manifestly and strongly scented of sulphur, and acquired an alkalizate taste, so that it seemed almost a coal of fire upon the tongue, if it were licked before it imbibed any of the air's moisture, and (which many perhaps will, though I do not, think stranger) obtained also a very red colour; which recalled to my mind that *Glauber* mentions such a change observable in his salt made of common salt, upon whose account he is pleased to call such a substance his *Carbunculus*.

BEING invited by this success to try whether I could make his *sal mirabilis*, notwithstanding his intimating, as I lately told you, that it is done with a differing menstruum from that wherewith the salt-petre is to be wrought upon; I observed, that where he points at a way of making his salt in quantity without breaking the vessels, he prescribes that the materials be distilled in vessels of pure silver; whence I conjectured

\* See the Sceptical Chemist.



that it was not aqua fortis or spirit of nitre that he employed to open his sea-salt; and that consequently, since common spirit of salt was too weak to effect so great a change as the experiment requires, it was very probable that he employed oil of sulphur or of vitriol, which will scarce at all fret unalloyed silver. And however I concluded, that whatsoever the event should prove, it could not but be worth the while to try what operation such a menstruum would have upon sea-salt, as I was sure had such a notable one upon salt-petre. And I remember, that formerly making some experiments about the differing manners of dissolution of the same concrete by several liquors, I found that oil of vitriol dissolves sea-salt in a very odd way (which you will find mentioned among my promiscuous experiments): wherefore, pouring upon a solution of bay-salt, made in but a moderate proportion of water, oil of vitriol to the full weight of the dry salt, and abstracting the liquor in a glass cucurbit placed in sand, I obtained, without stress of fire, besides phlegm, good store of a liquor, which by the smell and taste seemed to be spirit of salt. And to satisfy myself the better, mingling a little of it with some of the spirit of nitre lately mentioned, I found the mixture, even without the assistance of heat, to dissolve crude gold. And having for further trial's sake poured some of it upon spirit of fermented urine, till the effusion ceased to produce any conflict, and having afterwards gently evaporated away the superfluous moisture, there did, as I expected, shoot in the remaining liquor a salt figured like combs and feathers, thereby disclosing itself to be much of the nature of sal armoniac, such as I elsewhere relate my having made, by mingling spirit of urine with spirit of common salt made the ordinary way.]

THIS, *Pyrophilus*, is all I can find at present of that account, of which I hoped to have found much more: but you will be the more unconcerned for my not adding divers other things that, I remember, I tried, as well before and after the writing the above transcribed paper (as particularly that I found the experiment sometimes to succeed not ill, when I distilled the oil of vitriol and sea-salt together, without the intervention of water, whereby much time was saved, and also when I employed oil of sulphur, made with a glass bell, instead of oil of vitriol) if I inform you that afterwards I found that *Glauber* himself, in some of his subsequent pieces, had delivered more intelligibly the way of making what he, without altogether so great a brag as most think, calls his *sal mirabilis* (which yet some very ingenious readers of his writings have come to us to teach them) and that those experiments of his about it which I was able to make succeed (for some I was not, and some I did not think fit to try); you will find, together with those of my own, in more proper places of other papers. Only, to apply what hath been above related to my present purpose, I must not here pretermitt a couple of observations.

AND first, we may take notice of the power that mixtures, though they seem but very slight, and consist of the smallest number of ingredients, may, if they make great changes of texture, have, in altering the nature and qualities of the compounding bodies. For in our (above-recited) case, though sea-salt, being a body considerably fixed, requires a naked fire to be elevated even by the help of copious additaments of beaten bricks or clay, &c. to keep it from fusion, yet the saline corpuscles are distilled over in a moderate fire of sand; whilst the oil of vitriol, by whose intervention they acquire this volatility, though it be not (like the other) a gross, or as the same chemist speaks, corporeal salt, but a liquor that has been already distilled, is yet by the same operation so fixed as to stay behind not only in the retort, but, as I have sometimes purposely tried, in much considerabler heats than that needs in this experiment be exposed to. Nor only is the oil of vitriol made thus far fixed, but it is otherwise also no less changed: for

when



when the remaining salt has been exposed to a competent heat, that it may be very dry and white, to be sure of which I several times do, when the distillation is ended, keep the remaining mass (taken out of the retort and beaten) in a crucible among quick coals, you shall have a considerable quantity (perhaps near as much as the sea-salt you first employed) of a substance which, though not insipid, has not at all the taste of sea-salt, or any other pungent one, and much less the highly corrosive acidity of oil of vitriol.

And the mention of this substance leads me to the second particular I intended to take notice of, which is a phenomenon, to confirm what I formerly intimated, that notwithstanding the regular and exquisite figures of some salts, they may, by the addition of other bodies, be brought to constitute crystals of very differing, and yet of curious shapes. For if you dissolve the hitherto mentioned *caput mortuum* of sea-salt (after you have made it very dry, and freed it from all pungency of taste) in a sufficient quantity of fair water, and having filtrated the solution, suffer the dissolved body leisurely to coagulate, you will probably obtain, as I have often done, crystals of a far greater transparency than the cubes, wherein sea-salt is wont to shoot, and of a shape far differing from theirs though oftentimes no less curious than that of those cubes; and, which makes mainly for my present purpose, I have often observed those finely-figured crystals to differ as much in shape from one another, as from the grains of common salt. And indeed I must not on this occasion conceal from you, that whether it be to be imputed to the peculiar nature of sea-salt, or (which I judge more probable) to the great disparities to be met with in liquors, that do all of them pass for oil of vitriol, whether (I say) it be to this or to some other cause that the effect is to be imputed, I have found my attempts to make the best sort of *sal mirabilis* subject to so much uncertainty, that though I have divers times succeeded in them, I have found so little uniformity in the success, as made me reckon this experiment amongst contingent ones, and almost weary of meddling with it.

#### EXPERIMENT VII\*.

I REMEMBER, *Pyrophilus*, I once made an experiment, which if I had had the opportunity to repeat, and had done so with the like success, I should be tempted to look upon it, though not as a luciferous experiment (for it is the quite contrary) yet as so luciferous a one, as, how much soever it may serve to recommend chemistry itself, may no less displease envious chemists, who will be troubled, both that one who admits not their principles, should devise such a thing, and that, having found it, he should not (chemist like) keep it secret.

BUT to give you a plain and naked account of this matter, that you may be able the better to judge of it, and, if you please, to repeat it, I will freely tell you, that supposing all metals, as well as other bodies, to be made of one catholick matter common to them all, and to differ but in shape, size, motion, or rest, and texture of the small

\* Though this VIIth experiment, being considerable and very pertinent, the author thought fit to mention it, such as it is here delivered, when he writ it but to a private friend; yet, after he was induced to publish these papers, it was the (now raging) plague, which drove him from the accommodation requisite to his purpose, that frustrated the design he had of first repeating that part of the experiment which treats of the destruction of gold: for as for that part which teaches the volatilization of it, he had tried that often enough before.



parts they consist of, from which affections of matter the qualities, that difference particular bodies, result. I could not see any impossibility in the nature of the thing, that one kind of metal should be transmuted into another (that being in effect no more than that one parcel of the universal matter, wherein all bodies agree, may have a texture produced in it like the texture of some other parcel of the matter common to them both).

And having first supposed this, I further considered that in a certain menstruum, which, according to vulgar chemists doctrine, must be a worthless liquor, according to my apprehension there must be an extraordinary efficacy in reference to gold, not only to dissolve and otherwise alter it, but to injure the very texture of that supposedly immutable metal.

THE menstruum then I chose to try, whether it could not dissolve gold with, is made by pouring on the rectified oil of the butter of antimony as much strong spirit of nitre, as would serve to precipitate out of it all the *bezoarticum minerale*; and then with a good smart fire distilling off all the liquor that would come over, and (if need be) cohobating it upon the antimonial powder. For though divers chemists that make this liquor throw it away, upon presumption that, because of the ebullition that is made by the affusion of the spirit to the oil and the consequent precipitation of a copious powder, the liquors have mutually destroyed or disarmed each other; yet my notions and experience of the nature of some such mixtures invite me to prize this, and give it the name of *menstruum peracutum*.

HAVING then provided a sufficient quantity of this liquor (for I have observed that gold ordinarily requires a more copious solvent than silver) we took a quantity of the best gold we could get, and melted it with three or four times its weight of copper, which metal we chose rather than that which is more usual among refiners, silver, that there may be the less suspicion that there remained any silver with the gold after their separation: this mixture we put into good aqua fortis or spirit of nitre, that all the copper being dissolved, the gold might be left pure and finely powdered at the bottom; this operation with aqua fortis being accounted the best way of refining gold that is yet known, and not subject, like lead, to leave any silver with it, since the aqua fortis takes up that metal. And for greater security we gave the powder to an ancient chemist to boil some more of the menstruum upon it, without communicating to him our design. This highly refined gold being by a competent degree of heat brought, as is usual, to its native colour and lustre, we put to it a large proportion of the *menstruum peracutum* (to which we have sometimes found cause to add a little spirit of salt to promote the solution) wherein it dissolves slowly and quietly enough; and there remained at the bottom of the glass a pretty quantity (in shew, though not in weight) of white powder that the menstruum would not touch; and, if I much misremember not, we found it as indissolvable in aqua regis too. The solution of gold being abstracted, and the gold again reduced into a body, did, upon a second solution, yield more of the white powder, but not (if I remember aright) so much as at the first: now having some little quantity of this powder, it was easy, with borax or some other convenient flux, to melt it down into a metal, which metal we found to be white like silver, and yielding to the hammer, if not to a less pressure, and some of it being dissolved in aqua fortis or spirit of nitre, did, by the odious bitterness it produced, sufficiently confirm us in our expectation to find it true silver.

I DOUBT not but you will demand, *Pyrophilus*, why I did not make other trials with this facitious metal, to see in how many other qualities I could verify it to be silver; but the quantity I recovered after fusion was so small, some of it perhaps being left  
either



either in the flux or in the crucible, that I had not wherewithal to make many trials; and being well enough satisfied by the visible properties and the taste peculiar to silver, both that it was a metal, and rather silver than any other, I was willing to keep the rest of it for a while, as a rarity, before I made further trials with it; but was so unfortunate as with it to lose it in a little silver box, where I had something of more value, and possibly of more curiosity.

You will also ask why I repeated not the experiment? To which I shall answer, that, besides that one may easily enough fail in making the menstruum fit for my purpose, I did, when I had another opportunity (for I was long without it) make a second attempt; and having, according to the above mentioned method, brought it so far that there remained nothing but the melting of the white powder into silver, when having washed it, I had laid it upon a piece of white paper by the fire's side to dry, being suddenly called out of my chamber, an ignorant maid, that in the mean time came to dress it up, unluckily swept this paper, as a foul one, into the fire; which discouragement, together with a multiplicity of occasions, have made me suspend the pursuit of this experiment till another opportunity. But in the mean time I was confirmed in some part of my conjecture by these things.

THE first, by finding, that with some other menstrooms, which I tried, and even with good aqua regis itself, I could obtain from the very best gold I dissolved in them some little quantity of such a white powder as I was speaking of; but in so very small a proportion to the dissolved gold, that I had never enough of it at once to think it worth prosecuting trials with.

THE other was this, that a very experienced mineralist whom I had acquainted with part of what I had done, assured me that an eminently learned and judicious person that he named to me, had, by dissolving gold in a certain kind of aqua regis, and after by reduction of it into a body, re-dissolving it again, and repeating this operation very often, reduced a very great if not much the greater part of an ounce of gold into such a white powder.

AND the third thing that confirmed me was the proof given me by some trials, that I purposely made, that the *menstruum peracutum* I employed had a notable operation upon gold, and would perform some things (one of which we shall by and by mention) which judicious men, that play the great critics in chemistry, do not think feasible; so that there seems no greater cause to doubt, that the above-mentioned silver was really obtained out of the pure gold, than only this, that men have hitherto so often in vain attempted to make a real transmutation of metals (for the better or for the worse) and to destroy the most fixed and compacted body of gold, that the one is looked upon as an unpracticable thing, and the other as an indestructible metal.

To reflect then a little upon what we have been relating, if we did not mistake nor impose upon ourselves (I say upon ourselves, the project being our own, and pursued without acquainting any body with our aim) it may afford us very considerable consequences of great moment.

AND in the first place, it seems probably reducible from hence, that however the chemists are wont to talk irrationally enough of what they call *tinctura auri* and *anima auri*; yet, in a sober sense, some such thing may be admitted: I say, some such thing, because as on the one hand I would not countenance their wild fancies about these matters, some of them being as unintelligible as the Peripatetics substantial forms; so, on the other hand, I would not readily deny but that there may be some more noble and subtiler corpuscles, that being duly conjoined with the rest of the matter whereof gold consists, may qualify that matter to look yellow, to resist aqua fortis, and to exhibit those



other peculiar phaenomena that discriminate gold from silver ; and yet these noble parts may either have their texture destroyed by a very piercing menstruum or by a greater congruity with its corpuscles than with those of the remaining part of the gold ; may stick more close to the former, and by their means be extricated and drawn away from the latter. As when (to explain my meaning by a gross example) the corpuscles of sulphur and mercury do, by a strict coalition, associate themselves into the body we call vermilion, though these will rise together in sublimatory vessels, without being divorced by the fire, and will act in many cases as one physical body ; yet it is known enough among chemists that if you exquisitely mix with it a due proportion of salt of tartar, the parts of the alkali will associate themselves more strictly with those of the sulphur than these were before associated with those of the mercury ; whereby you shall obtain out of the cinnabar, which seemed intensely red, a real mercury that will look like fluid silver. And this example prompts me to mind you, *Pyrophilus*, that, at the beginning of this paragraph I said no more than that the consequence I have been deducing might probably be inferred from the premises. For as it is not absurd to think that our menstruum may have a particular operation upon some noble and (if I may so call them) some tinging parts of the gold, so it is not impossible but that the yellowishness of that rich metal may proceed not from any particular corpuscles of that colour, but from the texture of the metal ; as in our lately-mentioned example, the cinnabar was highly red, though the mercury it consisted of were silver-coloured, and the sulphur but a pale yellow ; and consequently the whiteness and other changes produced in the new metal we obtained, may be attributed, not to the extraction of any tinging particles, but to a change of texture, whereon the colour as well as other properties of the gold did depend. But that which made me unwilling to reject the way I first proposed of explicating this change of colour, was, that a mineralist of great veracity hath several times assured me that a known person in the relator's country, the *Netherlands*, got a great deal of money by the way of extracting a blue tincture out of copper, so as to leave the body white; adding, that he himself, having procured from a friend (to satisfy his curiosity) a little of the menstruum (whose chief ingredients his friend communicated to him and he to me) he did, as he was directed, dissolve copper in common aqua fortis, to reduce it into small parts ; and then having kept the calx of the powder of this copper for some hours in this menstruum, he perceived that the clear liquor, which was weak in taste, did not dissolve the body of the metal, but only extract a blue tincture, leaving behind a very white powder, which he quickly reduced by fusion into a metal of the same colour, which he found as malleable as before ; which I the less wonder at, because the experienced chemist *Johannes Agricola*, in his Dutch annotations upon *Poppius*, mentions the making of a white and malleable copper in good quantities upon his own knowledge ; and that of such a kind of copper I have with pleasure made trial, I elsewhere relate. But of these matters we may possibly say more in a convenient place.

THE second thing that seems deducible from our former narrative is, that however most (for I say not all) of the judiciousest among the chemists themselves, as well as among their adversaries, believe gold too fixed and permanent a body to be changeable by art, insomuch that it is a received axiom amongst many eminent Spagyricists, that *facilius est aurum construere, quam destruere* ; yet gold itself is not absolutely indestructible by art, since gold being acknowledged to be an homogeneous metal, a part of it was, by our experiment, really changed into a body that was either true silver, or at least a new kind of metal very different from gold. And since it is generally confessed, that among all the bodies we are allowed to observe near enough, and to try



our skill upon, there is not any whose form is more strictly united to its matter than that of gold; and since also the operation, by which the white powder was produced, was only made by a corrosive liquor, without violence of fire; it seems at least a very probable inference that there is not any body of so constant and durable a nature, but that, notwithstanding its persisting inviolated in the midst of divers sensible disguises, its texture, and consequently its nature, may be really destroyed, in case this more powerful and appropriated agent be brought by a due manner of application to work upon the body, whose texture is to be destroyed.

BUT this matter we elsewhere handle, and therefore shall now proceed to the last and chief confectaries of our experiment.

THIRDLY then, it seems deducible from what we have delivered, that there may be a real transmutation of one metal into another, even among the perfectest and noblest metals, and that effected by facitious agents in a short time, and, if I may so speak, after a mechanical manner. I speak not here of projection, whereby one part of an aurifick powder is said to turn I know not how many hundred or thousand parts of an ignobler metal into silver or gold, not only because, though projection includes transmutation, yet transmutation is not all one with projection, but far easier than it; but chiefly because it is not in this discourse you are to expect what I can say, and do think, concerning what men call the philosophers stone. To restrain myself then to the experiment we are considering, that seems to teach us that at least amongst inanimate bodies the noblest and constantest sort of forms are but peculiar contrivances of the matter, and may by agents, that work but mechanically, that is, by locally moving the parts and changing their sizes, shape, or texture, be generated and destroyed; since we see that in the same parcel of metalline matter which a little before was true and pure gold, by having some few of its parts withdrawn and the rest transposed or otherwise altered in their structure (for there appears no token that the menstruum added any thing to the matter of the produced silver) or by both these ways together, the form of gold, of that peculiar modification which made it yellow, indissoluble in aqua fortis, &c. is abolished; and from the new texture of the same matter there arises that new form or convention of accidents, from which we call a metal silver. And since ours was not only dissoluble in aqua fortis, but exhibited that excessively bitter taste which is peculiar to silver, there seems no necessity to think that there needs a distinct agent, or a peculiar action of a substantial form to produce in a natural body the most peculiar and discriminating properties. For it was but the same menstruum, devoid of bitterness, that by destroying the texture of gold changed it into another, upon whose account it acquired at once both whiteness in colour, dissolubleness in aqua fortis, and aptness to compose a bitter body with it, and I know not how many other new qualities are attributed.

I know it is obvious to object that it is no very thrifty way of transmutation, instead of exalting silver to the condition of gold, to degrade gold to the condition of silver. But a transmutation is nevertheless more or less real for being or not being luciferous; and since that may enrich a brain that may impoverish a purse, I must look upon your humour as that of an alchymist, rather than of a philosopher, if I durst not expect that the instructiveness in such an experiment will suffice to recommend it to you. And if I could have satisfied myself that good authors are not mistaken about what they affirm of the transmutation of iron into copper, though, the charge and pains considered, it be a matter of no gain, yet I should have thought it an experiment of great worth, as well as the transmutation of silver into gold. For it is no small matter to remove the bounds that nature seems very industriously to have set to the alterations of bodies;

especially among those durable and almost immortal kinds, in whose constancy to their first forms nature seems to have designed the shewing herself invincible by art.

I SHOULD here, *Pyrophilus*, conclude what I have to say of the experiment that hath already so long entertained us, by recommending to you the repetition of what I had not the opportunity to try above once from end to end, were it not that I remember something I said about the *menstruum peracutum*, may seem to import a promise of communicating to you something of the efficacy of that liquor upon gold. And therefore partly for that reason, and partly to make sure that the present discourse shall not be uninformative to you, I would add, that though not only the generality of refiners and mineralists, but divers of the most judicious cultivators of chymistry itself, hold gold to be so fixed a body, that it can as little be volatilized as destroyed, and that upon this ground, that the processes of subliming or distilling gold to be met with in divers chymical books are either mystical, or unpracticable, or fallacious (in which opinion I think them not much mistaken); though this, I say, be the persuasion even of some critical chymists, yet, upon the just expectation I had to find my *menstruum* very operative upon gold, I attempted and found a way to elevate it to a considerable height, by a far less proportion of additament, than one that were not fully persuaded of the possibility of elevating gold would imagine; and though I have indeed found by two or three several liquors (especially the *aqua pugilum*, enigmatically described by *Basilius*) that the fixedness of gold is not altogether invincible, yet I found the effect of these much inferior to that of our mixture; touching which I shall relate to you the easiest and shortest, though not perhaps the very best, manner of employing it.

We take then the finest gold we can procure, and having either granulated it or laminated it, we dissolve it in a moderate heat with a sufficient quantity of the *menstruum peracutum*, and having carefully decanted the solution into a conveniently sized retort, we very gently in a sand-furnace distil off the *menstruum*; and if we have a mind to elevate the more gold, we either pour back upon the remaining substance the same *menstruum*, or, which is better, re-dissolve it with fresh. The liquor being abstracted, we urge the remaining matter by degrees of fire, and in no stronger a one than what may easily be given in a sand furnace, a considerable quantity of the gold will be elevated to the upper part of the retort, and either fall down in a golden-coloured liquor into the receiver; or, which is more usual, fasten itself to the top and neck in the form of a yellow and reddish sublimate; and sometimes we have had the neck of the retort enriched with good store of large thin crystals, not yellow but red, and most like rubies, very glorious to behold (though even these being taken out and suffered to lie a due time in the open air, would lose their saline form, and run *per deliquium* into a liquor). Nor see I any cause to doubt but that by the re-affusions of a fresh *menstruum* upon the dry calx of gold that stays behind, the whole body of the metal may be easily enough made to pass through the retort, though for a certain reason I forbore to prosecute the experiment so far.

BUT here, *Pyrophilus*, I think myself obliged to interpose a caution, as well as to give you a further information about our present experiment. For first I must tell you, that though even learned chymists think it a sufficient proof of a true tincture, that not only the colour of the concrete will not be separated by distillation, but the extracted liquor will pass over tinted into the receiver; yet this supposition, though it be not unworthy of able men, may in some cases deceive them. And next I must tell you, that whereas I scruple not in several writings of mine to teach that the particles of solid and consistent bodies are not always unfit to help to make up fluid ones, I shall now venture to say further, that even a liquor made by distillation, how volatile soever such liquors may be



thought, may in part consist of corpuscles of the most compact and ponderous bodies in the world.

Now to manifest both these things, and to shew you withal the truth of what I elsewhere teach, *That some bodies are of so durable a texture, that their minute parts will retain their own nature, notwithstanding variety of disguises, which may impose not only upon other men, but upon chymists themselves*; I will add, that to prosecute the experiment, I dropped into the yellow liquor afforded me by the elevated gold a convenient quantity of clean running mercury, which was immediately coloured with a golden coloured film, and shaking it to and fro, till the menstruum would gild no more, when I supposed the gold to be all precipitated upon the mercury, I decanted the clarified liquor, and mixing the remaining amalgam (if I may so call it) of gold and mercury with several times its weight of borax, I did as I expected; by melting them in a small crucible, I easily recovered the scattered particles of the elevated metal, reduced into one little mass or bead of corporal or yellow (though perhaps somewhat palish) gold. But yet whether the gold that tinged the menstruum might not, before the metal was reduced or precipitated out of it, have been more successfully applied to some considerable purposes than a bare solution of gold that hath never been elevated, may be a question which I must not in this place determine, and some other things that I have tried about our elevated gold, I have elsewhere taken notice of; only this further use I shall here make of this experiment, that whereas I speak in other papers as if there may be a volatile gold in some ores and other minerals, where the mine-men do not find any thing of that metal, I mention such a thing upon the account of the past experiment and some analogies. And therefore as I would not be understood to adopt what every chymical writer is pleased to fancy concerning volatile gold; so I think judicious men that are not so well acquainted with chymical operations, are sometimes too forward to condemn the chymists observations; not because their opinions have nothing of truth, but because they have had the ill luck not to be warily enough proposed. And to give an instance in the opinion that some minerals have a volatile gold (and the like may be said of silver) I think I may give an account rational enough of my admitting such a thing, by explicating it thus: that as in our experiment, though after the almost total abstraction of the menstruum, the remaining body being true gold, and consequently in its own nature fixed, yet it is so strictly associated with some volatile saline particles, that these being pressed by the fire, carry up along with them the corpuscles of the gold, which may be reduced into a mass by the admission of borax, or some other body fitted to divorce the corpuscles of the metal from those that would elevate them, and to unite them into grains, too big and ponderous to be sublimed: so in some mineral bodies there may be pretty store of corpuscles of gold so minute, and so blended with the unfixed particles, that they will be carried up together with them by so vehement a heat, as is wont to be employed to bring ores, and even metalline masses to fusion. And yet it is not impossible but that these corpuscles of gold, that in ordinary fusions fly away, may be detained and recovered by some such proper additament, as may either work upon and (to use a chymical term) mortify the other parts of the mass, without doing so upon the gold; or, by associating with the volatile and ignobler minerals some way or other, disable them to carry away the gold with them, as they otherwise may do; or by its fixedness and cognation of nature make the dispersed gold imbody with it. On which occasion I remember that a very ingenious man desiring my thoughts upon an experiment which he, and some others that were present at it, looked upon as very strange; namely, that some good gold having for a certain trial been coupled with a great deal of lead, instead of being advanced in colour as in goodness, was grown manifestly paler than before; my conjecture being, that so great a proportion of lead

might contain divers particles of volatile silver, which meeting with the fixed body of the gold by incorporating therewith was detained, was much confirmed by finding upon inquiry that the gold, instead of losing its weight, had it considerably increased; which did much better answer my guess, than it did their expectation that made the experiment, and were much surprised at the event. But this is no fit place to prosecute the consideration of the additaments that may be used to unite and fix the particles of the nobler metals blended with volatile bodies; though perhaps what hath been said may afford some hint about the matter, as well as some apology for the chymical term, volatile gold: the possibility of which, I presume, we have evinced by the latter part of this experiment (in which I am sorry I cannot remember the proportion of the remaining salts that were able to elevate the gold) for that I have several times made, and therefore dare much more confidently rely on it, than I can press you to do on the former part (about the transmutation, or at least destruction of gold) till you or I shall have opportunity to repeat that trial.

### EXPERIMENT VIII.

THOUGH, *Pyrophilus*, the experiment I am about to subjoin may at the first glance seem only to concern the *production of tastes*, and be indeed one of the principal that I devised concerning that subject, and that belongs to the notes I have made about those qualities, yet, if you do not of yourself take notice of it, I may hereafter have occasion to shew you, that there are some particulars in this experiment that are applicable to more than tastes. And since I had once thoughts (however since discouraged by the difficulties of the attempt) to make my notes extend even to divers qualities, which the operations of chymists and the practice of physicians have made men take notice of (such as the powers of corroding, precipitating, fixing, purging, blistering, stupefying, &c.) I presume you will not dislike that one, who had thoughts to say something even of chymical and medical qualities, if I may so call them, should give you here an experiment or two about more obvious, though particular affections of bodies, when there are several things in the experiment that may be of a general import to the doctrine of the origin of qualities and forms.

WE took then an ounce of refined silver, and having dissolved it in aqua fortis, we suffered it to shoot into crystals, which being dried, we found to exceed the weight of the silver by several drachms, which accrued upon the coagulation of the acid salts that had dissolved and were united to the metal. These crystals we put into a retort, and distilled them in sand, with almost as great a heat as we could give in a hammered iron-furnace, wherein the operation was made; but there came over only a very little fourth phlegm with an ill scent: wherefore the same retort being suffered to cool, and then coated, it was removed to another furnace, capable of giving a far higher degree of heat, namely, that of a naked fire, and in this furnace the distillation was pursued by the several degrees of heat, till at length the retort came to be red-hot, and kept so for a good while: but though even by this operation there was very little driven over, yet that sufficiently manifested what we aimed at shewing, namely, that a body extremely bitter might afford, as well as it consisted of, good store of parts that are not at all bitter, but (which is a very differing taste) eminently sour. For our receiver being taken off even when it was cold, the contained spirit smoked out like rectified aqua fortis, and not only smelt and tasted like aqua fortis, to the annoyance of the nose and tongue, but being poured upon filings of crude copper, it fell immediately to corrode them with violence,

making



making much hissing, and sending up thick fumes, and in a trice produced with the corroded copper a bluish colour, like that which that metal is wont to give in good aqua fortis.

AFTERWARDS we took minium and aqua fortis, and made a solution, which being filtered and evaporated, left us a *saccharum Saturni* much like the common, made with spirit of vinegar; then taking this sweet vitriol of lead (as we elsewhere call it) we endeavoured in the formerly mentioned sand-furnace to drive it over in a retort; but finding that degree of fire incompetent to force over any thing, save a little phlegmatick liquor, we caused the retort to be coated and transferred to the other furnace, where being urged with a naked fire, it afforded at length a spirit somewhat more copious than the silver had done. This spirit smoked in the cold receiver as the other had, and did, like it, rankly smell of aqua fortis, and was so far from retaining any of the sweetness of the concrete that had yielded it, that it was offensively acid, and being poured upon minium, it did with noise and bubbles fall upon it, and quickly afforded us a liquor, which being filtered did, by its sweetness, as well as other proofs, assure us, that there would have needed but a gentle evaporation (if we had leisure to make it) to obtain from it a true sugar of lead. And it is remarkable that the concrete, which appeared white before distillation, remained, for the most part, behind in the retort in the form of a *caput mortuum* (sometimes we have had it in a yellowish lump) which was neither at all sweet, as the vitriol of lead itself had eminently been, nor at all sour, as the liquor distilled from it was in a high degree; but seemed rather insipid, and was indeed but a calx of lead, which the heat of the fire had in part reduced into true and manifest lead in the retort itself, as appeared by many grains of several sizes that we met with in the *caput mortuum* (the rest of which is easily enough reducible by fusion, with a convenient flux, into malleable lead).

THERE are some phaenomena of this experiment, that we may elsewhere have occasion to take notice of, as particularly, that notwithstanding silver be a body so fixed in the fire, that it will (as it is generally known) endure the cupel itself, and though in the dried crystals of silver, the salt, that adheres to the silver, increases the weight of the metal but about a fourth or a third part; yet this small proportion of saline corpuscles was able to carry up so much of that almost fixedest of bodies, that more than once we have had the inside of the retort, to a great height, so covered over with the metalline corpuscles, that the glass seemed to be silvered over, and could hardly, by long scraping, be freed from the copious and closely adhering sublimate.

BUT the phaenomenon that I chiefly desire to take notice of at present is this, that not only aqua fortis, being concoagulated with the differing bodies, may produce very differing concretes, but the same numerical saline corpuscles, that, being associated with those of one metal, had already produced a body eminent in one taste, may afterwards, being freed from that body, compose a liquor eminent for a very differing taste; and after that too, being combined with the particles of another metal, would with them constitute a body of a very eminent taste, as opposite as any one can be to both the other tastes; and yet these saline corpuscles, if, instead of this second metal, they should be associated with such a one as that they are driven from, would therewith exhibit again the first of the three mentioned tastes. To prove all this, we took crystals of refined silver made with aqua fortis, and though these crystals be, as we often note, superlatively bitter, yet having by a naked fire extorted from them what spirit we could, and found that, as we expected, extremely acid, we put one part of it upon a few filings of silver, of which it readily made a solution more bitter than gall, and the other

other part of the distilled liquor we poured upon minium. And though whilst it had been an ingredient of the crystals of silver committed to distillation, it did with that metal compose an excessively bitter substance, yet the same particles being loosened from that metal, and associated with those of the lead, did with them constitute a solution, which by evaporation afforded us a *saccharum Saturni*, or a vitriol sweet as sugar. And for further confirmation, we varied the experiment, having in a naked fire distilled some dried *saccharum Saturni* made with aqua fortis, the little liquor that came over, in proportion to the body that afforded it, was so strong a spirit of nitre, that for several hours the receiver was filled with the red fumes; and though the smoking liquor were hugely sharp, yet part of it being poured upon a piece of its own *caput mortuum* (in which we perceived not any taste) did at length (for it wrought but very slowly) exhibit some little grains of a saccharine vitriol; but the other part being put upon filings of silver fell upon it immediately with noise and store of smoke, and a while after concoagulated with part of it (which it had dissolved) into a salt excessively bitter.

### EXPERIMENT IX.

The artificial transmutation of bodies being, as the rarest and difficultest production, so one of the noblest and usefullest effects of human skill and power, not only the clear instances of it are to be diligently sought for and prized, but even the probabilities of effecting such an extraordinary change of bodies are not to be neglected; especially if the version hoped for be to be made betwixt bodies of primordial textures (if I may so call them) and such bodies as by the greatness of their bulk, and by their being to be found in most of the mixed bodies here below, make a considerable part of those that we men have the most immediately to do with. Invited by these considerations, *Pyrophilus*, I shall venture to give you the account of some observations and trials about the transmuting of water into earth, though it be not so perfect as I wish, and as I hope by God's blessing to make it.

THE first occasion afforded me to do any thing about this matter was my being consulted by a gentleman (an antient chymist, but not at all a philosopher) who relating to me how much he had (with the wonted success of such attempts) laboured after the grand *Arcana*, complained to me, among other things, that, having occasion to employ great quantity of putrified rain-water, he obtained from it much less than he wished of the substance that he looked for, but a great deal of a certain whitish excrementitious matter, which he knew not what to make of. This gave me the curiosity first to desire a sight of it, in case he had not thrown it away (which by good fortune he had not) and then, taking notice of the unexpected plenty, and some of the qualities of it, to ask him some questions which were requisite and sufficient to persuade me, that this residue came not from accidental foulness of the water, nor of the vessels it was received in. This I afterwards often thought of, and indeed it might justly enough awaken some suspicions, that the little motes that have been sometimes observed to appear numerous enough in pure rain-water, whilst it is distilling, might not be merely accidental, but really produced, as well as exhibited by the action of the fire. I thought it then worth while to prosecute this matter a little farther; and having put a pretty quantity of distilled rain-water in a clean glass body, and fitted it with a head and a receiver, I suffered it to stand in a digestive furnace, till by the gentle heat thereof the water was totally abstracted, and the vessel left dry; which being taken out of the sand, I found the bottom of the glass covered over with a white (but not so very white) substance, which being scraped off

with



with a knife, appeared to be a fine earth, in which I perceived no manifest taste, and which, in a word, by several qualities seemed to be earth.

THIS encouraged me to re-distil the rain-water in the same glass body, whose bottom, when the water was all drawn off, afforded me more of the like earth: but though the repetition of the experiment, and my having, for greater caution, tried it all the while in a new glass, that had not been employed before to other uses, confirmed me much in my conjecture, that unless it could be proved, which I think will scarce be pretended, that so insipid a liquor as rain-water should, in so gentle a heat, dissolve the most close and almost indestructible body of glass itself (which such corrosive menstrooms as aqua fortis and aqua regis are wont to leave unharmed) the earthy powder I obtained from already distilled rain-water, might be a transmutation of some parts of the water into that substance; yet having unhappily lost part of my powder, and consumed almost all the rest (for I kept a little by me, which you may yet see) I should, till I had more frequently reiterated my experiments (which then I had not opportunity to do, though I had thoughts of doing it also with snow-water that I had put into chymical glasses for that purpose, and with liquor of melted hail, which I had likewise provided) and thereby also obtained some more of this virgin earth (as divers chymists would call it) to make farther trials with, have retained greater suspicions, if I had not afterwards accidentally fallen into discourse of this matter with a learned physician, who had dealt much in rain-water; but he much confirmed me in my conjecture, by assuring me that he had frequently found such a white earth as I mentioned in distilled rain-water, after he had distilled the same numerical liquor (carefully gathered at first) I know not how many times one after another; adding, that he did not find (any more than I had done) any cause to suspect, that if he had continued to re-distil the same portion of water, it would have yielded him more earth.

BUT the oddness of the experiment still keeping me in suspense, it was not without much delight, that afterwards mentioning it to a very ingenious person, whom without his leave, I think not fit to name, well versed in chymical matters, and whom I suspected to have, in order to some medicines, long wrought upon rain-water, he readily gave me such an account of his proceedings, as seemed to leave little scruple about the transmutation we have been mentioning: for he solemnly affirmed to me, that having observed, as I had done, that rain-water would, even after a distillation or two, afford a terrestrial substance, which may sometimes be seen swimming up and down in the limpid liquor, he had the curiosity, being settled and at leisure, to try how long he could obtain this substance from the water. And accordingly having freed rain-water, carefully collected from its accidental, and as it were feculent earthiness, which it will deposit at the first slow distillation (and which is oftentimes coloured) whereby it may be distinguished from the white earth made by transmutation) he re-distilled it in very clean glasses, not only eight or ten times, but near two hundred, without finding that his liquor grew weary of affording him the white earth, but rather that the corpuscles of it did appear far more numerous, or at least more conspicuous in the latter distillation than in the former. And when I expressed my curiosity to see this earth, he readily shewed me a pretty quantity of it, and presented me with some, which comparing with what I had remaining of mine, I found to be exceeding like it, save that it was more purely white, as having been for the main afforded by rain-water that had been more frequently rectified. And to compare this welcome powder with that I made myself, I tried with this divers things, which I had before tried with my own and (because the quantity presented me was less inconsiderable) some others too. For I observed in this new powder, as I had done with my own, that being put into an excellent microscope, and placed where the sun-beams might:

might fall upon it, it appeared a white meal, or a heap of corpuscles so exceeding, not to say unimaginably small, that in two or three choice microscopes both I and others had occasion to admire it: and their extreme littleness was much more sensibly discerned by mingling some few grains of sand amongst them, which made a mixture that looked like that of pebble stones, and of the finest flower. For our earth, even in the microscope, appeared to consist of as small particles, as the finest hair-powder to the naked eye. Nor could we discern this dust to be transparent, though, when the sun shined upon it, it appeared in the microscope to have some particles a little glistening, which yet appearing but in a glaring light, we were not sure to be no *deceptio visus*.

2. I FOUND, that our white powder being cast into water, would indeed for a while discolour it by somewhat whitening it, which is no more than spaud will do, and the fine dust of white marble and other stones, whose corpuscles, by reason of their minuteness, swim easily for a while in the water; but when it was once settled at the bottom, it continued there undissolved (for aught I could perceive) for some days and nights, as earth would have done.

3. HAVING weighed a quantity of it, and put it into a new clean crucible, with another inverted over it for a cover, I placed it among quick coals, and there kept the crucible red-hot for a pretty while, causing the fire afterward to be acuated with a blast of a bellows; but taking out the powder, I neither found it melted nor clotted into lumps, nor, when I weighed it again, did I see cause to conclude, that there was much of it wasted, besides what stuck to the sides of the crucible and to a little clay, wherewith I had luted on the cover (and which, to shew you that the heat had not been inconsiderable, was in several places burnt red by the vehemence of fire): and when I afterwards kept this powder in an open crucible among glowing coals, neither I, nor one that I employed to assist me, perceived it at all to smoke; and having put a little upon a quick coal, and blown that too, I found that which I had not blown away, to remain fixed (which some bodies will not do) upon quick coals, that will endure the fire in a red-hot crucible.

4. I FOUND this powder to be much heavier in specie than water; for employing a nice pair of gold scales, and a method that would be too long here to describe, I found that this powder weighed somewhat (though not much) more than twice so much common water, as was equal to it in bulk. And lest some corollaries, that seem obviously contained in the common but groundless conceits of the Peripateticks, about the proportions of the elements in density, &c. should make you expect that this powder ought to have been much more ponderous, I shall add, that having had the curiosity, which I wonder no body should have before me, to examine the gravity of the earth, which seems the most elementary of any we have, I took some sifted wood-ashes, which I had caused to be three or four times boiled in a plentiful proportion of water, to free them from salt, and having put them very dry into common water, I found them but little heavier than our newly mentioned powder, surpassing in weight water of the same bulk but twice, and a little more than a 6th part (water and it being very little more than as 1 to  $2\frac{1}{6}$ ). And that you may the less doubt of this, I will yet subjoin, that examining the specific gravity of (white) glass itself, I found that compact body to be very little, if at all, more than two times and a half as heavy as water of equal bigness to it. So that the gravity of that powder, which, borrowing a chymical term, we have been calling virgin-earth, being added to its fixedness and other qualities, it may seem no great impropriety of speech to name it earth; at least if by earth we mean not the pure elementary earth of the schools, which many of themselves confess not to be found actually separate,  
but



but a body dry, cold, ponderous, induring the fire, and, which is the main, irresoluble by water and fire into other bodies specifically different.

[But to return to the guise of the powder; when I asked this learned man, whether he observed the glass he distilled in to have been fretted by the liquor, and whether this loss of its substance, according as it despoised more powder, he answered me (and he is a person of unsuspected credit) that he found not his glass to have been injured by the liquor, and that the water wasted (though he were careful it should not do so by evaporation and transfusions) by degrees so much, that there remained by his estimate but about an eighth part of the first quantity. And though for certain reasons he kept by him the liquor last distilled, yet he doubted not but that it might be very nigh totally brought into earth, since out of an ounce of distilled rain-water he had already obtained near three quarters of an ounce, if not more, of the often mentioned earth.]

THESE several relations will, I suppose, persuade you, *Pyrophilus*, that this experiment is hopeful enough to be well worth your pursuing; if not, that perhaps none but such a scrupulous person as I would think the prosecution of it other than superfluous; and if you do acquiesce in what hath been already done, you will, I presume, think it no mean confirmation of the corpuscularian principles and hypotheses; for if, contrary to the opinion that is so much in request among the generality of modern physicians and other learned men, that the elements themselves are transmuted into one another, and those simple and primitive bodies, which nature is presumed to have intended to be the stable and permanent ingredients of the bodies the compounds here below, may be artificially destroyed, and (without the intervention of a femal and plastick power) generated or produced: if, I say, this may be done, and that by such slight means, why may we not think that the changes and metamorphoses that happen in other bodies, which are acknowledged by the moderns to be far more liable to alterations, may proceed from the local motion of the minute or insensible parts of matter, and the changes of texture that may be consequent thereunto? Some bold atomists would here be determining, by what particular ways this strange transmutation of water into earth may be performed; and would perchance particularly tell you how the continually but slowly agitated parts of the water, by their innumerable occurrences, may by degrees rub, and as it were grind themselves into such surfaces, as either to stick very close to one another by immediate contact (as I elsewhere observe polished pieces of glass to do) or implicate and intangle themselves together so, as to make as it were little knots; which knots (he would add) or the newly mentioned clusters of coherent particles, being then grown too great and heavy to be supported by the water, must subside to the bottom in the form of a powder, which, by reason of the same gravity of the molecularæ, and the strict union of the lesser particles that compose them, obtain an indisposition to dissolve in water, and to be elevated or dissipated by the fire; as their insipidness may be accounted for by its being but the same with that of the liquor, whence they were made, and their transparency by that of the water they were made of, and by the multitude of the little surfaces that belong to so fine a powder. But though in favour of such conjectures I could somewhat illustrate them, partly by applying to this occasion what I elsewhere observe of the reducing of the fluid body of quicksilver, by a bare circulation (which is but a repeated distillation) with a proportionable heat, into a real powder, which also will not so easily be raised by the fire, as the fluid body, whence by change of texture it was made; and partly by subjoining, among other things, how by the conjunction of two distilled liquors digested together I have obtained good store of an insipid substance, that would not melt in water, and that would long enough endure no inconsiderable degree of fire; though, I say, by these and other such particulars I could make our

atomist's conjectures less improbable, yet the full disquisition of so difficult a subject is too long and intricate to be proper for this place \*.

AND therefore, without here examining our atomist's explication of this metamorphosis, we will give him leave for a while to suppose the transmutation itself to be real, and thereupon to consider whether the historical part of it do not much disfavour some of the chief doctrines of the chymists, and a fundamental one of *Helmont's*. For if the purest water may be turned into earth, it will not be easy to make it improbable that the other ingredients of mixt bodies, which the chymists call their hypostatical principles, are capable of being transmuted into one another, which would overthrow one of the main foundations of their whole philosophy; and besides, if out of the simplest water itself a moderate fire can produce a large proportion of earth, that was not formerly præ-existent in it, how shall we be sure that in all the analyses which the fire makes of mixed bodies, the substances thereby exhibited are obtained by separation only, without any transmutation? As for *Helmont*, it is well enough known, that he makes water to be the material principle of all bodies here below, which he would have to be either water itself, or but water disguised by those forms, which the seeds of things have given it. I will not here examine whether this opinion, if he had restrained it to animals and vegetables, might not with some restriction and limitations be kept from appearing absurd, since my *Eleutherius* hath (though without absolutely adopting it) elsewhere pleaded for its not being so extravagant as it hath been thought.

BUT whereas *Helmont's* grand argument from experience is grounded on this, that the alkahest doth, as he affirms, by being digested with and distilled from other tangible bodies, reduce them all at last into a liquor no way differing from rain-water, though we should grant the matter of fact, yet the experiment of our powder will warrant me to question their ratiocination. For if all mixed bodies be therefore concluded to be materially from water, because they are by the operation of the fire and a menstruum, after having passed through divers previous changes, reduced at length into insipid water; by the same way of arguing (and with great cogency) I might conclude, that all those bodies are materially but disguised earth, since without intervention of a seminal principle (for *Helmont* will not allow that title to fire, which he styles the artificial death of things) water itself may be turned into earth. Indeed if that acute chymist were now alive, and had such an immortal liquor, as he describes his alkahest to be, I would gladly put him upon trying whether that menstruum would reduce our white earth into water. But there being no more probability of that, than that such reproduced water, being just what it was before, might be turned into earth again; it may be probably said, that since these bodies are mutually convertible into one another (and as to the version of water into earth, by a seemingly slight operation) they are not either of them ingenerable and incorruptible elements, much less the sole matter of all tangible bodies, but only two of the primordial and of the most obvious schematisms of that, which is indeed the universal matter; which as it comes to have its minute particles associated after this or that manner, may, by a change of their texture and motion, constitute with the same corpuscles sometimes water and sometimes earth.

BUT, *Pyrophilus*, to leave these reflexions, to return to the bold conjectures, that they are ground on; though if I had leisure and indulgence enough, I could, I confess, add many things in favour of some thoughts: † yet I would not have you wonder, that

\* What is here delivered may be for the main verified by what the reader will meet with in the (following) Xth experiment, though that be not it which the author meant.

† Of the possible ways of turning liquors into consistent bodies, by bending, breaking, twisting, and by otherwise changing the texture of the liquor, see more particularly the *History of Fluidity and Firmness*, published by the author.



whilst I was mentioning the many particulars that seem to evince the change of water into earth, I should let fall some words that intimate a diffidence about it. For to disguise nothing unto you, I must confess, that having in spite of an unusual care unluckily lost a whole paper of the powder I had made myself, and having unexpectedly been obliged to remove from my furnaces before I had made half the trials I judged requisite in so nice a case, I have not yet laid aside all my scruples.

FOR 1. I would gladly know whether the untransmuted rain-water, by the deposition of so much terrestrial matter, were grown lighter in specie than before, or sharp in taste. Next I would be thoroughly satisfied. (which I confess I am not yet, notwithstanding all that the followers of *Angelus Sala* have confidently enough written) whether and how far insipid liquors (as rain-water is) may or may not work as menstrua upon stones or earthy bodies: not to question whether the particles of rain-water may not by their mutual attrition, or some other action upon one another, be reduced into shapes and sizes fit to compose such a menstruum as the liquor was not before; as in divers plants that seemed to be nourished only with water, the sap is endowed with a sharp taste and great penetrancy and activity of parts.

2. It were also fit to know whether the glass body, wherein all the distillations are made, do lose of its weight any thing near so much as the obtained powder amounts to over and above the decrement of weight which may be imputed to the action of the heat upon the substance of the glass, in case it appear by another glass, kept empty in an equal heat, and for the same time, that the glass loses by such operations any thing worth reckoning. And it were also not impertinent to try whether the gravity of the obtained powder be the same in specie with that of the glass wherein the distillations were made (for that it differed but about a fifth part from the weight of crystalline glass I lately mentioned). Which scruple and some of the former I might have prevented, if I had had convenient metalline vessels wherein to make the distillations instead of glass ones.

3. I could wish likewise that it were more demonstrably determined what is on all hands taken for granted (as it appears indeed highly probable) that distilled rain-water is a perfectly homogeneous body; which if it be not, divers suspicions might be suggested about its transmutation into earth; and if it be, it will be, as a very strange thing, so a matter of very great difficulty to conceive, how a perfectly and exquisitely homogeneous matter should, without any addition) or any seminal and plastick principle, be brought to afford great store of a matter of much more specific gravity than itself; since we see that no aggregate we can make of bodies, but equiponderant in specie with water, doth by virtue of their convention grow specifically heavier than it.

4. HAVING had the curiosity to try whether corrosive liquors would work upon our white powder, I found that not only good oil of vitriol would corrode it, but strong and dephlegmed spirit of salt did readily work upon part of it, and that without the assistance of heat, though not without hissing and exciting great store of bubbles, as I have known such menstrua do, when put upon *Lapis Stellaris* or *Offisfragus*, or some such soft stone; as if that so much defecated rain-water, actuated by heat, had resolved some of the looser corpuscles of the sand or stone, that together with some salts compose common glass, as I have observed in some petrifying water, that some of the bodies I took up, and which were presumed to be petrified, were but cruſted over with stone that seemed generated but by the successive apposition of stony particles that, lying invisibly mingled with the running water, stuck in their passage to the conveniently disposed bodies that lay in the stream's way. But yet I must not omit, that when I suffered this mixture to settle, as much of the powder as seemed to be a very great part

of it, remained in the lower part of the liquor, as if that had rather fretted than dissolved it; and that not because the menstruum was overcharged or glutted, as I found by putting in afterwards several fresh parcels of powder, which it readily fell upon, not without noise and froth. Nor must I forget that sometimes I have excited such an ebullition, by pouring the same liquors upon the earthy part of wood-ashes, several times washed in boiling water (though, I confess, I afterwards somewhat suspected there might remain some little adhering alkali which might occasion those bubbles, notwithstanding that both I and another, whom I also invited to taste it, took the earth to be quite saltless): I might, *Pyrophilus*, add, that sometimes also methought I found this powder (which yet likewise sometimes happened to me with the lately mentioned earth of wood-ashes) somewhat gritty between my teeth, and subjoin divers other particulars, if it were not too tedious to mention to you all the doubts and considerations that have occurred to me about the recited change of water into earth: which yet are not such as ought to hinder me from giving you the historical account I have set down, since to some of my scruples I could here give plausible answers, but that I cannot do it in few words. And if any part of our white powder prove to be true earth, no body perhaps yet knows to what the experiment may lead sagacious men: and whether in a strict sense it be true earth or no, yet the phenomena that are exhibited in the production of it are sufficient to give this ninth experiment a place among the others (of the same decade) with which it is associated. For since out of a substance that is universally acknowledged to be elementary and homogeneous, and which manifestly is fluid, transparent, much lighter in specie than earth, moist and fugitive, there is artificially generated or obtained, a substance consistent, white, and consequently opacous, comparatively ponderous, dry, and not at all fugitive; the alteration is so great, and effected in so simple a way, that it cannot but afford us a considerable instance of what the varied texture of the minute parts may perform in a matter confessedly similar. And if frequently distilled rain-water should not be allowed homogeneous, our experiment will at least shew us, better than perhaps any hath yet done, how little we are bound to believe what the chymists and others tell us, when they pretend manifestly to exhibit to us homogeneous principles and elementary bodies; and how difficult it is to be certain, when a body is absolutely irrisoluble into specifically differing substances, and consequently what is the determinate number of the perfectly simple ingredients of bodies (supposing that such there are): though I must confess that my only aim is not to relate what hath been done, but to procure the prosecution of it. For if the obtained substance be by the rain-water dissolved out of the glass, this will both prove a noble and surprising instance of what may be done by insipid menstrooms, even upon bodies that are justly reckoned among the compactest and most indissoluble that we know of, and may afford us many other considerable hints that have been partly intimated already: and if, on the other side, this powder, whether it be true elementary earth or not, be found to be really produced out of the water itself, it may prove a *magnale* in nature, and of greater consequence than will be presently foreseen, and may make the alchymists hopes of turning other metals into gold appear less wild; since that by experimentally evincing that two such difficult qualities to be introduced into a body, as considerable degrees of fixity and weight (whose requisiteness to the making of gold are two of the principal things that have kept me from easily expecting to find the attempts of alchymists successful) may, without the mixture of the homogeneous matter, be generated in it, by varying the texture of its parts.

I WILL not now adventure to add any thing of what I have been attempting about the transmuting (without additaments) of pure alkalizate salts into earth, because I do

not



not yet know whether the trials will answer my hopes (for I do not yet call them my expectations). But upon this subject of transmutations I could, if it did not properly belong to another treatise, tell you something about the changes that may be wrought upon highly rectified spirit of wine, which would perchance make you think of other things of the like kind less indefeasible. For whereas it is a known thing that that spirituous liquor being kindled (and that, if you please, by other spirit of wine actually fired) will, for aught appears, burn all away, that is, be totally turned into flame; if I durst rely in so important a case on a couple of trials, whilst I hope for an opportunity of making farther ones, I would tell you, that by a way unthought on (that I know of) by any body, I have, without any addition, obtained from such spirit of wine, as being kindled in a spoon would flame all away, without leaving the least drop behind it, a considerable quantity of downright incombustible phlegm. And by another way (mentioned indeed by *Helmont*, but not taught to almost any of his readers) some ingenious persons that you know and esteem, working by my directions (but without knowing what each other was doing) did both of them reduce considerable quantities of high rectified spirit of wine (that would before have burnt all away) into a liquor that was for the most part phlegm, as I was informed, as well by my own taste, as by the trials I ordered to be made (being forced myself to be most commonly absent). From which change of the greatest part of that first liquid spirit into phlegm, it seems deducible that the same portion of matter, which by being kindled may be turned all into fire, may be, by another way of handling, turned into phlegm or water, and this without the addition of any thing, and without being wrought upon by any visible body, but one so extremely dry as duly prepared salt of tartar; and that itself is not so indispensably necessary to the obtaining of phlegm out of totally inflammable spirit of wine, but that, as I was saying, I did by another way obtain that dull liquor, without employing the salt or any other visible body whatsoever. But I make a scruple to entertain you any longer with extravagancies of this nature, and yet if I were sure you would contain your smiles, I would add for conclusion, that if I had time and opportunity to furnish myself with any quantity of that water, I had it in my thoughts to try whether that would have afforded me such a terrestrial substance as rain-water had done, and thereby have undergone a new and further metamorphosis.

#### EXPERIMENT X.

THERE is one experiment more, two of the chief phenomena of which belong to another discourse (where I particularly mention them); and yet I shall conclude this little treatise with the recitation of the experiment itself, not only because divers of the phenomena do eminently belong to our present subject, but because I have scarce met with any experiments more suitable to the design I have of shewing, before I conclude this discourse, what great and sudden productions and destructions of qualities may be effected by the composition of the smallest number of ingredients, even among liquors themselves; and such too as are believed to be both of them simple and homogeneous, and incapable of putrefaction; that so it may appear what notable alterations of qualities even seemingly slight and easy mixtures can perform among bodies both of them fluid, as well as among those that were either both of them stable, or one of them stable, and the other consistent.

TAKE then of good oil of vitriol and of spirit of wine that will burn all away, equal parts, not in quantity, but in weight; put them together by little and little, and having placed,

placed the mixture in a bolt-head or glass-egg with a long neck, and carefully stopped it with a cork and hard wax, set the vessel in a moderate heat to digest for a competent while (two or three weeks may do well) then pour out the mixture into a tall glass cucurbite, to which lute on a head and a receiver with extraordinary care, to prevent the avolation of the spirits, which will be very subtle: then with a very gentle fire abstract the spirit of wine that will first ascend; and when the drops begin to come over fourth, shift the receiver, and continue the distillation with great care, that the matter boil not over: and when you judge that about half the acid liquor is come over, it will not be amiss, though it be not necessary, to change the receiver once more: but whether you do this or no your distillation must be continued, increasing the fire towards the latter end till you have brought over all you can, and what remains in the cucurbite must be put into a glass well stopped to keep it from the air.

N. B. 1. That to the production of most, if not all of the phenomena of this experiment, it is not absolutely necessary that so long a digestion (not to say, not any) be premised; though if the time above prescribed be allowed, the experiment will succeed the better.

2. THAT, I remember, I have sometimes made use of oil of sulphur *per campanam* (as they call it) instead of oil of vitriol, to produce the recited phenomena; and though the attempt succeeded not ill as to divers particulars, yet I afterwards chose rather to employ oil of vitriol; both because it did in some points better answer my expectation than the other liquor, and because I would not give occasion to suspect that the odours, hereafter to be mentioned as phenomena of our experiment, were due to the common sulphur, whence the unctuous liquor made *per campanam* was obtained as such, and did no way proceed from the acid vitriolate salt, which that oil (as it is improperly called) doth abound with.

3. THAT I had likewise the curiosity to digest oil of vitriol with Spanish wine instead of spirit of wine, by which means I obtained an odd spirit and residence, and some other phenomena, which I content myself to have in this place given hint of, in regard that wine being a liquor of a much less simple nature than its spirit, the phenomena afforded me by this are much fitter for my present purpose.

4. THAT great care must be had in regulating the fire, when once a good part of the acid spirit mentioned in the process is come over. For if the fire be not increased, the rest will scarce ascend; and if it be increased but a little too much, the matter will be more apt than one would suspect to swell exceedingly in the cucurbite, and perhaps run over into the receiver, and spoil what it finds there, as it hath more than once happened to me, when I was fain to commit the management of the fire to others.

Now the oil of vitriol and the spirit of wine being both of them distilled liquors, and the latter of them several times re-distilled, and one of them being drawn from so simple and familiar a substance as wine, and the other from a concrete not more compounded than what nature herself (which, as I elsewhere shew, can without the help of art produce vitriol) doth divers times present us with; these liquors, I say, being both of them distilled, and consequently volatile, one would expect that by distilling them they should be brought over united, as I have tried, that the spirit of wine and of nitre, or also of common salt, may be, and as the spirits of differing vegetables are wont to be; or that at least the distillation should not much alter them from what it found them, after they had been well mingled together. But this, notwithstanding these two liquors being of very odd textures in reference to each other, their conjunction and distillation will make them exhibit divers considerable and perhaps surprising phenomena.



FOR, first, whereas spirit of wine has no great scent, nor no good one, and moderately dephlegmed oil of vitriol is wont to be inodorous; the spirit that first comes over from our mixture hath a scent not only very differing from spirit of wine, but from all things else that I remember I ever smelt. And as this new odour doth, to almost all those whose opinions I have asked about it, seem very fragrant and pleasant, so I have sometimes had it so exceeding subtile, that in spite of the care that was taken to lute the glasses exactly together, it would perfume the neighbouring parts of the laboratory, and would not afterwards be kept in by a close cork covered with two or three several bladders, but smell strongly at some distance from the phial wherein it was put. I did not think it unlikely that so noble and piercing a liquor might be of no mean efficacy in physick; and though I missed of receiving an account of its effects from some ingenious physicians, into whose hands I put it to have trials made of it, yet I cannot despair of finding it a considerable medicine, when I remember partly what hath been done by some acquaintances of mine with bare phlegm of vitriol, upon the account (as is supposed) of that little sulphur of vitriol that, though but sparingly, doth enrich that liquor; and partly, what the masters of chymical arcana tell us of the wonderful virtues of the volatile sulphur of vitriol, and what I have observed myself, that may invite me to have a good opinion of remedies of that nature.

2. BUT to shew how much the odours of bodies depend upon their texture, I shall now add, that after this volatile and odoriferous spirit is come over, and has been followed by an acid spirit, it will usually towards the latter end of the distillation be succeeded by a liquor that is not only not fragrant, but stinks so strongly of brimstone, that I have sometimes known it almost take away the breath (as they speak) of those who, when I had the receiver newly taken off in my hand, did (either because to make sport, I gave them no warning, or because they would not take it, as thinking what I told them was impossible) too boldly adventure their noses in the trial.

3. THERE is in this operation produced a liquor that will not mingle either with the fragrant or with the fetid spirit hitherto described, but is very differing from both of them, and is so very pleasant, subtile, and aromatical, that it is no less differing as well from spirit of wine as oil of vitriol. But of this liquor I give a further account in a more convenient place.

4. WHEN the distillation is carried on far enough, you will find at the bottom, that the two above-mentioned diaphanous spirits (for oil of vitriol is indeed rather a saline spirit than an oil) have produced a pretty quantity of a substance, not only very opacous, but black almost like pitch or jet.

5. And this substance, though produced by two bodies that were not only fluid but distilled, will not alone be consistent, but (if the distillation have been urged far enough) brittle.

6. AND though spirit of wine be reputed the most inflammable, and oil of vitriol the most corrosive liquor that is known, yet I could not find that this black substance would easily, if at all, be brought, I say, not to flame, but to burn, nor that it had any discernible taste; though both the liquors from whose mixture it was obtained, have an exceeding strong and pungent taste.

7. AND whereas both oil of vitriol and spirit of wine will each of them more readily than most liquors that are yet known, mingle with common water, and diffuse itself therein, I observed that this pitchy mass, if the distillation had been continued till it was perfectly dry, would not, that I could perceive, dissolve in common water for very many hours, and, if I much misremember not, for some days.

8. AND

8. AND lastly, whereas the oil of vitriol and the spirit of wine were both of them distilled liquors, and one of them exceeding volatile, and fugitive, yet the black mats produced by them was so far fixed that I could not make it rise by a considerably strong and lasting fire that would have raised a much more sluggish body than the heaviest of those that concurred to produce it.

THE remaining particulars that I have observed in this experiment belong to another treatise, and therefore I shall forbear to mention them in this: nor shall I at present add any new phenomenon to those I have already recited; those freshly mentioned experiments, and those that preceded them, being, even without the assistance of the four observations I have delivered before them, sufficient to manifest the truth I have been endeavouring to make out. For, in the experiments we are speaking of, it cannot well be pretended, or at least not well proved, that any substantial forms are the causes of the effects I have recited; for in most of the (above-mentioned) cases, besides that in the bodies we employed, the seminal virtues, if they had any before, may be supposed to have been destroyed by the fire, they were such, as those I argue with would account to be factitious bodies artificially produced by chymical operations. And it is not more manifest, that in the production of these effects there intervenes a local motion and change of texture by these operations, than it is evident and precarious, that they are the effects of such things as the schools fancy substantial forms to be: since it is in these new experiments, by the addition of some new particles of matter, or the recess or expulsion of some præ-existent ones, or, which is the most frequent way, by the transposition of minute parts, yet without quite excluding the other two, that no more skilful a chymist than I have been able to produce by art a not inconsiderable number of such changes of qualities, that more notable ones are not ordinarily presented us by nature, where she is presumed to work by the help of substantial forms; I see not why it may not be thought probable that the same catholick and fertile principles, motion, bulk, shape, and texture of the minute parts of matter, may, under the guidance of nature (whose laws the modern Peripateticks acknowledge to be established by the all-wise God) suffice likewise to produce those other qualities of natural bodies, of which we have not given particular instances.





# FREE CONSIDERATIONS

A B O U T

# SUBORDINATE FORMS,

As they are wont to be maintained by divers Learned Moderns.

## AN ADVERTISEMENT.

[THE following discourse about subordinate forms had come forth the last year, annexed to the foregoing examen of substantial forms, as a part of, or an appendix to it, being then written, and promised in the Preface to the Reader, if by reason of the book-feller's haste, who was desirous the book might be printed and published at the beginning of the term, it had not been left out, and is here added in this edition, wherein no other addition is made.]

THE generality of vulgar philosophers have for many ages so handled the doctrine of forms, as if they suspected not that more than one form could belong to a natural body; but some later writers, especially the learned *Sennertus*, and, if you will believe him, the famous Peripatetick *Zabarel* himself, have endeavoured to introduce an hypothesis, which teaches that in animals and plants, besides the specifick form, as *Sennertus* calls it, which alone is wont to be taken notice of, there may reside in those bodies, and especially in some determinate parts of them, certain other forms proper to those parts, but nevertheless so subjected to the predominant mistress form, if I may so call it, that they deserve the title but of subordinate forms, and, during the reign of the specifick form, are subservient to it, but in the capacity (as it were) of matter; yet so that when the specifick form comes to be abolished or deposed, these subordinate forms may come to set up for themselves, and in reference to those parts of matter they belong to, exercise the functions of specifick forms: as in a dog or a horse, besides the sensitive soul, which is the specifick form of the whole beast, the flesh, and blood, and bones have their distinct forms, which appertain to them as they are such bodies, though they are ruled by the soul, but as the matter which she animates and informs; and when

by death the sensitive soul or specifick form is deposed or abolished, the body is not presently resolved into the four elements, much less reduced into the first matter, but those subordinate forms do still keep the flesh, flesh; and the bone, bone; the one, for a little, and the other for a much longer time.

To make out this doctrine, he ingeniously urges the specifick virtues observable in gathered plants, and particularly the purgative faculty of rhubarb, fenna, and other cathartick vegetables. And though, as to this noble sort of examples afforded him by the specifick properties they are endowed with, when they are deprived of the life they enjoyed as plants, it may not be pretended by the obstinate, that, for aught has been yet tried, rhubarb, fenna, &c. are not purgative while they are living plants, and so, when they are dead, do not so much retain as require that specifick virtue; as wine obtains divers medicable virtues (as that of cooling, dissolving coral, pearl, &c.) when (by some alteration imperceptible to sight) losing its predominant form it turns to vinegar, which it had not before; yet it were not difficult to propose experiments that would determine this scruple, if it were thought important enough. And I shall add, that it is evident that damask-roses, for instance, which are purgative, retain for a considerable time the same colour and fragrant odour, &c. when they are gathered, and consequently acknowledged to be deprived of life, as when they grew upon the tree.

This doctrine of subordinate forms has been so well entertained, and supposed to be of such importance, and (which nearly concerns the past discourse) to afford such countenance to substantial forms, that the nature of our present discourse forbids me to leave it altogether untouched; and the rather because I have not found it so much as taken notice of by the corpuscularian philosophers. But as (on the one hand) this consideration invites me to offer something about this matter, so (on the other side) joining with the difficulty and abstruseness of the subject, it would deter a bolder writer than I, to pretend to give a full and satisfactory account of so perplexed and abstruse a matter. And therefore I shall think my attempt may be excusable (if not acceptable) if I can at present show that subordinate forms may be intelligibly explicated in a general way, according to the corpuscularian principles, or are at least very reconcileable thereunto. And in regard that, as I just now intimated, the patrons of these subjugated forms assert substantial ones, and proceed upon other notions that we do not admit, I must venture to explicate this matter in a way very differing from theirs. And it will not be amiss to begin my discourse with laying down some observations, which may serve partly to add some things unmentioned to those that are mentioned by *Sennertus* or *Zabarel*, towards clearing up the notion and nature of subordinate forms (a subject not obvious nor easy to be made plain) and partly to make way for the carrying on the subsequent part of the discourse, without those excursions that would else too much interrupt it.

FIRST then, we may consider, that, according to what I have formerly discoursed, the name form is a technical word or term of art, whose signification, as I there also noted, is not so well defined as is presumed, and were to be wished. But without much injury to the more obvious and usual notion of it, we may observe, that it is commonly some one considerable thing (or at most some few things) such, for the most part, as some conspicuous phenomenon, that is exhibited, or some peculiar operation, that is performed by it, or some particular use, to which it is applicable, upon whose account this or that form is attributed to this or that natural body; and only upon the recess or abolition of which it is said to lose its form, or, if you please, denomination.

SECONDLY, I consider that the bodies whose being or not being endowed with subordinate forms is contended for, are generally either vegetables, or animals; or bodies belonging to them; and consequently these bodies being of a very compounded nature



consist of parts, whether organical or not, that are not all of them of the same nature, which I take to be true, not only of those parts that are unanimously owned to be organical, but of many of those that are reputed similar, because as to sense they are so. This is evident in bones, which, though believed to have as good a right as any to the title of similar parts, do yet by distillation afford salt, oil, phlegm, spirit, and ashes. And vitriol, though similar as to sense, may be (as we formerly noted) artificially produced by uniting the metalline particles of iron or copper with the saline corpuscles of distilled salt or nitre; which instance I the less scruple to make use of, because, that though the patrons of subordinate forms seem to have asserted them, to give some account of what happens in vegetables and animals, when the ultimate form is abolished or expelled; yet for my own part I see not why we may not also attribute subordinate forms to divers inanimate bodies. To illustrate this matter, I will borrow an example from rhubarb (for this drug, as it is sold in the shops, is an inanimate body) wherein the purgative faculty is affirmed to proceed from a substantial form; which virtue, whilst the rhubarb grew in the ground, did, as they teach us, proceed from the specific form. For if from the same rhubarb we do, by a convenient menstruum, extract, together with the finer parts of the body, all the purgative virtue (which, as Sennertus himself teaches, may very well be done) I see not why, according to his grounds, the remaining rhubarb, which will retain divers of its former qualities, if not disclose some new ones, ought not to have a peculiar form distinct from that which he and the schools call *Forma missionis* assigned to it; to which those qualities may be attributed, and which consequently may be looked upon as a subordinate form in reference to that which the intire, though inanimate rhubarb, had before. But whatever become of this instance, there are other bodies, wherein I see not why, according to his grounds, a subordinate form may not be allowed: for in an olive or an almond (for example) though when it is gathered it ceases to be animated by the vegetative soul of the tree, yet it retains the same shape, colour, &c. that it had before it was gathered (which it retains upon the account of the subordinate form that belonged to it as such a fruit) by virtue of which form it may be preserved sound during a whole year, or perhaps much longer; so when by barely crushing the pulp of the olive between your fingers you may immediately squeeze out oil, which confessedly was pre-existent there (the pressure only associating so many parts as to make them visible) and which is a peculiar liquor endowed with noble qualities, and capable of preserving itself divers years: I see not (I say) why the form of this oil, from whence its qualities must be said to flow, may not be looked upon as having been, whilst the liquor made a part of the olive, a subordinate form to that of the entire fruit; whose remaining part having also its own peculiar qualities, and that such, whereby, for instance, an olive that has lost its oil much differs from an almond that has lost its also, may, for aught I see, deserve to have a distinct subordinate form ascribed unto it. But to make this out the better, I shall here add a couple of examples that perhaps will seem clear enough; the one is sulphur vive, wherein (to speak according to the chymical notions) nature has united under one form two bodies of very differing kinds, the one readily inflammable, and the other a great resister of fire; and yet these two are easily separable, as may appear by the known chymical practice of kindling sulphur under a glass bell; for the oleaginous part (as the combustible is supposed) manifestly burns away with a blue flame, and the saline corpuscles meeting with the moist vapours that are commonly interpersed in the air, are condensed against the sides of the glass into a highly sharp and corrosive menstruum (which may several ways be brought to exhibit its salt in a dry and brittle form). The other instance I was to mention is also of a body that cannot be pretended to be factitious (namely, *cinnabaris fossilis*)

for in this concrete under the form of a mineral stone, nature has ranged three (if not more) complete bodies, that has each of them its own distinct form, and that exceeding different from the others, as may appear when these bodies are skilfully separated. For thence, as we noted above, we have obtained a running mercury, an inflammable sulphur, which itself will be easily allowed to be a compounded body, and a strange concrete, whose properties I had not occasion to look into. To these instances I might add divers others, if it were necessary so to do. And if it be said that these forms are not subordinate, but rather co-ordinate, it will lie upon the objectors to prove it; who perhaps will find it no easy matter to evince, that the same ingredient, for instance, of sulphur, is not as much subjugated by the form of the entire body, as that of the purgative portion of rhubarb, by the form of that drug. But if it did appear that these forms were more properly styled co-ordinate than subordinate, it would not much trouble me, who am inclined to think that divers of the forms which *Sennertus* and his followers call subordinate or subjugated, may be as fitly styled co-ordinate or concurrent; since I shall show anon that I do not ascribe to the specific or supreme form, in reference to the rest, such a coercive power and dominion, as those learned men are pleased to do.

THIRDLY, I consider that all these differing bodies, whereof, as of parts, or as of ingredients, a compounded body is made up, are by virtue of the composition and peculiar fabrick thereof so put together or contrived, that they concur to those actions or operations which are proper to the body as such, and therefore are presumed to flow immediately from the form of it. For an instance of which I shall name gunpowder, where three ingredients upon a very slight mixture (as I shall anon shew theirs to be) do, by a concurrent action, produce those wonderful effects that are scarce to be matched by nature herself. And that these stupendous operations really result from the proportion of the ingredients, and the manner of their commixture, will be hereafter manifest.

FOURTHLY, I consider that notwithstanding these several parts, whereof the compounded body consists, do in the proper, and, if I may so call them, specific actions of the body so concur, as to perform them jointly, and (as the schools in divers cases express themselves) *per modum unius*: yet these thus conspiring bodies may each of them retain those attributes or that modification, which made it a distinct natural body, before it came to be associated with those others, with which it makes up a more compounded body.

And if it be proper to propose here an argument *ad hominem*, I shall add, that the more considerate of the modern schoolmen themselves do, though perhaps unawares, teach such things as do very well agree with the doctrine of subordinate forms. For when in the generation of man they tell us, that, as *Aristotle* also observes, the embryo lives the life of a plant and of an animal, before he attains to live the life of a man, it is plain, that, according to them, upon the introduction of the rational soul the vegetative and sensitive souls, that before successively informed the embryo, do so no more, the advenient human soul becoming now the true form of the human body. And these pre-existent souls are not abolished, and do not lose their being, but only their office, which at first was to inform the body of the embryo, but now ceases, so that they are not destroyed, but only deposed. And this consideration seems to afford ground enough to admit in divers natural bodies, forms that dispose the matter they modify for the reception of a noble stamp, for which reason I sometimes call them preparatory forms, besides those more noted forms, that the schools usually term specific, (and which I sometimes call predominant or supreme) by which I suppose is meant (to speak intelligible) the last and highest stamp, or modification, that nature gives that parcel of matter; whereas the preparatory form is but (if I may so speak) a harbinger that dis-

poses



poses the matter to receive a more perfect form, which, if it be not to be succeeded by any other more noble, is intitled the specifick form of that body; as in the embryo the vegetative and the sensitive soul is but preparatory to the rational, which alone is said to be the specifick form of man.

BUT here I would not be thought to adopt for mine all those opinions upon which I think it allowable for me to argue with those that own them. For I must not omit to intimate *in transitu*, that I elsewhere consider with what congruity to some other of their tenets they can assert, and in what sense, in regard of the nature of the thing itself, we may admit, that the souls of all living creatures be the true forms of their bodies, notwithstanding the scruples suggested to me, as by other things, so particularly by the great difference I take notice of by some of these animating forms (if I may so call them) and other natural forms in reference to the manner of their informing the respective bodies they belong to; of this to give an instance, it is evident that the reasonable soul (which some call *animus*, to distinguish it from the *anima* or sensitive) is not the architect of the human body (which they confess and teach must be organized before it be fit to have that united to it) as many other forms are said to be of theirs; nor do all the properties, or so much as all the specifick ones, flow from that soul (whose mansion was a living animal of a determinate kind before it was united thereto) as those of other natural compounds are held to flow from their forms. And even in beasts and plants (if we will rather consider the thing than mens opinions) if the soul be all the form, there will remain in the matter after the abolition of the form great store of qualities, that by their so remaining show, that they do not flow from the soul, as gravity is said to flow from the form of the earth, and transparency from that of the air, and these surviving qualities are oftentimes not only many, and several of them noble and specifick, as appears in the beauty, fragrancy, and cordial virtue of oranges, lemons, &c. but oftentimes the same, that were there in the body, for aught our senses can perceive, whilst it was said to be informed by the soul. And I believe it would puzzle a Peripatetick to discriminate an apple or an orange, which, having been plucked off from the tree, were with a slender thread artificially tied on again by the stalk, from the other fruit as yet growing on the same branch. And not only the letters that were carved on the bark of young trees, and grew with them, remain as fair and legible as ever, when the trees are cut down; but a dead body for some time after death (and it matters not how little a while, provided the soul, and consequently the specifick form, be really destroyed or departed) does oftentimes so exactly retain the shape, feature, and even colour, warmth, and other qualities, which it had whilst the soul (a little before) was there, that it often puzzles the best physicians (especially if the sick person were hysterical or apoplectical) to discern with certainty whether the patient be dead or alive. And as for that conceit of a *forma cadaveris*, whereby divers of the modern Peripateticks have attempted to decline the inconvenience of allowing, contrary to the doctrine of very many of their party, that the same qualities remain in *corrupto* (as they speak) as were in *genito*; and that in spite of their general and fundamental tenet, the matter may be for some time (how little soever it imports not) without a substantial form: this cadaverous form, I say, that seems much to disparage our senses (which witness divers of the remaining qualities to be the same they were before) seems to be deduced without any ground from the phænomena of nature, being introduced (as \* *Suarez* himself, though a friend to this expedient, ingeniously confesses) but because it is consonant to the Peripatetick doctrine that it should be so (though that itself be not

so evident, but that *Scotus*, and I know not how many of the Aristotelians themselves, reject this form, if they do not also deride it).

Nor need we be very solicitous how the parts of a dead body can be kept together, if neither the soul, nor some new substantial form that succeeds it, perform that office; since competent agents, whatever they were, having contexted a portion of matter into such a human body as the soul left upon its departure, the fabrick of the body and connection of the parts will suffice to make it retain for a little while (and that is enough for our purpose, since dead bodies are not wont to remain long unaltered) their pristine shape, and divers other manifest qualities which may continue till the action of outward agents upon the less solid parts of the body, or the internal and inordinate commotions of the juices, and the softer, though not fluid parts that are contained in it, do by their degeneration vitiate the texture, and consequently the manifest qualities of it. And if these inordinate agitations of the blood, humours, &c. be hindered, though by an external cause, the body, notwithstanding the loss of the soul, will continue unputrefied, not only for some hours, but for many months together; as a learned eye-witness, whom I inquired of, assured me, he, as well as many others have observed in very cold countries, as *Russia*, *Sweden*, &c. where they often keep those bodies that die in the winter unburied, and yet sweet, till the spring, when the sun's heat makes them begin to putrefy. And it is plain, in some aromattick gums and fruits, that bodies that were once plants, may, after they have lost the vegetative soul, not only continue many years uncorrupted, but by embalming other bodies keep them so too.

BUT, as I was saying, the prosecution of this inquiry (*whether in living creatures the soul be always the true form, to all the intents and purposes that the vulgar philosophers would have it*) belongs not to this place, else I might also question the congruity of what is taught as well by *Sennertus*, as the schools, that upon the supervening of the ultimate or specifick form, the forms, that thereupon become subordinate, do but make a part of the matter informed by the new form. I grant indeed, that they may qualify and dispose the several portions of matter they belong to in such a way, as that they make the body they consist of a fitter subject or receptacle for the ultimate form that is to be introduced: and there may be a necessity of such previous dispositions in the subject, because the compounded body is of such a nature, as that no other bodies but such as are thus and thus qualified, are fit to make it up. But it seems not to me so easy to conceive how a substance distinct from matter (for such both he and they make their substantial forms to be) can properly be said to have its capacity confounded with that of the matter. And notwithstanding the lately mentioned distinction betwixt specifick and preparatory forms, those (last named) seem to me as true ones, whilst they are either sole or predominant, as the specifick themselves. For bodies are what they are by the matter and modification that do for the present constitute them, whatever they may prove to be in the future; and it is extra-essential to the form that is said to be previous, that it is to be succeeded by another which is said to be more noble. A spring of steel is a true and perfect spring before it be made a part of the watch, and by becoming so it is not really bettered in its nature, though it be made indeed more useful to man: and when copper is turned into vitriol, copper was a true and complete metal before, and it is accidental to the copper, that corrosive spirits coagulate themselves with it into a salt-like substance. And antimony is true and perfect antimony before it be turned into glass, whether afterwards it happen to be or be not changed, by the bare operation of the fire, from a black and opacous mineral to a fine red and transparent glass.

AND



AND though I know *Aristotle* attributes to forms *τιμὸς καὶ ἀτιμία*; yet it is not always so easy duly to apply those civil appellations to physical things, and to determine whether a succeeding form be more or less noble than the precedent: as when, for instance, pearls are reduced by salts into a chymical magistery, and vitriol is made of iron or copper; where, for divers oeconomic and military uses, the metals themselves are fitter than the magisteries (as in goldsmiths shops and on ladies necks, the intire pearls are much more prized than the prepared ones) and for other purposes, especially in physick, which regards the health of man, the magisteries are better than the crude metals. And these instances put me in mind of taking notice, that as the supervening of a form does not always destroy the old, as in vitriol (such as I formerly mentioned) the copper retains its metalline nature under the disguise of a salt; so upon the abolition of the ultimate form, the previous form may in divers cases be reduced to the exercise of its former functions; as, out of such vitriol as I am speaking of, it is easy, without the addition of any metalline substance, to recover true and malleable copper. But I have dwelt too long upon this fifth consideration, which will invite me to make shorter work with those that follow.

FIFTHLY, But before I proceed to them, it will not be amiss to intimate, that one may, if one pleases, make some distinction between subordinate forms, there being one sort of them that may deserve a peculiar name. For in men, horses, sheep, and other perfect animals, there are divers parts, especially those that physicians call similar (in opposition to organical ones) such as bones, ligaments, membranes, which seem evidently to challenge peculiar and distinct forms: for the diversity of their nature, being very manifest and stable, persevering oftentimes a great while (as appears in bones) after the death of the animal, those that allow that a natural body is what it is upon the account of its form, cannot well deny these so distinct bodies distinct forms; which, because the bodies they constitute are the parts of a human body, some modern school-men have (not very inconveniently) called partial forms. But this distinction being not of so great weight that we need insist upon it, the notice already taken of it may at present suffice.

SIXTHLY, I consider, that among the constituent parts of an animal or plant there may lurk some seminal principles or rudiments, that is, small parcels of matter of such a texture, that though whilst they remain associated with the other parts of the compounded body, they are not by sense (especially when that is employed with no greater attention than is usual) distinguishable from the rest of the compounded body, comes to have its predominant form abolished, these seminal principles or rudiments being set at liberty, and befriended by external heat, and the softness which usually attends corrupting bodies, and perhaps by a lucky concurrence of other circumstances may fall to act according to their own nature, and generate insects, moss, &c. as I have more amply declared in other papers\*.

SEVENTHLY, I consider, that besides that when the specific form of a body is destroyed, the change is not oftentimes so great as vulgar philosophers imagine; the corruption of the animal or other body ought not to be looked upon, as if it happened in some of those imaginary and empty spaces that are conceived to be beyond the universe, but in this world of ours, where the body, which is deprived of its specific form, is subject to be acted upon by the sun, the air, and I know not how many powerful agents, by whose various concurrence with, and operations upon the body, either the pre-existent, though lately eclipsed forms, may be assisted to set up for them-

\* Essays about spontaneous generation,

selves, or new forms may result from new leagues and contexts of the particles that composed the body that lost its principal, or, if I may so call it, sovereign form (as we have largely discoursed in the lately mentioned papers)\*.

THESE observations being laid down, to avoid the necessity of too much interrupting our future discourse, by being obliged to interpose some of the premised explications and other passages, as obscure and difficult, as we readily confess the subject we are treating of to be, we shall now adventure to try, whether about *Sennertus's* doctrine we can propose any conjectures, that being as agreeable to the phenomena, as his are congruous to the corpuscularian hypothesis, according to which we have hitherto discoursed of forms.

AND (to begin with a concession) I allow the learned *Sennertus* and his followers to be in the right, who, without fearing the invidious title of innovators, asserted, that in an animal or a plant there was something else besides the bare *materia prima*, and the vegetative or sensitive soul, with its essential faculties. And the instances they bring to shew, that in some parts of such bodies there may lurk peculiar forms, which, when the life of the plant or animal determines, come to disclose themselves, are probable enough; and the instances taken by *Sennertus* from the specifick virtue that survives in gathered plants, and particularly the above considered purgative faculty of rhubarb, are ingeniously alledged for their purpose. And it is probable too, what *Sennertus*, according to their grounds, teaches, that this purging property in rhubarb, senna, &c. as it does not flow from the vital soul of the plant, which is already destroyed, so it does not proceed barely from the form of a mixed body as such; it being no way likely that so great a variety of specifick properties, as roots plucked out of the ground, and fruits torn from the tree, are endowed with, should proceed merely from that general form that belongs in common to compounded bodies as such. To which argument I forbear to add that other, wherewith it is seconded by *Sennertus*, though he and others of several parties are wont to lay much weight upon it; namely, that these properties flow from the specifick form, which, even in inanimate bodies, is of a sublime nature, and must be the author of such peculiar virtues, which, according to him, being far above the reach of elementary qualities, cannot be produced by any mixture whatever of the elements: this argument, I say, I decline to urge in this place, because I elsewhere purposely examine it, and having declared in what sense only it seems to be safely grantable, I reject the chief supposition on which it leans.

To proceed then to the next part of our discourse: though (as I was saying) there be some things about the doctrine of subordinate forms, wherein I dissent not from these learned men, yet there are others, wherein I must confess myself unsatisfied; for neither do I acquiesce in some of the notions whereon they ground the things, wherein we agree, nor do I agree with them in some of the main things they assert: and especially having in the past discourse rejected substantial forms, it is not to be expected that we should either employ them in our explications, or admit those explications that necessarily suppose them.

THEY teach us indeed that the specifick form of a body does command all the subordinate forms, and use them but as instruments to its own purposes, those forms belonging then to the matter, which the specifick informs and rules. But for my part, that do not acknowledge in many bodies, that are or may be said to have subordinate forms, any thing substantial distinct from matter, I confess I do not readily conceive, which way this dominion attributed to the specifick form is exercised, nor do I see any necessity

\* Especially in the later part or essay.



of admitting any such power in that form, nor that the portions of matter that are endowed with those forms, that are said to be subordinate, can, being under the degrees of souls, and consequently unfurnished with knowledge and will, pay this presumed superintendant form any obedience; I mean any other obedience, than some such kind of one as the parts of a clock or engine may be said to yield to one another. I should therefore rather conceive the matter thus; when divers bodies of differing natures or schematisms come to be associated, so as to compose a body of one denomination, though each of them be supposed to act according to its own peculiar nature, yet by reason of the coaptation of those parts, and the contrivement of the compounded body, it will many times happen, that the action or effect produced will be of a fixed nature, and differing from that, which several of the parts, considered as distinct bodies or agents, tended to, or would have performed. As when in a balance, by putting in a weight into one of the scales, the opposite scale, though as a heavy body it will naturally tend downwards, yet by virtue of the fabrick of the instrument is made to mount upwards. And when an archer kills a deer with his arrow, the bow being a springy body, naturally endeavours to unbend itself; and the spring being fastened to the bow, must necessarily follow the motion of it, and the shaft, though a heavy body, and as such, tending directly downwards, is by the forcible impulse it receives from the spring, thrown with such violence (not directly downwards, but in a parabolical or some such crooked line) as far more strongly to hit the mark, than it would (if left to itself) have struck the ground. So that those actions, which *Sennertus* and others attribute to the conspiring of subordinate forms to assist the specifick and presiding form, we take to be but the resultant actions of several bodies, which being associated together, are thereby reduced in many cases to act jointly, and mutually modify each other's actions; and that, which he ascribes to the dominion of the specifick form, I attribute to the structure, and especially to the connexion of the parts of the compounded body: as in a clock, though all the parts it consists of do contribute to the performance of those things that belong to a clock, as regularly if they intended so to do, and did not only concur, but knowingly conspire in what they do, yet in all this there is no substantial form to superintend their motion; but the lead (or other weight) tends downwards as it is wont to do; and the hand, wheels, and other parts do only perform such motions as they are forcibly put into by other bodies, which by the descending weight (that does not in the least intend what it effects) are themselves set a moving. And notwithstanding the prodigious operations that men admire in gun-powder, yet not only, as we formerly intimated, this strange power is but the effect of the mechanical texture, and of the way wherein the ingredients are mingled, and as it were contexted; but this artificial mixture is far more slight than those made by nature are wont to be. For as the efficacy of the mechanical texture in gun-powder may appear by this, that neither of the ingredients (whether the sulphur, the nitre, or the coal) is apart able to produce effects any thing near like gun-powder; so to convince others how slightly the ingredients are mingled, I thought the best way was to shew how easily they may be separated again; to which effect I beat good gun-powder small, and having boiled it a pretty while in a considerable proportion of water, by exhaling a sufficient quantity of the well-filtrated and limpid decoction. I obtained store of crystals, whose figure, taste, and way of flashing upon a quick coal proclaimed them to be good salt-petre; the black stuff left in the filter remaining, if the solution had been well made, insipid enough, and when dried it will not blow up like the gun-powder, but (in great part) burn along with a blue flame like common brimstone. And for farther proof we may, by boiling this black stuff in a pretty strong lixivium, dissolve the sulphur, as will appear both by the smell that the lixivium will



acquire, and by this, that if you filter it, though the liquor will pass clear enough, and leave the black and coal-like part in the filter; yet by dropping into it some quantity of an acid liquor (I used spirit of salt) the sulphureous smell will be increased, and the liquor will be made white by the precipitation of sulphureous corpuscles; whereas if I put spirit of salt into that clear solution, which (I was saying) afforded me the crystals of nitre, the liquor, not troubled by any such precipitation, would continue limpid as before; which argued that the salt-petre had not intimately incorporated any sensible quantity of the sulphur with itself, but had been only slightly associated with it.

AND to illustrate what I said of resultant actions by an instance purely physical, I shall subjoin what I somewhere mention, with another aim, that by taking a couple of powders fit for my purpose, one blue and the other yellow, and mingling them in a certain proportion, the mixture exhibited a green colour, which did not flow from any new predominant form, which made the blue and yellow corpuscles subservient to its purposes (for an excellent microscope shewed me the blue and the yellow particles such as they were before) but only hence, that from the mixture of those bodies, the distinct actions of the blue and the yellow corpuscles did upon the eye make a compounded impression, like that made by bodies to whom their specifick forms are supposed to impart, among other qualities, greenness. And when vitriol or sublimate are made by art, there needs nothing besides the manner, wherein the saline and metalline particles are contexted, either to contain the parts together, and keep them united into body, or (notwithstanding their not only distinct, but very differing forms) to enable the mixture they compose to effect divers things, which neither of them single would have performed; nay and some of them such things (as to vomit, purge, &c.) as merit to be reckoned among such specifick properties, as many of those are, which when preserved in vegetables, are thought to argue the conspiring of several forms under the direction of a superintendent one.

AND as in a watch the spring is really a spring, and acts as a spring, whilst it is a part of the watch, though by reason of its connexion with the other parts it is reduced to concur with those other parts towards exhibiting the phenomena proper to the whole engine; and though the watch were taken all in pieces, the spring would be a spring still: so in many compound bodies, besides the specifick form, which the body has as such, and which may be called its total or general form, particular bodies (by whose association and conjunction it is made up) may enjoy their own distinct forms, which may therefore be called partial ones; and these bodies, though whilst the whole subsists they are part of it, and by their connexion with the rest concur to the operations of the body as such, which joint operations are wont to be those, that are attributed to the specifick form; yet they do not always so depend upon it, but that when it is abolished, they may retain their own nature; as a bone will be a bone still, whole ages after the animal it belonged to is dead: whence we need not wonder, that divers forms should survive in bodies deprived of their specifick form. For indeed those that are called subordinate, may be as true and real forms (nay, and substantial forms, if in any living creature, besides man, there were any such) as that which bears the title of specifick; and even whilst this is in being, there are many things which compounded bodies perform by virtue of their particular forms, rather than upon the account of the specifick: as (not to repeat the newly mentioned instance of a spring) in vitriol the friableness, transparency, and aptness to mingle with water, need not be attributed to the composition as such, but may, for aught we know, be due to the saline corpuscles, which not only retain their own nature (as may be argued from some vitriols that I have made since I have been able to separate and recover them again out of the mixture) but to reduce the metal, they have corroded, into a salt-like body with themselves. And in

gun-



gun-powder it is manifest, that the blackness proceeds not from the compositum as such, but from the coals, as the nitrous taste does from the salt-petre. And the fragrantcy of a rose, whilst it grows upon the bush, need not be conceived to proceed from the soul or life of the plant, since, when it is gathered, it retains the same grateful smell.

AND this last instance leads me to a farther consideration, wherewith I shall conclude this discourse. We may call to mind what was observed a little after the beginning of it, of the arbitrary, or at least not sufficiently settled use of the word *form*; and that it not seldom happens, that those things, upon whose account we attribute this or that form to a natural body, are but very few of those many attributes that belong to it. Now the form of a body being really no more than a convention of accidents, whereby the matter is stamped and denominated, it is very consonant to reason, that oftentimes hostile agents or causes may deprive the matter of those accidents, which constituted the specifick form, and yet leave the rest, which, according to the law of nature, ought to continue there, till some competent agent put the body out of that state, wherein, upon the form's decease, it was left.

AND to clear up this matter, we may consider, that the same body may have a two-fold modification, and be thereby fitted for two, if not more, states and kinds of operations, not necessarily dependent upon one another. For as the spring of a watch by virtue of its texture is an elastic body, and upon the account of another is iron, and therefore though being cast red-hot into cold water it will become stiff and brittle, and consequently cease to be a spring, yet it will continue iron, that is, a hard metalline body easily subject to rust, capable of striking fire with a flint, and of being attracted (as men commonly speak) by a load-stone, and of attracting a magnetical needle; so in a rose, for instance, we may distinguish or consider a two-fold modification of the matter, one, whereby it is fitted to receive from the bush it grows on a certain peculiar and spirituous sap, by whose intervention and concurrence it has nourishment and growth, and consequently exercises vital functions as a part of a living plant; and another which does not so much require the accession of fluid and moveable parts, but consists rather in the texture of the more stable parts: and this texture being commonly more permanent and durable than the other part of the modification (consisting much in the peculiar motion of a fluid substance) wherein the life participated by the rose consisted, may last, when the flower is deprived of its soul and specifick form by its avulsion from the bush, and retain those qualities, as well occult as manifest, that naturally result from a parcel of matter so contrived.

I MAY somewhat illustrate my meaning on this occasion, by making a comparison betwixt a living creature and a mill. For as a mill is capable of performing divers things only when the water, that passes through certain of its parts, put them, and, by their intervention, others into motion; so there are divers things, that are not performable by a plant, unless when it is irrigated by a vital liquor. And as a mill may nevertheless retain the nature of a structure useful for other purposes, though the drought of summer have per chance made it lose, or the frost have congealed into ice the water that used to drive it; so although the soul of a plant be destroyed, or cease to act, the body may, upon the account of the more permanent structure of its stabler parts, retain a fitness for diverse of the same purposes it served for before. And if it were here pertinent, the comparison might be carried on a little farther by adding, that as when a mill does upon either of the lately mentioned accounts cease to perform the peculiar operation of a mill, as the wood, iron, and other materials of that mill are not destroyed; so neither does the water vanish into nothing, but either loses its motion, and by being congealed exchanges the name of water for that of ice, or else is dissipated and scattered into exhalations,



lations, which contain all the substance that ever the water had, that, which is lost, being but the usual manner of coexistence of the water and the mill it was wont to drive : so when a plant is pulled out of the earth, or a rose from the bush, as the dismembered part of the plant may retain the texture of its more stable parts ; so the sap or juice, that were wont to enliven the body, does, though invisibly, remain either in the form of steams exhaled into the air, or perhaps in parts condensed and intercepted upon the loss of its wonted agitation in the imperceptible cavities of the fibres, and other parts of the plant, so that nothing, that is substantial, perishes, but only the particular modification, that resulted from the peculiar kind of union of the more permanent parts with those congruously shaped and fitly agitated fluid ones, that permeated them. As to some purposes, the example of a wind-mill, being set on work merely by the impulse of the air, may be more apposite than that of the water-mill ; but neither of them affords any more than an imperfect comparison in this regard, among others, that whereas the mill itself by losing even for a very long time, the motion it was wont to be in, is not thereby considerably impaired, because of the solidity of the materials it is made up of ; in vegetable, when that fluid substance, whereof the soul chiefly consists, quite ceases to be influent, one of its chief functions being to repair continually, by assimilated or transmuted aliments, the waste that was continually made of looser parts in the body it belonged to, the same agents or causes, that destroyed the life of the plant, are wont likewise to produce or occasion such a discomposure in the texture of the remaining part (especially those that are more tender or more slightly connected) that they quickly become unfit to be animated again, though a fluid substance, like that which was wont to irrigate it whilst the vegetative life lasted, should be again communicated to it ; but yet even in this regard the difference betwixt a mill and a plant is not always so vast as one would imagine. For in classick authors we have relations of a staff or a pike made of a durable wood, that many years after the tree had been cut down, being casually struck into the ground, took root there. And as for the rose of *Jericho*, as they call it, a late modern writer, followed, as I remember, by another naturalist of good account, affirms, that divers years after it is gathered, and seems to be quite shriveled up and withered, it may by the help of water be so far recovered, as to be plumped up again, and display its leaves almost as if it had not been long since gathered. And I myself have, not without some wonder, observed, how very long a plant of aloes torn from the ground, and hung in the air near the ceiling of my chamber, would not only continue lucculent, but (perhaps after some years) be capable of being made to perform acts that are wont to be ascribed to vitality and growth, upon the dexterous administration of a convenient liquor : and even some animals themselves are not so very unlike to these plants, and consequently to engines, as one would think. For that which children are said to do for sport about reviving drowned flies, challenges a more serious consideration than were fit for me to insist on now, and deserves to be both heedfully experimented, and seriously reflected on by a naturalist. I chose to try it chiefly upon wasps and bees, rather than upon flies, because their bigness renders the phenomena more conspicuous ; and having drowned them so, that, if let alone, they would not in probability have ever recovered, I found that the heat of the sun would recover them, as well as it has been observed, that warm ashes would recover flies (so that these trials argue celestial heat to be as little more as less vital than elementary) : and the degrees and manner of their recovering again the operations of life suggested observations, not unworthy to be taken notice elsewhere, though not fit to be delivered here ; where I shall not so much as mention what with warm applications we have done, to revive the expired motion of the parts even of perfect and sanguineous animals, when they seemed to have been killed ;



Killed; because I fear that the excursions I have unawares made already will be looked upon as too much a digression.

WHEREFORE, to take up my discourse where a while since I left it, I shall proceed now to observe, that even in a body that has lost its specifick form, the noble qualities that remain do not always flow from the form of the entire body as such, but from the peculiar form of some particular parts of that body, which being separated from it, though perhaps the more stable parts that remain, will keep the visible structure from being manifestly altered, yet this remaining body will be quite deprived of the noble properties we were mentioning; as may be gathered as well from what has been above mentioned out of *Sennertus* about drawing an extract from rhubarb, in which its whole purgative virtue resides, as in some preparations of cinnamon, and divers other substances endowed with fine parts, which upon the loss of those parts remain but the carcasses of what they were. And even in the grois bark of oak, tanners find that when the water has extracted the dissoluble parts, or time has wasted some subtle parts, and changed the texture of the rest, though the bark retains its outward form, they cannot make use of it as they might have done before. And (as I formerly intimated \*) besides this pre-existent and surviving modification, it is in divers cases very possible that new qualities and properties (whose principle may be called a form) may be disclosed upon the abolishing of the specifick form, though they were not actually in any part of the matter, but are produced in it by a concurrence of the texture and dispositions left there by the late form, and the operation of external agents. As when out of the flesh of a dead animal there is generated muck; for not only those seminal rudiments that actually were latent in the corrupted body, gain opportunity to set up for themselves, and become perfect insects, or other creatures of their own kind, but the external agents, to whose action, according to the common course of Providence, deceasing plants or animals happen to be exposed, do oftentimes (not without the foreknowledge of the most wise Author of nature) so agitate the small parts of the widowed matter, and perhaps by associating themselves with them do so alter their texture, and thereby introduce a new modification, that by the conjunction of the former dispositions that were regularly left in the matter with these new agents, promoted by a concurrence of favourable circumstances, there may be produced new and noble forms (however not vital ones). As when a lime-stone, being calcined and left in the open air, will in tract of time, as I have particularly observed, by the assistance of congruous particles it meets with there, and befriended by the more catholick causes or physical mutations, afford true and inflammable salt-petre; and I have seen certain marcasites that, being burnt and exposed to the air in convenient places, would have such a change produced in their parts, as, after a due time, to afford an efflorescence, which both by the colour, taste, and operation appeared to be vitriol.

BUT of such matters no more at present. I will rather take notice, in prosecution of what I was not long since observing about the two-fold modification of living creatures; that I fear we sometimes attribute to the specifick form or soul things that may be well enough performed without it by the more stable modification of the body, befriended by an easy concurrence of natural agents. Thus, though the exclusion of excrements be unanimously ascribed to the soul, which for that purpose is said to be endowed with a peculiar faculty, that they call expulsive; yet it has been observed and affirmed by many, that divers times the excrements have been discharged out of the bodies of men a good while after they were unquestionably dead; so much (it seems) of the former structure of

\* Especially in the 5th and 6th considerations.

the parts remaining, as sufficed to co-operate with the excrements themselves, changed by the death of the animal, to that exclusion. And thus (to add an instance of another kind) though the maturation of fruit be a great, and, as the schools speak, a perfective alteration, which is supposed to be wrought by the vegetative soul of the plant; yet it has been vulgarly observed, that apples and grapes gathered before they be ripe, and laid on heaps together, will ripen well enough afterwards (and the example were more eminent in medlars, if what some call their ripeness, others did not call their rottenness). And very remarkable is that account which the inquisitive *Owiedo* gives the emperor *Charles* the fifth of the *Ananas*, if I mistake not the name; which having mentioned as one of the considerablest fruits he met with in the *West-Indies*, he takes notice that though, notwithstanding their largeness, they grow in clusters, yet they must be gathered whilst all but one are green. For as soon as the first begins to be yellow, the whole cluster must be taken off, leaving the rest to ripen, and attain to the same colour in the chamber, which they will very well do. The learned † *Josephus Acosta* speaks thus of the fruit of the plane-tree, to the same: “ This fruit (says he) inclines more to “ cold than heat.”—They are accustomed to gather the boughs or clusters (as I have said) being green, and put them into vessels, wherein they ripen being well covered, especially when there is a certain herb mingled with it, which serves for this effect. But the diligent *Piso* speaking of those *Brasilian* plants, which he calls *Pacoeira* and *Bananiara*, punctually relates that which comes up yet more fully to our present purpose. For not only speaking of the fruit, he says \*, *continentur plerumque in uno ramo quatuordecem aut sedecem numero, ut ita una planta proferat septuaginta aut octuaginta, qui subinde virides avulsi, nunc in ædibus, nunc in navibus suspenduntur, donec justam maturitatem & flavedinem consequantur*: but adds this memorable passage concerning the lopped boughs themselves; *Ramus autem ille fructibus onustus, interea dum illi maturescunt, augetur, floresque semper protrudit ex corpore illo foliaceo, &c.* On this occasion I might here add, that even in our cold climate, onions and some other bulbous plants will in the spring-time shoot out of their own accord. And I have taken pleasure to keep potatoes in the air, to observe how at that season, when they usually begin to sprout in the ground, they would put forth leaves at so many of the little holes or dimples, as to give themselves a verdant livery: but that not being willing now to examine, whether or how far an animated seed may have an interest in these last mentioned productions, I will rather take notice, that even in animals some things that are confidently presumed to be the proper effects of the animal’s soul, may be really performed by the texture of the body, and the ordinary and regular concourse of external causes. For (not here to repeat what I lately noted of the exclusion of excrements in dead bodies) though the nails of a man are nourished, and do grow as well as other parts; and though the hair in most animals be sometimes, even in determinate parts of the body, peculiar to the species of animals to which it belongs; and though in man hairs do not only grow, but in the disease called the *plica Polonica* it appears to participate of blood (since the hairs being cut, weep out that liquor) yet nails themselves are observed to grow in dead men. And that they do not so only (as is supposed) for a little time, whilst the impressions left by the soul upon the carcass are yet vivid and recent, but for a much longer while than has been imagined, I have been with pleasure informed by a memorable observation I met with in the experienced ‡ *Paræus*, who speaking of a body that he by embalming preserved for more than twenty-five years, he affirms it still remained whole and found,

† *Acosta*, lib. 4. p. 269.\* *Piso*, *Natur. Hist.* cap. 21. pag. 155.‡ *Paræus*, lib. 28.



and that, as to the nails, he found, that *having often pared them, he still observed them to grow again to their former bigness.*

I KNOW the patrons of *Sennertus's* opinion look upon it as a clear and cogent argument, to prove the soul's performing almost all things done in the body, that in the corpse of a man or other animal newly dead, though the organization remain the same, yet all the animal and vital functions perfectly cease. But besides that I have already taken notice, that some things wont to be attributed to the sensitive soul may be observed in a body avowedly dead, I confess, that this argument seems to me, though very specious, yet grounded upon what is but precarious. For though it may be true, that the visible fabrick may continue for a while without any manifest alteration, yet who can assure us that the internal organization is not considerably changed and vitiated? For the body of an animal is an engine, that consists not only of solid and stable parts, as bones, muscles, skin, &c. but of divers soft ones, as the brain, nerves, &c. and of some that are fluid, as the blood and other liquors; and, which is in our case exceedingly considerable, requires a convenient coaptation, or composition of all these: whence it follows, that the external frame of the body remaining unaltered, yet upon death there may be great and sad alterations in the texture of the blood and humours, and the contexture or structure of other internal parts. And these changes may quite spoil the organization of the body, and make it unfit to perform the wonted functions of such an engine. Thus we see, that in dead bodies, even whilst they are warm, the blood oftentimes coagulates in the vessels, whereby the circulation, that grand wheel of life, is stopped. And in sudden palsies, though there be usually no visible change in the affected limb, yet it loses sense, or motion, or both: and not only in syncope or great swoonings, and in apoplexies, a great part of the animal functions are for the time suspended or unperformed; but even in so natural a state as sleep, the body appears not to move, nor do sounds and odours affect the senses, as when one is awaked, though the soul be present in the body, and the ears and nostrils be open. And how great changes in the nature of liquors, and consequently of the blood, may be produced without any visible alteration, may be guessed at by what often happens in wine upon thunder; for that liquor, which was pleasant, spirituous, inflammable before, speedily degenerates into a sour and uninflamable vinegar. Which instance will not, I suppose, appear inconsiderable to those many modern philosophers and physicians, that would have life maintained by a *biolychnium*, or vital flame continually burning in the heart, and fed by the spirituous parts of the circulating blood.

It were not perhaps time mispent to prosecute such inquiries, as we have lately touched. But though I did not want leisure, I should be discouraged by considering, that even some of the things I have already delivered, may be questioned by those who take not the word *life*, and some other terms by me employed, in the same sense that I do. And indeed it is very difficult that men should avoid falling either into mistakes, or into unprofitable disputes, if they discourse largely of such themes, where the names that are of a very common and necessary use, have (yet) their significations very little stated or agreed on. For *life*, for instance, is a word, whose meaning is not yet defined, and is applied to subjects, that are exceeding different; for it is ascribed not only to all sorts of animals and plants, but by many chymists and mineralists to stones and metals growing in the bowels of the earth. Nor is it attributed only to things corporeal, but to those that differ *totò generà*, as they speak, from them; namely, to separate souls, angels good and bad, and to God himself. Nay, what that is even among animals, wherein it consists, is not yet sufficiently agreed on, as may appear by the dispute among the modern naturalists, whether profligate, but as yet unhatched, eggs have life



or no? and whether flies be really dead in winter, which some affirm them to be, not only because those insects seem to be devoid of sense and motion, but because they place the notion of life in a constant circulation of the blood, or some analogous juice, and a distribution of the aliment thereby performed to repair the wastes of the body; whilst others, on the contrary, think them to be rather benumbed than dead, because regularly recovering the manifest actions of life in the spring (or oftentimes before, if a due application of heat be made unto them) it cannot be supposed, that they were during the winter really destitute of life: death being a privation, which, by physical means, admits not of a return to the former state. Nor are the boundaries and differences betwixt the life of a plant and that of an animal so settled and defined, but that divers not impertinent questions may be made about them; and particularly it may be doubted, whether some, as nails, hair, horns, &c. that belong to the body of an animal, may not for all that (even whilst he is alive) have the nature of a plant, to which the part where it grows serves as the stock does to a graft for a soil, and is but an appropriated one. But to do more than point at such matters, would add too much to the digressions, of which I fear the passed discourse may be thought to be guilty already. I shall not therefore add any thing at present further about the subordinate forms of plants and animals: but in regard I ventured, about the beginning of this little tract, to ascribe subordinate forms to divers bodies that never had life, which I doubt will seem a paradox to many, I think it will not be amiss to apply the chief points of our doctrine about subordinate forms to inanimate bodies, because this course, as it will invite me to make some new, though short additions, to illustrate and enlarge some points, so it will help to call to your memory most of the heads of that doctrine, which the several excursions whereto divers subjects tempted me, may have kept you from taking a distinct view of. And for order's sake I shall cast the main things, I would have considered, into distinct propositions, with short comments annexed to them; having only intimated in general once for all, that you will not, I hope, wonder, that I should often use for examples, such bodies as are looked upon as factitious, if you recal to mind what I have formerly said to shew, that the difference betwixt them and those that are confessedly natural, is not always near so great as men are wont to imagine. To which I shall now add, that in the following discourse they are often employed, not so much to prove, as to illustrate the notions on whose occasion they are alledged; which sure they may very properly do. And among the bodies themselves, in whose productions man's power or skill has a share, I reckon that there is a great difference between those, wherein man gives an outward shape, such as himself designs, by tools of his own making, that are always external to the produced body, and those (such as are most chymical productions, besides others) wherein his chief work is to apply physical agents to patients, by which means it oftentimes comes to pass, that (as in productions, that all allow to be natural) the instruments he works by are parts of the matter itself he works upon, or at least intrinsic to it. But of this more perchance elsewhere; I come to the propositions themselves.

*THE word form* is of a sufficiently indeterminate signification.

*THIS* I have already had occasion to shew, that it can scarce be denied by them that shall consider, though it be a technical word or term of art, yet men have not intelligibly defined and agreed, on how many, or what things they are sufficient to intitle a portion of matter to a determinate and distinct form. For besides that there are I know not how many bodies, such as treacle, beer, gun-powder, coal, ink, &c. about which men seem not so much as to have considered whether they ought to have particular forms ascribed to them (or to be looked upon only as factitious things) there are other bodies, that have been taken notice of, about which even the Peripateticks dispute, whether



whether they ought to have particular forms allowed, or no. For not only ice is by some made to be a distinct kind of natural body, whereas others will have it to be only water altered, and thereby deprived of its fluidity, not its form; but even touching the elements themselves the school-men fiercely dispute, a whole party denying them to have any other forms, than the first qualities by which they are wont to be distinguished. If I affected paradoxes, I might here add, that perchance there may be bodies, which, as they may be diversly considered, seem to have a title to more than one form, and upon that score may puzzle the schools about the assignation of their forms. When, for instance, I have (though not without some difficulty) reduced lead *per se* into a body like that which chymists call *vitrum Saturni*, and which they make by the addition of flints sand; and it is not easy to determine whether this shall be one of those kind of bodies that are called metals, and in our instance is only disguised, or belongs to that other kind of bodies we call glass; for it seems to have the properties of both. For, like lead, it is very ponderous, and dissoluble in aqua fortis and spirit of vinegar, which dissolve not common glass; it affords a very sweet solution, as lead is wont to do; and which is more, it may without addition by bare heat be quickly reduced in great part into true and malleable lead. On the other side, it is a body fusible, transparent, and brittle, which are the three grand properties of glass, besides which, I have observed in it some others that will be more properly taken notice of elsewhere. So likewise when mistletoe grows (as I have sometimes seen it) to a very great bigness on a hazle, which (you know) is but a very small tree; or when an apricot or peach is inoculated, and prospers upon the bough of a plum-tree, the rest of whose branches bear plums as they did before (to which I might add some instances of trees that I have seen to bear more kinds of fruit); and when red or blue amel is made, which consists of calcined tin, which they call putty, and of the salt and sand (or fusible stones) whereof the glassmen make what they call their fritta, and of some burnt copper, or some other metalline pigment, most, if not all, of which so differing ingredients may perchance be re-obtained out of the amel, which has divers properties of the respective bodies it consists of, and yet wants others of them: if, I say, such examples as these, to which I could add several others, were proposed, it would perhaps somewhat perplex the schoolmen to accommodate them fairly to the vulgar doctrine of forms, at whose framing probably such instances were not dreamed of.

II. It is not easy to decide the nobleness of forms.

THIS point also has been partly handled already, which will make it the less needful to insist long upon it; and indeed, besides that nobleness is rather a civil or political than a physical qualification, it is oftentimes difficult enough to determine even in this sense of nobleness, which of the two forms is the most noble. Of this difficulty we have already elsewhere given some examples, to which we may add, besides the lately mentioned instance of the glass of lead, that of antimony made *per se*; crude antimony being fitter for several purposes, both mechanical and medical, than the vitrified calx; and this again being better for divers other uses than that which has not been freed from its more fugitive parts.

It seems it was disputable among the ancients, whether or no their electrum (which learned men tell us was a composition of gold and silver) was a nobler thing than either of those metals. And it may be questioned, whether, when chymists have made a precipitate of gold and mercury, the produced powder be a nobler thing than the gold alone. For chymists think it worth while to put themselves to much trouble and some charge to bring gold, by the addition of mercury, to this new state; and therefore if a spagyric physician were judge, he would think, that in such a medicine the gold is improved

by the change; but if a goldsmith were to be judge he would conclude that gold, being the noblest of metals, an alloy must needs imbase it; and he would take the pains, by melting it with borax or some other additament, to free the gold from the quicksilver, and restore it to its pristine form.

It may also be disputed, whether, though in living creatures the ultimate form be wont to be more noble than its previous harbinger, it may not be sometimes otherwise in bodies inanimate, as well as in the productions of art. I will not urge for an example to this purpose, that when corn is ground in a horse-mill, though the whole aggregate, consisting of the horse, the wheels, grindstones, and other parts of the mill, be looked upon as but one engine in reference to the use of the whole, which is comminution of grain; yet the horse, though contained in the mill, and looked on as a part of it, is of a much nobler nature than the engine, to whose effects it co-operates. This example, I say, and others of the like kind (as that of a turnspit-dog, included in a wheel, to make it go round) I shall not press, but rather give this for an instance, that when an artificer, who makes silver fodder, adds to the silver a certain proportion (which I have elsewhere specified) of brass or copper, and melts them together, though he thereby obtains a mixture of good and frequent use for joining together the pieces of brass and silver instruments, and stopping holes and cracks in them, yet it may be much questioned, whether this brittle substance be not less noble than the silver alone was. And when a plant, that grows by some petrifying spring, by imbibing that water is at length turned into a stone; though the rarity of such things makes men prize them, yet it may well be questioned, whether the supervenient form be not less noble than that which the plant had before.

III. In divers bodies the form is attributed upon the account of some one eminent property or use; which, if it be present and continue, though many other things supervene or chance to be wanting, the matter is nevertheless looked upon as retaining its form, and is wont to be allowed its usual denomination.

An example of this we may be furnished with by our lately mentioned instance of *vitrum antimonii*: for the account, upon which we take a body to be a glass, being chiefly fusibleness and transparency, this antimonial preparation, by virtue of those qualities, passes without scruple for glass, and would in effect be taken for yellow or red glass (according as it happens to have more or less of tincture) by an indifferent spectator, who were asked what it is. And yet this substance is not only by its solubility, unfixedness, and disposition to afford a regulus very differing from common glass, but retains so much of an antimonial nature and properties, as to be vomitive and purgative, as well as *crocus metallorum*. It can scarce be unknown to chymists that there is a vast difference between those liquors that are expressed out of olives, almonds, and other unctuous vegetables, and those fine essential oils, as they call them, which are drawn by the help of water in limbecks; and even of these, some are wont to swim upon water, as oil of anniseeds or nutmegs; others to subside, as the oil of cloves and cinnamon, &c. well drawn; and all these essential oils are very differing from the oil of guaiacum or of box, and other empyreumatical oils that are distilled in retorts by the violence of the fire; and these do as much differ from expressed oils as from essential ones: and yet all these so differing liquors are reckoned among oils, because they agree in this, that they are fluid bodies, unctuous to the touch, and mingle not with water. And so although some sorts of salts be very fugitive (as the volatile salts of hartshorn, urine, &c.) others very fixed (as that drawn from the calx of tartar, and from the ashes of wormwood, ash, celandine, and other plants); and though some saline liquors, as vinegar and juice of lemons, are acid, and dissolve pearl, coral, &c. which lixiviate

salts,



salts, whose taste is fiery, will precipitate what the others have dissolved; yet all these are numbered among salts, because they agree in the accounts upon which we allow bodies that denomination; namely, their being very sapid, and readily dissoluble in water. Examples to the same purpose with the foregoing I could give you in flame, smoke, glass, coal, and divers other sorts of bodies. And indeed, by reason of the unsettled notion and almost arbitrary use of the word *form*, I have observed it to be so uncertainly applied to the constituting of the distinct classes or kinds of bodies, that I have doubted whether divers of those forms by which such kinds are constituted, be not a kind of metaphysical conceptions, by virtue of which, bodies very differing in nature are comprized in the same denomination, because they agree in a fitness for some use, or in some other thing that is common to them all; as whether a bullet be silver, or brass, or lead, or cork, if it swing at the end of a string, it is enough to make it a pendulum; and whether a burned body be chalk, or rag-stone (which is very hard and coarse) or alabaster, which is a soft and fine stone, or an oyster-shell, or a cockle-shell, or a piece of coral; yet if it have been calcined to whiteness, it is lime, rather than such true physical forms, as are said to make the bodies that have forms of the same denomination, to be of the same specifick nature. However these forms seem to be very general things, and more such than is commonly heeded. And I have also sometimes questioned, whether some of those things, upon whose score men constitute bodies in this or that species or classis, be so properly the true and intrinsic forms of those bodies, as certain states of matter, wherein bodies very differing in nature may agree. As water, wine, and I know not how many other differing liquors, may each of them apart be made, by congelation, to pass into that sort of body we call ice. And not only the tallow and grease of animals, and the expressed oils and spirits of fermented vegetables (some whereof differ exceedingly among themselves) but also (as I have tried) divers mineral and even metalline concretes may be made (some of them without destruction of their nature) to pass into that classis of body we call flame.

IV. By reason of the conjunction or connexion of the parts that make up a *totum* (or at least an aggregate of bodies that, for their connexion, are looked upon as such) it will often happen that several things will be performed by the joint or concurrent action of these united (or coherent) parts.

THESE kinds of operations are of kin to those mentioned by the schoolmen, when they tell us that some things are done by divers agents *actione communi*; as when a man disputes *vivâ voce* syllogistically, the rational soul, which dictates the words, and the vocal organs that pronounce them, are *actione communi* the efficient of the pronounced syllogism. But to give an instance nearer to our purpose; when a bullet is let fall upon a level pavement, though it touches the body it falls on but in a very small part of its superficies (geometricians having demonstrated that a perfect sphere can touch a perfect plane but in one point); yet the plane receives the action of the gravity of the whole body; those parts that do not immediately come to touch it, striking it nevertheless by the intervention of those that do. And so likewise in a boat, the limbs and clothes of a man that stands upon the deck, and all the parts of a watch, if he carry one in his pocket, gravitate concurrently on the vessel, though only the soles of his feet or shoes do immediately press upon it, and the wheels and other parts of the watch may be moving at the same time very differing ways. Now in organical bodies, and divers others, both natural and factitious, those things that are performed by the parts as in a state of conjunction, and, as it were, conspiring, or (if you will have it so expressed) *actione communi* (which action may sometimes be successive) are oftentimes ascribed to the form: as in a watch, most of the chief of the phenomena do so depend upon the

concurrent action of the several parts, that few of them can be out of order, but that they will hinder those phenomena to be at all, or at least to be well and regularly produced.

V. WE may yet in a sound sense admit, that in some bodies there may be subordinate forms.

WHAT I mean by a *sound sense* in this proposition may be clearly collected from several passages of the past and remaining parts of this discourse, where we carefully exclude those senses, in which we do not allow the received doctrine of subordinate forms. Wherefore, having met with a couple of plausible objections started since the death of *Sennertus*, against the admitting them at all, we shall here briefly examine them, not only to make them appear not to be cogent, but because some of our answers may serve for reasons why we admit the forms disputed of.

THE first argument we are to consider, is, that a body can have but one form, being but one body.

BUT though to this I might frame an answer from the loose and indeterminate signification of the word *form*, yet it may be directly replied, that though a body can have but one total and adequate form, yet nothing hinders but that its parts may have their partial forms subordinate to that: as the steel-spring and the brass-wheels of a watch may retain their distinct metalline forms though the watch they compose be but one. And it is not wholly to be premitted on this occasion, that among the schoolmen themselves there has long been a considerable party who asserted with many of the ancients, that in compounded bodies the elements retained their respective forms notwithstanding the new form that belonged to the mixed body as such.

SECONDLY, it is objected against the supervening of a higher form, that a body being already complete in its own kind by its own form, no other form can accrue to it without making that which they call *Ens per accidens*.

TO this I answer, that the notion of an *Ens per accidens* belongs rather to metaphysics than natural philosophy; and in what its essence consists, is still so hotly disputed among the moderns, that till the business be agreed on, or at least more clearly stated, an argument drawn from thence will not much press us. And indeed when I consider that the schools themselves are fain to allow the soul and the body, that is, an immaterial substance and a corporeal, to make up a man, who according to them is *unum per se*, and not *per accidens*, and that the same schools scruple not to teach, that the rational soul, which is a substance, and the understanding and will, which are said to be its faculties, and so its accidents, to make *unum per se*; I cannot but think, that, by a parity of reason, that name, predicate, or qualification, may well be taken in as large a sense as is requisite for our purpose. And indeed if the parts of a body, whether merely natural or factitious, be by their union or conjunction brought to become the principle of a property or operation, which belongs to neither of them single; I see not why such a body may not pass for *unum per se*, as well as divers bodies that are wont to be looked upon under that notion. But to proceed to our further answer, not here to urge that a whole sect of the Peripateticks themselves maintained (as we newly noted) the forms of the elements to remain in the mixed body, which notwithstanding they hold to be very consistent with the unity or oneness of that compounded body, it may be answered further, that though a body by its own form be complete in its own kind, yet it may be such, as to be capable of being advanced to a nobler state by an accession that shall not ruin its former nature, but enable it to cooperate to nobler actions than its former could reach to. As when a spring is made part of a watch, it does thereby, without losing the nature of a spring, mainly contribute to the noblest phenomena of so curious an engine; and



and the ingredients of gun-powder, by the superinduction of the form or new contexture they acquire by being compounded into that concrete, are each of them enabled to cooperate to the performance of things far exceeding the utmost it could do before. Nor will it follow from this superinduction of forms, that there may be a form of a form as well as of matter, but only that to a body that has already a form, an ulterior form may supervene, wherein we see no absurdity. But of this point more elsewhere; only in this place it will not be amiss to take notice, that in our proposition we thought to employ the words, *some bodies*, and, *may be*, because that though in living creatures we may often meet with subordinate forms either properly or less improperly so called, yet that in bodies inanimate this happens not so often, you will be induced to think by what you will find said upon the last proposition of this discourse.

VI. THE supervening of the new form is often but accidental to the præexistent form, and (then) does not at all destroy its nature, but modify its operations.

For illustration sake, let us consider a needle that is not yet touched by a loadstone: this needle has its own form as a piece of steel, as well as its figuration as a needle; but when afterwards it comes to be excited by the loadstone, there are then new and wonderful properties superinduced, and this needle is able to point regularly north and south, and attract other needles, and communicate a verticity to them, and is fit for much nobler uses than it was before. And this new modification does so regulate its motions, that whereas before it was indifferent, if it were nicely poised, to rest at east and west, or at any other point of the compass, it is now determined to keep moving till it points north and south, and to rest in that position. And yet by drawing this magnetick needle after a certain manner upon the pole of a vigorous loadstone, you may in a trice deprive it of all its accessorial faculties; notwithstanding which it will remain as true a steel-needle as it was first.

AND perhaps we shall need to add but a little reflection on the formerly mentioned instance of the spring of a watch, to declare intelligibly what it is, that the structure or modification, whence the *forma totius* according to us results, does to a body endowed already with its own form. For as the spring, though it retains its own nature, and acts according to it, yet by the contrivance of the watch, it is not only so pent in, that it cannot fly out to its full extent as else it would, but by the same contrivance has its incessant endeavour to stretch itself so moderated and managed by the wheels and balance, that it mainly concurs to set all the other parts a moving, and perform what is done by a watch as such: so in natural bodies, that which is performed by the supervening of a higher and total form is, that by virtue of the connection and structure of the parts introduced with this new form, the action of the particular parts, though they retain their own partial forms, and act as far as they can, according to them, is so mastered or otherwise modified, that they are brought to concur to those things that are done by the whole body as one agent, and become subservient to the operations that are proper to the body in its new and ultimate capacity. So when a piece of lead is without addition vitrified by the mere action of the fire, this happens to the body upon its acquiring the form of glass; that whereas before the metalline particles were so inconveniently situated, and perhaps shaded, that they denied passage to the beams of light, and by reason of their contexture composed a body that was very flexible, they become now to be so ranged and otherwise altered, that they freely admit the light to traverse them, but admit not of being freely bent as before. And when salt-petre, by the addition of a small proportion of brimstone and coal, is made into gun-powder, this accrues to it from its acquired modification, that if a little fire fall on it, it will not, as before, leisurely consume, and leave behind it a considerable portion of the whole body (perhaps a third part or more)

in

in the form of a fixed or alkalizate salt, but will fly away all at once, and leave little or nothing behind it.

VII. BESIDES the specifick actions of a body that harbours subordinate forms, there may be divers others, wherein some of the parts or ingredients may act according to their particular and pristine nature.

THIS might be well enough gathered from what we lately delivered, when we shewed that the total and specifick form has not such a dominion over the partial and subordinate ones, as the patrons of these forms have imagined. For though, by virtue of the modification of the whole, the operations of the parts or ingredients are so compounded and guided, and in some cases, as it were, over-ruled, as to concur to those operations that belonged to the *totum* as such, and are requisite to be performed *actione communi*; yet in other respects, and as to other purposes, as it is not necessary that such bodies as we speak of should have their parts entirely under the dominion of the ultimate form (that is subservient only to the operations and uses of the *totum*, as such); so those parts may in such cases act according to their distinct and particular qualifications. This answer, I say, may be deduced from what has been above delivered; but I chuse rather to clear the matter by two or three particular instances, which may shew that the same may happen to several bodies that is manifest in a watch, where, though the form of the engine do in many things make the spring and other parts concur to perform the operations proper to such an engine, yet the wheels may look bright and yellow, the spring may move a magnetick needle freely placed, and other parts may do other things, not by virtue of the form of the watch, but by virtue of their own qualities. An example to this purpose may be afforded, by what I remember I have not long since mentioned concerning gun-powder. To which I shall now add, that whereas in pills and divers other medicines made up of several ingredients, the *compositum* has, if the physician do his part well, some resulting virtues distinct from those of the ingredients, and belonging to the *compositum* as such; it may oftentimes happen, that notwithstanding the emergent form of the compounded medicine, some particular ingredient may not only retain its former nature, but so retain it, that the *compositum* is endowed with that quality only upon the score of that ingredient. This I have divers times observed in certain pills wherein good ambergrease, being mingled with some purgative ingredients, retained its own grateful smell, and communicated it to the whole mass whereof the pills are made: and the most sort of purging pills in our apothecaries shops taste strong of the aloes, whatever the other ingredients be. And a further instance (and that a considerable one) we may take from treacle that hath not been too long kept. For though it be acknowledged that opium works by a specifick, and, as they call it, occult quality, and though it be in (*Venice*) treacle blended with above threescore other ingredients, most of which enter that famous composition in far greater quantity than does the opium; yet in spite of the *forma compositi*, which so elaborate a mixture produces, and to which such great peculiar virtues are ascribed by Peripatetick as well as other physicians, yet it is noted by many, that before treacle grows old it manifestly derives an opiate quality from the little opium admitted in it, and upon that account is a potent remedy in fluxes, and divers other distempers where quieting medicines are proper. A no less evident example to our purpose we have in the precipitate of gold and mercury made by heat alone. For though by virtue of the union of the ingredients the resulting powder may have divers qualities, as particularly, a red colour, which neither the gold nor the quicksilver had apart; yet the salivating faculty, which this precipitate usually, though not always, exercises, though it be reckoned among occult qualities, as not having by any been deduced from the first or second, yet it belongs to this medicine barely upon the account



of the mercurial ingredient; the gold, without that, having no such faculty, and mercury alone without gold being sufficient (by more ways than one of application) to cause a flux of spittle.

VIII. In divers bodies that which is called or looked upon as the specifick form, is often not so much as the presiding, but only the eminentest.

To make out this, we may take notice of the following particulars. 1st, We observed above, that the word *form* has not a settled and determinate signification, but it is employed arbitrarily enough; so that divers bodies, to whom particular forms are used to be assigned, deserve not that privilege better than many other, in which (perhaps for want of men's having particularly considered them) they are not wont to consider any peculiar and distinct form. 2dly, We have also elsewhere shown that the forms of inanimate bodies (which we here speak of) are wont to be but respective things resulting from the co-existence of such corpuscles or parts after such a determinate manner. 3dly, It may likewise be remembered, what we have already noted, that it is usually from some particular respect, or for a fitness to some particular use, that men ascribe this or that form to this or that body; as we exemplified in oils, salt, &c. as well as in watches, burning-glasses, and the like. 4thly, To these things it will be agreeable, that the nature and fabrick of a body may be such, that it may have a manifold structure (if I may so speak) answerable to more than one of those respects, on whose score bodies are denominated, or may be fit for more than one of those uses; an aptitude for which, when it is found single in another body, is sufficient to make it be referred to this or that distinct kind or class of things corporeal. I cannot in few words express this notion more clearly, and therefore shall illustrate it by the example of antimonial glass: for one that would make beads or microscopes with it would readily find in it subtileness and transparency; which, when they are found in common glass or *vitrum Saturni*, are enough to refer them to that sort of bodies that are comprehended under the name of glass. But besides this combination, or (if many convene) this conjugation of qualities, or (to express it in one word) besides this modification, the body we speak of has another, upon whose account it is yet to work upwards and downwards in a human body; upon which score, as the artificer considers it only as glass, so the chymist and physician look upon it as a medicine. 5thly, Nor is it necessary that these conjugations of qualities, or (these) modifications, should have a strict dependency upon one another; as for instance, the emetick and cathartick properties of the antimonial glass belong not to it as glass, or (if you please) do not flow immediately from the form it hath of glass; for neither has common glass, nor (that we know of) glass of lead any such properties, nor is it necessary, that if this very portion of matter had not the form of glass, it should want or lose these properties; for the calx of antimony, before its vitrification, had them; and you may, even without addition, obtain from this glass a regulus, that is not, like glass, transparent, but looks like a metalline body, under which form it may yet preserve the virtues of the calcined antimony. 6thly, To these things it will also be congruous, that since, as was said above, the nobleness or ignobleness of forms is not easy to be decided, and is wont to be measured by men, by the greater or lesser use, that the estimated body affords them, one man may in the same body look upon one kind of modification, and another upon a quite differing one, as the highest form of that body. As in the lately mentioned example of the melted calx of antimony, an artificer may think its noblest form to be that of glass, and a chymist or a physician that of antimony. And so if an ordinary watch, that shows only the hours and their quarters, being hung at a string, were made to swing as a pendulum, to an astronomer or some other that were to make nice observations, it would be most useful in the capacity of a pendulum, because,

because, as that, it may divide a minute into seconds, and a second itself into half or fourth parts; but for other men, who, though they need an instrument to measure time, need not such minute subdivisions of it, the little engine we speak of will be much more useful and considerable in the capacity of a watch than of a pendulum. 7thly, From all which particulars it will be reasonable to collect, that it may often happen in inanimate bodies, whether confessedly natural, or such as are called factitious, that that which is looked upon as the ultimate, or at least the chief form, is not the presiding, but only the eminentest: by which I mean, not simply the noblest (for that were hard to determine, and according to men's estimate would not be always true) but that, which in that body is at least for the time the most considered; or, if that expression will please better, we may say that sometimes the most regarded form is not so much the predominating as the denominating form.

IX. THE lately mentioned forms seem to be rather concurrent than subordinate.

THIS, as I was saying, follows well enough from what has been freshly discoursed. For if a body may have divers such conjugations of accidents or modifications, as may intitle it in differing respects to differing forms, and that form, which is considered as the eminentest, be not a presiding form, nor so much as always the noblest; what will remain but that these forms (for I have granted above that some bodies may have subordinate ones) that happen to coexist in the same body, be more fitly termed concurrent or coincident than subordinate?

AND indeed, though I cannot now stay to examine how far what I shall say may be applied to bodies in general, I confess that, as to inanimate bodies, this dominion and subjection, that is imagined between forms, seems to me, at least in many cases, neither well established, nor easy to be well explicated. And I doubt that sometimes we mistake names for things; and because when a body, by the action of proper agents, obtains such a modification as fits it for such and such actions and uses, we are wont to call it by such a name, and attribute a form to it, we are prone to conclude that the faculties and qualifications it enjoys, and the things it is able to perform, are due to this form we have assigned it; as if this form were some distinct and operative substance that were put into the body as a boy into a pageant, and did really begin, and guide, and overrule the motions and actions of the *compositum*. Whereas indeed what we call the form, if it be not sometimes little more than one of those airy things that schools call an external denomination, seems oftentimes to be rather a metaphysical conception in our mind than a physical agent that performs all things in the body it is ascribed to: as when a conveniently shaped piece of steel is, by having a due temper given it, turned into the spring of a watch, not only the motions of the watch, though proceeding from this spring, proceed not from the form of the iron (for a spring made of another elastic body, though it would not be so convenient, might set a watch a moving) but, which is here the main observable, the springiness itself flows not immediately from the form (for steel is not less steel, when it is not springy than when it is) but from the mechanical and adventitious texture that is superinduced in the metal, and may be given it by several outward agents, as the fire, the hammer, &c. And it is so far from being evident, that in bodies inanimate and compounded the eminentest and most considered form must have a dominion over, and an efficacy in all operations and actions of the *compositum*, that even in bodies not so compounded it is not always necessary that the specific form should have so much as a concurrent stroke in what is performed; for external agents may introduce such qualities into the body we speak of, as, being once there, will suffice for actions and productions suitable to their own nature, whether the form be active in assisting them or no. We see that boiling water taken off the fire

will



will raise blisters on one's hands, and dress meat, and perform other things wont to be the effects of the fire, only by virtue of the adventitious heat it has received, though, according to the Peripateticks, the form of water, which is an element naturally cold as well as moist, ought rather to oppose than further the action of the preternatural heat. Another example to the same purpose may be given in the operations of a heated iron taken from the fire (nay, though that be quite put out) to which divers other instances might be added. I know it may be pretended in favour of the schools, that it is the fire that was got in, and yet remains in the iron, that was the cause of these effects. But besides that this subterfuge would involve the makers in very perplexing difficulties, I will, to prevent the allegation, put a case, where it cannot be pretended, by supposing the iron to be heated not by the fire, but by forcible strokes between a hammer and an anvil, both of them actually cold. When a piece of silver is, by being hammered or drawn into wire, made to be a springy body, it will be able to act many things by that acquired elasticity, which do not at all flow from the form peculiar to the metal. For not only copper, steel, and many other bodies may be made springy too; but, if you heat it in the fire the goldsmith will assure you that it is as true and as good silver as before, and yet it will cease to be a spring. And so when a smith makes a file, by making in it many little impressions across one another, and afterwards hardening the steel, by virtue of this roughness which is given it by external agents, it acquires a durable asperity, upon whose account it is qualified to perform many and considerable things, whereto the form of the metal as such does not, that appears, concur. And though the hardness contribute to the making a good file, yet not only the iron was as true and perfect iron before it became rough, as afterwards; but even that degree of hardness, which qualifies our instrument to be a good file, flows not immediately from the form of the metal, for that was true iron, when it was soft, and its eminent degree of hardness was (as I freshly intimated) given it by the temper it received from the smith. I could easily increase the number of these instances, if it were necessary; and I should here add some examples to shew, that even in occult qualities, which are so generally presumed to flow from the specifick form of a body, it is not always necessary that this form should have any great interest (or perhaps any at all) in the operations; but that the matter need but be duly excited and disposed by outward agents to be enabled to perform them; this, I say, I should here make probable, were it not that such instances do more properly belong to our notes about particular, and especially about occult qualities.

But that I may at length conclude this discourse, I shall now in the close, as I have done in some of the passages of it, complain that the uncertain signification and use of terms, wont to be employed about the points I have been handling, are apt to occasion much darkness and difficulty in our inquiries into the things themselves; and I am apt to think that if the meaning of the words *form, life, soul, animal, vegetative*, and some few other terms, were clearly defined and agreed on, a great part of the perplexing controversies that are agitated about subordinate forms, and points relating unto them, would appear to be disputes about words or terms. And I am not sure but that some parts of the passed discourse would not be looked upon as of the same kind too, not out of choice, but a necessity imposed upon me, by the nature of my design, that I was drawn to meddle with any controversy that I think may hereafter look like a verbal one; so if I have not missed my aim, I have both discovered some errors and deficiencies in the received doctrine I took upon me to consider, and contributed something towards the future establishing of a clearer, as well as truer theory about these matters.

## A way of preserving BIRDS taken out of the EGG, and other small FOETUSES.

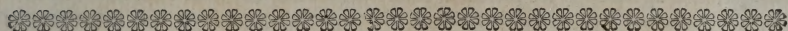
First printed in the *Philosophical Transactions*, N<sup>o</sup> XII. p. 199.

For Monday, May 7, 1666.

THIS was imparted in a letter, as follows: The time of the year invites me to intimate to you, that among the other uses of the experiment I long since presented the society of preserving whelps taken out of the dam's womb, and other *foetuses*, or parts of them, in spirit of wine; I remember I did, when I was solicitous to observe the process of nature in the formation of a chick, open hen's eggs, some at such a day, and some at other days after the beginning of the incubation; and carefully taking out the embryos, embalmed each of them in a distinct glass (which is wont to be carefully stopp'd) in spirit of wine; which I did, that so I might have them in readiness to make on them, at any time, the observations I thought them capable of affording; and to let my friends at other seasons of the year see, both the differing appearances of the chick at the third, fourth, seventh, fourteenth, or other days, after the eggs had been sat on, and (especially) some particulars not obvious in chickens that go about; as the hanging of the guts out of the abdomen, &c. How long the tender embryo of the chick soon after the *punctum saliens* is discoverable, and whilst the body seems but a little organized gelly, and some while after that, will be this way preserved, without being too much shrivell'd up, I was hindered by some mischances to satisfy myself: but when the foetuses I took out were so perfectly formed as they were wont to be about the seventh day, and after, they so well retained their shape and bulk, as to make me not repent of my curiosity; and some of those, which I did very early this spring, I can yet shew you. I know I have mentioned to you an easy application of what I, some years since, made publick enough; but not finding it to have been yet made by any other, and being persuaded by experience, that it may be extended to other foetuses, which this season (the spring) is time to make provision of, I think the advertisement will not seem unseasonable to some of our friends; though being now in haste, and having in my thoughts divers particulars relating to this way of preserving birds taken out of the egg, and other small foetuses, I must content myself to have mentioned that which is essential, leaving divers other things, which a little practice may teach the curious, unmentioned. Notwithstanding which, I must not omit these two circumstances; the one, that when the chick was grown big, before I took it out of the egg, I have (but not constantly) mingled with the spirit of wine a little spirit of sal armoniac, made



made (as I have \* elsewhere delivered) by the help of quick lime: which spirit I chuse, because, though it abounds in a salt not sour, but urinous, yet I never observed it (how strong soever I made it) to coagulate spirit of wine. The other circumstance is, that I usually found it convenient to let the little animals I meant to embalm lie for a little while in ordinary spirit of wine, to wash off the looser filth that is wont to adhere to the chick, when taken out of the egg; and then, having put either the same kind of spirit or better upon the same bird, I suffered it to soak some hours (perhaps some days, *pro re nata*) therein, that the liquor, having drawn, as it were, what tincture it could, the foetus being removed into more pure and well dephlegmed spirit of wine, might not discolour it, but leave almost as limpid, as before it was put in.



# An Account of a new kind of BAROSCOPE, which may be called STATICAL; and of some Advantages and Conveniences it hath above the MERCURIAL.

First printed in the *Philosophical Transactions*, No. XIV. p. 256.

For Monday, July 2, 1666.

AS for the new kind of baroscopes, which, not long ago † I intimated to you, that my haste would not permit me to give you an account of; since your letters acquaint me that you still design a communicating to the curious as much information as may be, in reference to baroscopes, I shall venture to send you some account of what I did but name (in my former letter) to you.

THOUGH by a passage you may meet with in the 19th and 20th pages of my thermometrical experiments and thoughts you may find that I did some years ago think upon this new kind of baroscope; yet the changes of the atmosphere's weight not happening to be then such as I wished, and being unwilling to deprive myself of all other use of the exactest balance ‡ that I or perhaps any man ever had, I confess to you that successive avocations put this attempt for two or three years out of my thoughts; till afterwards returning to a place, where I chanced to find two or three pair of scales I had left there, the sight of them brought it into my mind; and though I were then unable to procure

\* In the Usefulness of Experimental Philosophy.

† See No. XI. p. 185. Phil. Transactions.

‡ The scales here meant were before competent eye-witnesses made to turn manifestly with the thousandth part of a grain.

exacter, yet my desire to make the experiment some amends for so long a neglect, put me upon considering, that if I provided a glass-bubble, more than ordinary large and light, even such balances, as those might in some measure perform, what I had tried with the strangely nice ones above-mentioned.

I CAUSED then to be blown at the flame of a lamp, some glass-bubbles, as large, thin, and light, as I could then procure; and chusing among them one that seemed the least unfit for my turn, I counterpoised it in a pair of scales, that would lose their equilibrium with about the 30th part of a grain, and were suspended at a frame. I placed both the balance and the frame by a good baroscope, from whence I might learn the present weight of the atmosphere. Then leaving these instruments together; though the scales being no nicer than I have expressed, were not able to shew me all the variations of the air's weight, that appeared in the mercurial baroscope, yet they did what I expected, by shewing me variations no greater than altered the height of quick-silver half a quarter of an inch, and perhaps much smaller than those: nor did I doubt, that, if I had either tender scales, or the means of supplying the experiment with convenient accommodations, I should have discerned far smaller alterations of the weight of the air, since I had the pleasure to see the bubble sometimes in an equilibrium with the counterpoise; sometimes, when the atmosphere was high, preponderate so manifestly, that the scales being gently stirred, the cock would play altogether on that side at which the bubble was hung; and at other times (when the air was heavier) that which was at the first but the counterpoise, would preponderate, and, upon the motion of the balance, make the cock vibrate altogether on its side. And this would continue sometimes many days together, if the air so long retained the same measure of gravity; and then (upon other changes) the bubble would regain an equilibrium, or a preponderance: so that I had oftentimes the satisfaction, by looking first upon the statical baroscope (as for distinction's sake it may be called) to foretel, whether in the mercurial baroscope the liquor were high or low. Which observations, though they hold as well in winter, and several times in summer (for I was often absent during that season) as the spring, yet the frequency of their vicissitudes (which perhaps was but accidental) made them more pleasant in the latter of these seasons.

So that the matter of fact having been made out by variety of repeated observations, and by sometimes comparing of those new baroscopes together, I shall add some of these notes about this instrument which readily occur to my memory, reserving the rest to another opportunity.

AND first, if the ground on which I went in framing this baroscope be demanded, the answer in short may be; 1. That though the glass-bubble, and the metalline counterpoise, at the time of their first being weighed, be in the air, wherein they both are weighed, exactly of the same weight, yet they are nothing near of the same bulk; the bubble, by reason of its capacious cavity (which contains nothing but air, or something that weighs less than air) being perhaps a hundred or two hundred times (for I have not conveniency to measure them) bigger than the metalline counterpoise. 2. That according to a hydrostatical law (which you know I have lately had occasion to make out) if two bodies of equal gravity, but unequal bulk, come to be weighed in another medium, they will be no longer equiponderant; but if the new medium be heavier, the greater body, as being lighter in specie, will lose more of its weight than the lesser and more compact; but if the new medium be lighter than the first, then the bigger body will out-weigh the lesser: and this disparity, arising from the change of mediums, will be so much the greater, by how much the greater inequality of bulk there is between the bodies formerly equiponderant. 3. That laying these two together, I considered that



it would be all one, as to the effect to be produced, whether the bodies were weighed in mediums of differing gravity, or in the same medium, in case its (specifick) gravity were considerably altered; and consequently, that since it appeared by the baroscope that the weight of the air was sometimes heavier and sometimes lighter, the alterations of it, in point of gravity, from the weight it was of at first counterpoising the bubble of it, would unequally affect so large and hollow a body as the bubble, and so small and dense a one as a metalline weight: and when the air, by an increase of gravity, should become a heavier medium than before, it would buoy up the glass more than the counterpoise; and if it grew lighter than it was at first, would suffer the former to preponderate. (The illustrations and proof can scarce be added in a few words; but if it be desired, I may, God permitting, send you them at my next leisure). And though our *English* air be about a thousand times lighter than water, the difference in weight of so little air, as is but equal in bulk to a bubble, seemed to give small hopes that it would be sensible upon a balance; yet by making the bubble very large and light, I supposed and found the event I have already related.

SECONDLY, The hermetically sealed glass-bubble I employed, was of the bigness of a somewhat large orange, and weighed about 1 drachm and 10 grains. But I thought it very possible, if I had been better furnished with conveniences (wherein I afterwards found I was not mistaken) to make among many, that might be expected to miscarry, some that might be preferable to this, either for capacity or lightness, or both; especially if care be taken, that they be not sealed up whilst they are too hot. For though one would think that it were advantageous to rarify and drive out the air as much as is possible, because in such sealed bubble the air itself (as I have elsewhere shewn) has a weight; yet this advantage countervails not the inconvenience of being obliged to increase the weight of the glass, which when it includes highly rarified air, if it be not somewhat strong, will be broken by the pressure of the external air, as I have sufficiently tried.

THIRDLY, I would have tried whether the dryness and moisture of the air would in any measure have altered the weight of the bubble, as well as the variation of gravity produced in the atmosphere by other causes; but the extraordinary constant absence of fogs kept me from making observations of this kind; save that one morning early, being told of a mist, I sent to see (being myself in bed) whether it made the air so heavy as to buoy up the bubble; but did not learn that that mist had any sensible operation on it.

FOURTHLY, By reason of the difficulties and casualties that may happen about the procuring and preserving such large and light bubbles as I have been lately mentioning, it may in some cases prove a convenience to be informed, that I have sometimes, instead of one sufficiently large bubble, made use of two that were smaller. And though a single bubble of competent bigness be much preferable, by reason that a far less quantity and weight of glass is requisite to comprize an equal capacity, when the glass is blown into a single bubble, than when it is divided into two; yet I found that the employing of two instead of one did not so ill answer my expectations, but that they may for a need serve the turn instead of the other; than which they are more easily to be procured. And if the balance be strong enough to bear so much glass without being injured, by employing two or a greater number of larger bubbles, the effect may be more conspicuous, than if only a single bubble (though a very good one) were employed.

This instrument may be improved by divers accommodations. As

## An Account of a new Kind of Baroscope,

FIRST, There may be fitted to the anse (or cheeks of the balance) an arch (of a circle) divided into 15 or 20 degrees (more or less) according to the goodness of the balance) that the cock resting over against these divisions, may readily and without calculation shew the quantity of the angle, by which, when the scales propend either way, the cock declines from the perpendicular, and the beam from its horizontal parallelism.

SECONDLY, Those that will be so curious may, instead of the ordinary counterpoise (of brass) employ one of gold, or at least of lead, whereof the latter being of equal weight with brass, is much less in bulk, and the former amounts not to half its bigness.

THIRDLY, These parts of the balance that may be made of copper or brass, without any prejudice to the exactness, will, by being made one of those metals, be less subject than steel (which yet, if well hardened and polished, may last good a great while) to rust with long standing.

FOURTHLY, Instead of the scales, the bubble may be hung at one end of the beam, and only a counterpoise to it at the other, that the beam may not be burthened with unnecessary weight.

FIFTHLY, The whole instrument, if placed in a small frame, like a square lanthorn with glass-windows, and a hole at the top for the commerce of the internal and external air, will be more free from dust, and irregular agitations; to the latter of which it will otherwise be sometimes incident.

SIXTHLY, This instrument being accommodated with a light wheel and an index (such as have been applied by the excellent Dr. *Christopher Wren* to open weather-glasses, and by the ingenious Mr. *Hook* to baroscopes) may be made to shew much more minute variations than otherwise.

SEVENTHLY, And the length of the beam, and exquisiteness of the balance, may easily, without any of the foregoing helps (and much more with them) make the instrument far exacter than any of those I was reduced to employ. And to these accommodations divers others may be suggested, by a farther consideration of the nature of the thing, and a longer practice.

THOUGH in some respects this statical baroscope be inferior to the mercurial; yet in others it has its own advantages and conveniences above it.

AND, 1. It confirms *ad oculum* our former doctrine, that the falling or rising of the mercury depends upon the varying weight of the atmosphere; since in this baroscope it cannot be pretended, that a *fuga vacui*, or *funiculus*, is the cause of the changes we observe. 2. It shews not only that the air has weight, but a more considerable one, than some learned men, who will allow me to have proved, it has some weight, will admit; since even the variation of weight in so small a quantity of air, as is but equal in bulk to an orange, is manifestly discoverable upon such balances as are none of the nicest. 3. This statical baroscope will oftentimes be more parable than the other: for many will find it more easy to procure a good pair of gold-scales, and a bubble or two, than a long cane sealed, a quantity of quicksilver, and all the other requisites of the mercurial baroscope; especially if we comprise the trouble and skill that is requisite to free the deserted part of the tube from air. 4. And whereas the difficulty of removing the mercurial instrument has kept men from so much as attempting to do it, even to neighbouring places; the essential parts of the scale-baroscope (for the frame is none of them) may very easily in a little room be carried, whither one will, without the hazard of being spoiled or injured. 5. There is not in statical baroscopes, as in other, a danger of uncertainty as to the goodness of the instruments, by reason that in those the air

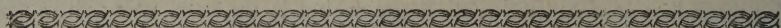


is, in some more, and in some less perfectly excluded; whereas in those, that consideration has no place. (And by the way, I have sometimes, upon this account, been able to discover by our new baroscope, that an esteemed mercurial one, to which I compared it, was not well freed from air). 6. It being, as I formerly intimated, very possible to discover hydrostatically both the bigness of the bubble, and the contents of the cavity, and the weight and dimensions of the glassy substance (which together with the included air make up the bubble) much may be discovered by this instrument, as to the weight of the air absolute or respective. For, when the quicksilver in the mercurial baroscope is either very high, or very low, or at a middle station between its greatest and least height, bringing the scale-barometer to an exact æquilibrium (with very minute divisions of a grain) you may, by watchfully observing, when the mercury is risen or fallen just an inch, or a fourth, or half an inch, &c. and putting in the like minute divisions of a grain to the lighter scale, till you have again brought the balance to an exquisite æquilibrium; you may, I say, determine what known weight in a statical baroscope answers such determinate altitudes of the ascending and descending quicksilver in the mercurial. And if the balance be accommodated with a divided arch, or a wheel and index, these observations will assist you for the future to determine readily, by seeing the inclination of the cock or the degree marked by the index, what pollency the bubble hath, by the change of the atmosphere's weight, acquired or lost. Some observations of this nature I watchfully made, sometimes putting in a  $64^{\text{th}}$ , sometimes a  $32^{\text{d}}$ , sometimes a  $16^{\text{th}}$ , and sometimes heavier parts of a grain, to the lighter scale. But one that knew not for what uses those little papers were, coming to a window where my baroscopes stood, so unluckily shook them out of the scales, and confounded them, that he robbed me of the opportunity of making the nice observations I intended, though I had the satisfaction of seeing that they were to be made. 7. By this statical instrument we may be assisted to compare the mercurial baroscopes of several places (though never so distant) and to make some estimates of the gravities of the air therein. As if, for instance, I have found by observation, that the bubble I employ (and one may have divers bubbles of several sizes, that the one may repair any mischance that may happen to another) weighed just a drachm, when the mercurial cylinder was at the height of  $29\frac{1}{2}$  inches (which in some places I have found a moderate altitude); and that the addition of the  $16^{\text{th}}$  part of a grain is requisite to keep the bubble in an æquilibrium, when the mercury is risen an  $8^{\text{th}}$ , or any determinate part of an inch above the former station: when I come to another place, where there is a mercurial barometer, as well freed from air as mine (for that must be supposed) if taking out of my scale instrument, it appear to weigh precisely a drachm, and, the mercury in the baroscope there stand at just  $29\frac{1}{2}$  inches, we may conclude the gravity of the atmosphere not to be sensibly unequal in both those two places, though very distant. And though there be no baroscope there, yet if there be an additional weight, as for instance, the  $16^{\text{th}}$  part of a grain requisite to be added to the bubble, to bring the scales to an æquilibrium, it will appear that the air at this second place is, at that time, so much heavier than the air of the former place was when the mercury stood at  $29\frac{1}{2}$  inches.

BUT in making such comparisons we must not forget to consider the situation of the several places, if we mean to make estimates not only of the weight of the atmosphere, but of the weight and density of the air. For though the scales will shew (as has been said) whether there be a difference of weight in the atmosphere at the two places; yet if one of them be in a vale or bottom, and the other on the top or some elevated part of a hill, it is not to be expected that the atmosphere in this latter place should gravitate as much.

much as the atmosphere in the former, on which a longer pillar of air does lean or weigh.

AND the mention I have made of the differing situation of places, puts me in mind of something that may prove another use of our statical baroscope, and which I had thoughts of making trial of, but was accidentally hindered from the opportunity of doing it; namely, that by exactly poising the bubble at the foot of a high steeple or hill, and carrying it in its close frame to the top, one may, by the weight requisite to be added to counterpoise there, to bring the beam to its horizontal position, observe the difference of the weight of the air at the bottom, and at the top; and in case the hill be high enough at some intermediate stations. But how far this may assist men to estimate the absolute or comparative height of mountains, and other elevated places; and what other uses the instrument may be put to when it is duly improved; and the cautions that may be requisite in the several cases that shall be proposed, I must leave to more leisure and farther consideration.



A NEW  
FRIGORIFICK EXPERIMENT,  
S H E W I N G

How a considerable degree of COLD may be suddenly produced without the help of SNOW, ICE, HAIL, WIND, or NITRE, and that at any time of the year.

First printed in the *Philosophical Transactions*, N<sup>o</sup>. XV. p. 255.

For Wednesday, July 18, 1666.

AS for the experiment you saw the other day at my lodgings, though it belongs to some papers about cold, that (you know) could not be published when the rest of the history came forth, and therefore was reserved for the next edition of that book; yet the weather having been of late very hot, and threatening to continue so, I presume, that to give you here, in compliance with your curiosity, an account of the main and practical part of the experiment, may enable you to gratify not only the curious among your friends, but those of the delicate, that are content to purchase a coolness of drinks at a somewhat chargeable rate.

You



You may remember that the spring before the last, I shewed you a particular account of a way wherein, by a certain substance obtained from sal armoniac, I could presently produce a considerable degree of cold, and that with odd circumstances, without the help of snow, ice, nitre, &c. But that experiment being difficult and costly enough, and designed to afford men information, not accommodations, I afterwards tried what some more cheap and facile mixtures of likely bodies with sal armoniac would do towards the production of cold, and afterwards I began to consider, whether to that purpose alone (for my first experiment was designed to exhibit other phenomena too) those mixtures might not without inconvenience be omitted. And I was much confirmed in my conjecture by an accident which was casually related to me by a very ingenious physician of my acquaintance, but not to be repeated to you in a few words, though he complained, he knew not what to make of it.

AMONG the several ways by which I have made infrigidating mixtures with sal armoniac, the most simple and facile is this: take one pound of powdered sal armoniac and about three pints (or pounds) of water; put the salt into the liquor, either all together, if your design be to produce an intense, though but a short coldness; or at two, three, or four several times, if you desire that the produced coldness should rather last somewhat longer than be so great. Stir the powder in the liquor with a stick or whalebone (or some other thing that will not be injured by the fretting brine that will be made) to hasten the dissolution of the salt, upon the quickness of which depends very much the intensity of the cold, that will ensue upon this experiment. For the clearing up whereof, I shall annex the following particulars.

I. THAT a considerable degree of cold is really produced by this operation, is very evident: First, to the touch; Secondly, by this, that if you make the experiment (as for this reason I sometimes chuse to do) in a glass-body or a tankard, you may observe, that whilst the solution of the salt is making, the outside of the metalline vessel will, as high as the mixture reaches within, be bedewed (if I may so speak) with a multitude of little drops of water, as I have † elsewhere shewn, that it happens when mixtures of snow and salt being put into glasses or other vessels, the aqueous vapours that swim to and fro in the air, and chance to glide along the sides of the vessels, are by the coldness thereof condensed into water. And in our armoniac solution you may observe, that if you wipe off the dew from any particular part of the outside of the vessel, whilst the solution does yet vigorously go on, it will quickly collect fresh dew, which may be sometimes copious enough to run down the sides of the vessel. But thirdly, the best and surest way of finding out the coldness of our mixture is that which I shewed you, by plunging into it a good sealed weather-glass furnished with tinted spirit of wine; for the ball of this being put into our frigorifick mixture, the crimson liquor will nimbly enough descend much lower than when it was kept in the open air, in common water, of the same temper with that wherein the sal armoniac was put to dissolve. And if you remove the glass out of the mixture into common water, the tinted spirit will (as you may remember it did) hastily enough re-ascend for a pretty while, according to the greater or lesser time that it continued in the armoniac solution. And this has succeeded with me, when instead of removing the mixture into common water, I removed it into water newly impregnated with salt-petre.

THE duration of the cold produced by this experiment depends upon several circumstances; as, first, upon the season of the year, and present temperature of the air; for in summer and hot weather the cold will sooner decay and expire. Secondly, upon the quan-

† In the *History of Cold*.



city of the salt and water; for, if both these be great, the effect will be as well more lasting, as more considerable. Thirdly, for aught I yet know, we may here add the goodness and fitness of the particular parcel of salt that is employed: for though it be hard to discern beforehand, which will be the more, and which the less proper; yet some trials have tempted me to suspect that there may be a considerable disparity, as to their fitness to produce cold, betwixt parcels of salt, that are without scruple looked upon as sal armoniac; of which difference it were not perhaps very difficult to assign probable reasons from the nature of the ingredients of this compound concrete, and the ways of preparing it. But the duration of the cold may be conceived to depend also, fourthly, upon the way of putting the salt into the water; for if you cast it in all at once, the water will sooner acquire an intense degree of coldness, but it will also the sooner return to its former temper; whereas, if you desire but an inferior degree of quality, but that may last longer (which will usually be the most convenient for the cooling of drinks) then you may put in the salt by little and little. For keeping a long weather-glass for a good while in our impregnated mixture, I often purposely tried, that, when the tinged liquor subsided but slowly, or was at a stand, by putting in from time to time two or three spoonfuls of fresh salt, and stirring the water to quicken the dissolution, the spirit of wine would begin again to descend, if it were at a stand, or rising, or subside much more swiftly than it did before. And if you would lengthen the experiment, it may not be amiss, that part of the sal armoniac be but grossly beaten, that it may be the longer in dissolving, and consequently in cooling the water. Whilst there are dewy drops produced on the outside of the vessel, it is a sign that the cold within continues pretty strong; for when it ceases, these drops, especially in warm weather, will by degrees vanish. But a surer way of measuring the duration of the cold is, by removing from time to time the sealed weather-glass out of the saline mixture into the same common water, with part of which it was made. And though it be not easy to determine any thing particularly about this matter, yet it may somewhat assist you in your estimates, to be informed, that I have in the spring by a good weather-glass found a sensible adventitious cold, made by a pound of sal armoniac at the utmost, to last about two or three hours.

3. To cool drinks with this mixture, you may put them in thin glasses, the thinner the better; which (their orifices being stopped, and still kept above the mixture) may be moved to and fro in it, and then be immediately poured out to be drunk: though when the glass I employed was conveniently shaped, as like a sugar-loaf, or with a long neck, I found it not amiss to drink it out of that, without pouring it into any other; which can scarce be done without lessening the coldness. The refrigeration, if the glass-phial be convenient, is quickly performed: and if one have a mind to cool his hands, he may readily do it by applying them to the outside of the vessel that contains the refrigerating mixture; by whose help, pieces of crystal, or bullets for the cooling of the mouths or hands of those patients, to whom it may be allowed, may be potently cooled, and other such refreshments may be easily procured.

4. How far sal armoniac, mingled with sand or earth, and not dissolved, but only moistened with a little water sprinkled on it, will keep bottles of wine or other liquors more cool than the earth or than sand alone will do, I have not yet had opportunity by sufficient trials fully to satisfy myself, and therefore resign that inquiry to the curious.

5. For the cooling of air, and liquors, to adjust weather-glasses, to be able to do which, at all times of the year, was one of the chief aims that made me bethink myself of this experiment; or to give a small quantity of beer, &c. a moderate degree of cool-



coolness, it will not be requisite to imploy near so much as a whole pound of sal armoniac at a time. For you may easily observe by a sealed weather-glass, that a very few ounces, well powdered and nimble dissolved in about four times the weight of water, will serve well enough for many purposes.

6. And that you may the less scruple at this, I shall tell you, that even before and after Midsummer, I have found the cold producible by our experiment to be considerable, and useful for refrigerating of drinks, &c. but if the sal armoniac be of the stiffest sort (for I intimated above, that I suspected it is not equally good) and if the season of the year do make no disadvantageous difference, the degree of cold that may be produced by no more than one pound (if not by less) sal armoniac may, within its own sphere of activity, be much more vehement, than, I presume, you yet imagine, and may afford us excellent standards to adjust sealed weather-glasses by; and for several other purposes. For I remember that in the spring, about the end of *March*, or beginning of *April*, I was able with one pound of sal armoniac, and a requisite portion of water, to produce a degree of cold much greater, than was necessary the preceding winter, to make it frosty weather abroad; nay, I was able to produce ice in a space of time almost incredibly short. To confirm which particulars, because they will probably seem strange to you, I will here annex the transcript of an entry, that I find in a note-book of phenomena and success of one of those experiments, as I then tried it; though I should be ashamed to expose to your perusal a thing so rudely penned, if I did not hope you would consider that it was hastily written only for my own remembrance. And that you may not stop at any thing in the immediately annexed note, or the two that follow, it will be requisite to premise this account of the sealed thermoscope (which was a good one) wherewith these observations were made; that the length of the cylindrical pipe was 16 inches, the ball about the bigness of a somewhat large walnut, and the cavity of the pipe by guess about an eighth or ninth part of an inch diameter.

THE first experiment is thus registered. *March* the 27<sup>th</sup>, in the sealed weather-glass, when first put into water, the tinted spirit rested at  $8\frac{1}{8}$  inches: being suffered to stay there a good while, and now and then stirred to and fro in the water, it descended at length a little beneath  $7\frac{1}{8}$  inches: then the sal armoniac being put in, within about a quarter of an hour, or a little more, it descended to  $2\frac{1}{6}$  inches, but before that time, in half a quarter of an hour it began manifestly to freeze the vapours and drops of water on the outside of the glass. And when the frigorifick power was arrived at the height, I several times found, that water thinly placed on the outside, whilst the mixture within was nimble stirred up and down, would freeze in a quarter of a minute (by a minute watch). At about three quarters of an hour after the inrigidating body was put in, the thermoscope that had been taken out a while before, and yet was risen but to the lowest freezing mark, being again put in the liquor, fell an inch beneath the mark. At about 2½ hours from the first solution of the salt, I found the tinted liquor to be in the midst between the freezing marks, whereof the one was at  $5\frac{1}{2}$  inches (at which height when the tincture rested, it would usually be some, though but a small frost abroad; and the other at  $4\frac{1}{2}$  inches; which was the height to which strong and durable frosts had reduced the liquor in the winter. At three hours after the beginning of the operation, I found not the crimson liquor higher than the upper freezing mark newly mentioned; after which, it continued to rise very slowly for about an hour longer; beyond which time I had not occasion to observe it.

Thus far the note-book; wherein there is mention made of a circumstance of some former experiments of the like kind, which I remember was very conspicuous in this newly recited. For the frigorifick mixture having been made in a large glass-body (as



they call it) with a large and flattish bottom, a quantity of water which I (purposely) spilt upon the table, was, by the operation of the mixture within the glass, made to freeze, and that strongly enough, the bottom of the cucurbit to the table; that stagnant liquor being turned into solid ice, that continued a considerable while unthawed away, and was in some places about the thickness of a half-crown piece.

ANOTHER observation made the same spring, but less solemn, as meant chiefly to shew the duration of cold in a high degree, is recorded in these terms: the first time the sealed weather-glass was put in, before it touched the common water, it stood at  $8\frac{1}{2}$ , having been left there a considerable while, and once or twice agitating the water, the tinted liquor sunk but to  $7\frac{1}{2}$ , or at furthest  $7\frac{3}{8}$ ; then the frigorifick liquor being put into the water with circumstances disadvantageous enough, in (about) half a quarter of an hour the tinted liquor fell beneath  $3\frac{3}{4}$ ; and the thermoscope being taken out, and then put in again, an hour after the water had been first infrigedated, subsided beneath 5 inches, and consequently within a quarter of an inch of the mark of the strongly freezing weather.

7. WHEREAS the grand thing that is like to keep this experiment from being as generally useful, as perhaps it will prove lucriferous, is the dearness of sal armoniac, two things may be offered to lessen this inconvenience. For first, sal armoniac might be made much cheaper, if instead of fetching it beyond sea, our countrymen made it at home; which it may easily be, and I am ready to give you the receipt, which is no great secret. But next I considered, that probably the infrigiding virtue of our mixture might depend upon the peculiar texture of the sal armoniac, whereby, whilst the water is dissolving it, either some frigorifick particles are extricated and excited, or rather some particles, which did before more agitate the minute parts of the water, are expelled (or invited out by the ambient bodies) or come to be clogged in their motion: whence it seemed reasonable to expect that upon the re-union of the saline particles into such a body as they had constituted before, the redintegrated sal armoniac, having near upon the same texture, would, upon its being redissolved, produce the same, or a not much inferior degree of coldness: and hereupon, though I well enough foresaw that an armoniac solution, being boiled up in earthen vessels (for glass ones are too chargeable) would, by piercing them, both lose some of the more subtle parts, and thereby somewhat impair the texture of the rest; yet I was not deceived in expecting that the dry salt remaining in the pipkins, being re-dissolved in a due proportion of water, would very considerably infrigide it; as may further appear by the notes, which for your greater satisfaction you will find here subjoined, as soon as I have told you, that, though for want of other vessels I was first reduced to make use of earthen ones, and the rather, because some metalline vessels will be injured by the dissolved sal armoniac, if it be boiled in them; yet I afterwards found some conveniencies in vessels of other metal, as of iron; whereof you may command a farther account.

*March* the 29th the thermoscope in the air was at  $8\frac{7}{8}$  inches; being put into a somewhat large evaporating glass, filled with water, it fell (after it staid a pretty while, and had been agitated in the liquor) to 8 inches: then about half the salt, or less, that had been used twice before, and felt much less cold than the water, being put in and stirred about, the tinted spirit subsided with a visible progress, till it was fallen manifestly beneath four inches; and then, having caused some water to be freshly pumped and brought in, though the newly mentioned solution were mixt with it, yet it presently made the spirit of wine manifestly to ascend in the instrument, much faster than one would have expected, &c.

AND



AND thus much may suffice for this time concerning our frigorifick experiment; which I scarce doubt but the *Cartesians* will lay hold on as very favourable to some of their tenets; which you will easily believe it is not to the opinion I have elsewhere opposed of those modern philosophers, that would have salt-petre to be the *primum frigidum* (though I found by trial, that whilst it is actually dissolving, it gives a much considerable degree of cold, than otherwise). But about the reflections that may be made on this experiment, and the variations and improvements, and uses of it, though I have divers things lying by me; yet, since you have seen several of them already, and may command a sight of the rest, I shall forbear the mention of them here, not thinking it proper to swell the bulk of this letter with them.

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## The Method observed in Transfusing the Blood out of one Animal into another.

First printed in the *Philosophical Transactions* No. XX. p. 353.

For *Monday*, December 17, 1666.

THIS method was promised in the last of these papers. It was first practised by Dr. *Lower* in *Oxford*, and by him communicated to the author, who imparted it to the Royal Society, as follows:

FIRST, Take up the carotid artery of the dog or other animal whose blood is to be transfused into another of the same or a differing kind, and separate it from the nerve of the eighth pair, and lay it bare above an inch; then make a strong ligature on the upper part of the artery not to be untied again; but an inch below, *viz.* towards the heart, make another ligature of a running knot, which may be loosened or fastened as there shall be occasion. Having made these two knots, draw two threads under the artery between the two ligatures; and then open the artery, and put in a quill, and tie the artery upon the quill very fast by those two threads, and stop the quill with a stick. After this, make bare the jugular vein in the other dog about an inch and a half long; and at each end make a ligature with a running knot, and in the space betwixt the two running knots draw under the vein two threads, as in the other; then make an incision in the vein, and put into it two quills, one into the descendent part of the vein, to receive the blood from the other dog and carry it to the heart; and the other quill put into the other part of the jugular vein which comes from the head (out of which the second dog's own blood must run into dishes). These two quills being put in and tied fast, stop them with a stick till there be occasion to open them.

ALL things being thus prepared, place the dogs on their sides towards one another so conveniently that the quill may go into each other; (for the dog's necks cannot be brought so near but that you must put two or three several quills more into the first two, to convey the blood from one to another). After that unstop the quill that goes down into the first dog's jugular vein, and the other quill coming out of the other dog's artery;

## *The Method of transfusing the Blood*

artery; and by the help of two or three other quills put into each other, according as there shall be occasion, insert them into one another; then slip the running knots, and immediately the blood runs through the quills, as through an artery, very impetuously. And immediately, as the blood runs into the dog, untie the other quill, coming out of the upper part of his jugular vein (a ligature being first made about his neck, or else his other jugular vein being compressed by one's finger); and let his own blood run out at the same time into dishes (yet not constantly, but according as you perceive him able to bear it) till the other dog begin to cry, and faint, and fall into convulsions, and at last die by his side.

THEN take out both the quills out of the dog's jugular vein, and tie the running knot fast, and cut the vein asunder (which you may do without harm to the dog, one jugular vein being sufficient to convey all the blood from the head and upper parts, by reason of a large anastomosis, whereby both the jugular veins meet about the larynx). This done, sew up the skin and dismiss him, and the dog will leap from the table and shake himself, and run away, as if nothing ailed him.

AND this I have tried several times before several in the universities, but never yet upon more than one dog at a time for want of leisure and convenient supplies of several dogs at once. But when I return I doubt not but to give you a fuller account, not only by bleeding several dogs into one, but several other creatures into one another, as you did propose to me before you left *Oxford*; which will be very easy to perform, and will afford many pleasant, and perhaps not unuseful experiments.

BUT because there are many circumstances necessary to be observed in the performing of this experiment, and that you may better direct any one to do it, without any danger of killing the other dog that is to receive the other's blood, I will mention two or three.

FIRST, that you fasten the dogs at such a convenient distance, that the vein nor artery be not stretched; for then, being contracted, they will not admit or convey so much blood.

SECONDLY, that you constantly observe the pulse beyond the quill in the dog's jugular vein (which it acquires from the impulse of the arterious blood) for if that fails, then it is a sign the quill is stoped by some congealed blood, so that you must draw out the arterial quill from the other, and with a probe open the passage again in both of them, that the blood may have its free course again. For this must be expected when the dog that bleeds into the other hath lost much blood, his heart will beat very faintly, and then the impulse of blood being weaker, it will be apt to congeal the sooner, so that at the latter end of the work you must draw out the quill oftener, and clear the passage; if the dog be faint-hearted, as many are, though some stout fierce dogs will bleed freely and uninterruptedly, till they are convulsed and die. But to prevent this trouble, and make the experiment certain, you must bleed a great dog into a little one, or a mastiff into a cur, as I once tried; and the little dog bled out at least double the quantity of his own blood, and left the mastiff dead upon the table; and after he was untied he ran away, and shook himself, as if he had been only thrown into water. Or else you may get three or four several dogs prepared in the same manner; and when one begins to fail and leave off bleeding, administer another; and I am confident one dog will receive all their blood (and perhaps more) as long as it runs freely, till they are left almost dead by turns; provided you let out the blood proportionably, as you let it go into the dog that is to live.

THIRDLY, I suppose the dog that is to bleed out into dishes, will endure it better, if the dogs that are to be administered to supply his blood, be of near an equal age, and

fed



fed alike the day before, that both their bloods may be of a near strength and temper.

THERE are many things I have observed upon bleeding dogs to death, which I have seen since your departure from *Oxford*, whereof I shall give you a relation hereafter. In the mean time since you were pleased to mention it to the Royal Society, with a promise to give them an account of this experiment, I could not but take the first opportunity to clear you from that obligation, &c.

So far this letter; the prescriptions whereof having been carefully observed by those who were employed to make the experiment, have hitherto been attended with good success; and that not only upon animals of the same species (as two dogs first, and then two sheep); but also upon some of very differing species (as a sheep and a dog, the former emitting the other receiving).

NOTE only, that instead of a quill, a small crooked thin pipe of silver or brass, so slender, that the one end may enter into the quill, and having at the other end, that is to enter into the vein and artery, a small knob for the better fastening them to it with a thread, will be much fitter than a strait pipe or quill, for this operation; for so they are much more easy to be managed.

IT is intended that these trials shall be prosecuted to the utmost variety the subject will bear; as by exchanging the blood of old and young, sick and healthy, hot and cold, fierce and fearful, tame and wild animals, &c. and that not only of the same but also of differing kinds. For which end, and to improve this noble experiment, either for knowledge, or use, or both, some ingenious men have already proposed considerable trials and inquiries; of which perhaps an account will be given hereafter. For the present we shall only subjoin some.

### *Considerations about this kind of Experiments.*

1. IT may be considered in them, that the blood of the emittent animal may, after a few minutes of time, by its circulation, mix and run out with that of the recipient. Wherefore, to be assured in these trials that all the blood of the recipient is run out, and none left in him but the adventitious blood of the emittent, two, or three, or more animals (which was also hinted in the method above) may be prepared and administered to bleed them all out into one.

2. IT seems not irrational to guess beforehand that the exchange of blood will not alter the nature or disposition of the animals, upon which it shall be practised; though it may be thought worth while, for satisfaction and certainty, to determine that point by experiments. The case of exchanging the blood of animals seems not like that of grafting, where the cyon turns the sap of the stock, grafted upon, into its nature; the fibres of the cyon so straining the juice which passes from the stem to it, as thereby to change it into, that of the cyon: whereas in this transfusion there seems to be no such percolation of the blood of animals, whereby that of the one should be changed into the nature of the other.

3. THE most probable use of this experiment may be conjectured to be, that one animal may live with the blood of another; and consequently those animals that want blood, or have corrupt blood, may be supplied from others with a sufficient quantity, and of such as is good, provided the transfusion be often repeated, by reason of the quick expence that is made of the blood.

Trials proposed to Dr. LOWE R,

To be made by him, for the Improvement of transfusing Blood out of one live Animal into another.

(Promised Numb. XX. p. 357.)

First Printed in the *Philosophical Transactions*, No. XXII. p. 385.

For *Monday*, February 11, 1666.

THE following queries and trials were written long since and read about a month ago in the Royal Society, and do now come forth against the author's intention, at the earnest desire of some learned persons, and particularly of the worthy doctor to whom they were addressed; who thinks, they may excite and assist others in a matter, which to be well prosecuted will require many hands. At the reading of them, the author declared that of divers of them he thought he could foresee the events, but yet judged it fit not to omit them, because the importance of the theories they may give light to may make the trials recompence the pains, whether the success favour the affirmative or the negative of the question, by enabling us to determine the one or the other upon surer grounds than we could otherwise do. And this advertisement he desires may be applied to those other papers of his that consist of queries or proposed trials.

*The Queries themselves follow.*

1. WHETHER by this way of transfusing blood, the disposition of individual animals of the same kind may not be much altered? (as whether a fierce dog, by being often quite new stocked with the blood of a cowardly dog, may not become more tame) & *vice versa*, &c.

2. WHETHER immediately upon the unbinding the dog, replenished with adventurous blood, he will know, and fawn upon his master, and do the like customary things as before? and whether he will do such things better or worse at some time after the operation?

3. WHETHER those dogs that have peculiarities will have them either abolished, or at least much impaired by transfusion of blood? (as whether the blood of a mastiff, frequently



frequently transfused into a blood-hound, or a spaniel, will not prejudice them in point of scent)?

4. WHETHER acquired habits will be destroyed or impaired by this experiment? (as whether a dog taught to fetch and carry, or to dive after ducks, or to sett, will, after frequent and full recruits of the blood of dogs unfit for those exercises, be as good at them as before?)

5. WHETHER any considerable change is to be observed in the pulse, urine, and other excrements of the recipient animal by this operation, or the quantity of his insensible transpiration?

6. WHETHER the emittent dog, being full fed at such a distance of time before the operation, that the mass of blood may be supposed to abound with chyle, the recipient dog, being before hungry, will lose his appetite more than if the emittent dog's blood had not been so chylous? and how long, upon a vein opened of a dog, the admitted blood will be found to retain chyle?

7. WHETHER a dog may be kept alive without eating by the frequent injection of the chyle of another, taken freshly from the receptacle, into the veins of the recipient dog?

8. WHETHER a dog that is sick of some disease, chiefly imputable to the mass of blood, may be cured by exchanging it for that of a sound dog? and whether a sound dog may receive such diseases from the blood of a sick one, as are not otherwise of an infectious nature?

9. WHAT will be the operation of frequently stocking (which is feasible enough) an old and feeble dog with the blood of young ones, as to liveliness, dulness, drowsiness, squeamishness, &c. & *vice versa*.

10. WHETHER a small young dog, by being often fresh stocked with the blood of a young dog of a larger kind, will grow bigger than the ordinary size of his own kind?

11. WHETHER any medicated liquors may be injected, together with the blood, into the recipient dog? and in case they may, whether there will be any considerable difference found between the separations made on this occasion, and those which would be made, in case such medicated liquors had been injected with some other vehicle, or alone, or taken in at the mouth?

12. WHETHER a purgative medicine being given to the emittent dog a while before the operation, the recipient dog will be thereby purged, and how? (which experiment may be hugely varied).

13. WHETHER the operation may be successfully practised, in case the injected blood be that of an animal of another species, as of a calf into a dog, &c. and of a cold animal, as of a fish, or frog, or tortoise, into the vessels of a hot animal? & *vice versa*?

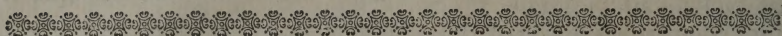
14. WHETHER the colour of the hair or feathers of the recipient animal, by the frequent repeating of this operation, will be changed into that of the emittent?

15. WHETHER by frequently transfusing into the same dog, the blood of some animal of another species, something further, and more tending to some degrees of a change of species, may be effected, at least in animals near of kin (as spaniels and setting dogs, Irish grey-hounds and ordinary grey-hounds, &c.)?

16. WHETHER the transfusion may be practised upon pregnant bitches, at least at certain times of their gravitation? and what effect it will have upon the whelps?

THERE were some other queries proposed by the same author; as the weighing of the emittent animal before the operation, that (making an abatement for the effluvioms,

and for the excrements, if it voids any) it may appear how much blood it really loses. To which were annexed divers others, not so fit to be perused but by physicians, and therefore here omitted.



## P R O P O S A L S

To try the Effects of the Pneumatick Engine exhausted, in Plants, Seeds, Eggs of Silk-worms.

First Printed in the *Philosophical Transactions*, No. XXIII. p. 424.

For *Monday*, March 11, 1666.

THE ingenious Dr. Beale did formerly suggest, as follows:

It would be, I think (saith he) very well worth the trial, to see what effects would be produced on plants put into the pneumatick (or rarefying) engine of Mr. Boyle, with the earth about their roots, and flourishing; whether they would not suddenly wither if the air were totally taken from them. And particularly to try in the season cherry-blossoms, when partly opened, partly not opened, upon a branch; to wit, whether the air may be so attenuated as to blast. But it may be noted, that the blossoms do not forthwith discover the blast; an old experienced countryman having once given me notice of a blasty noon (it being then sultry weather and somewhat gloomy with the thickness of exhalations, almost like a very thick mist) and within a day or two shewing the proof upon the cherry-blossoms then flagging, but not much altering their colour till two days more were past.

THE noble Mr. Boyle suggests, as proper for the approaching season, that it may be tried,

1. WHETHER seeds (especially such as are of a hasty growth, viz. orpin, lettuce, garden-cress-seeds, &c.) will germinate and thrive in the exhausted receiver of the said engine?

2. WHETHER the exclusion of air from the sensitive plant would be harmful to it?

3. WHETHER the grafting of pears upon spina cervina (the almost only purgative vegetable known in England) will produce the effect of communicating to the fruit that purging quality, or not?

4. WHETHER silk-worms eggs will be hatched in such an exhausted receiver, in the season proper for hatching?

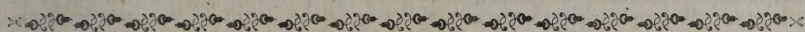
To which may be added the trials of putting in a phial, full of water, some of those herbs that will shoot and grow in water alone, including them in such a receiver, and pumping out what air you can, to see whether they will then shoot or not?

AND



AND though some of these proposals have been formerly begun to be experimented, yet ought they to be diligently prosecuted, to see how far the air is necessary to vegetation; and whether plants do indeed live as much upon the air, as the earth; and the branches of them are rooted (as it were) in, and quickened by the air, as their roots are planted and nourished in and by the earth?

THE experiment hitherto made of this kind was, that some lettuce-seed being sown upon some earth in the open air, and some of the same seed at the same time upon other earth in a glass-receiver of the above mentioned engine, afterwards exhausted of air; the seed exposed to the air was grown up an inch and a half high within eight days; but that in the exhausted receiver, not at all. And air being again admitted into the said emptied receiver, to see whether any of the seed would then come up, it was found that in the space of one week it was grown up to the height of two or three inches.



A Confirmation of the Experiments mentioned in No. XXVII. to have been made by Signor Fracassati in *Italy*, by injecting Acid Liquors into Blood.

First printed in the *Philosophical Transactions*, No. XXIX. p. 551.

For *Monday*, November 11, 1667.

[THE author having seen the particulars inserted in Numb. XXVII. concerning some experiments made by Signor *Fracassati*, and recollecting what himself had experimented of that nature several years ago, was pleased to give to the publisher the following information about it by the favour of a letter, written to him from *Oxford*, Oct. 19, 1667.]

S I R,

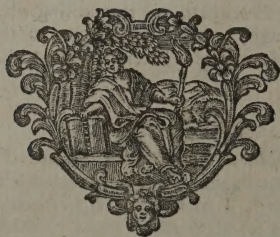
I Hinted to you in my last something about the original of the experiments made in *Italy*, by injecting acid liquors into blood; to explain which, I shall now tell you, that about this time three years \*, I mentioned, at *Gresham* college, to the Royal Society, an odd experiment I had formerly made (not by chance, but design) upon blood

\* The Journals of the Royal Society being looked into by the publisher (who, by the honour of his relation to that illustrious body, hath the advantage of perusing them, as he by his office hath the care of seeing them faithfully managed) do fully agree with the affirmation of this noble person, as well in the circumstance of the time, as the substance of the matter in question; it being in the month of December of An. 1664, when what is now alledged in this letter was publicly related by its author.

yet warm, as it came from the animal, viz. That by putting into it a little aqua fortis, or oil of vitriol, or spirit of salt (these being the most usual acid menstruums) the blood not only would presently lose its pure colour, and become of a dirty one, but in a trice be also coagulated; whereas if some fine urinous spirit, abounding in volatile salt, such as the spirit of sal armoniac, were mingled with the warm blood, it would not only not curdle it, or imbase its colour, but make it rather more florid than before, and both keep it fluid, and preserve it from putrefaction for a long time.

THIS experiment I devised, among other things, to shew the amicableness of volatile spirits to the blood; and I remember it was so much taken notice of, that some very inquisitive members of the Society came presently to me and desired me to acquaint them more particularly with it; which I readily did, though afterwards I made some further observations about the same experiment that I had no occasion to relate.

THIS having been so publickly done, though I shall not say that Signor *Fracassati* may not have hit, as well as I, upon the experiments published in his name, yet there is so little difference between the warm blood of an animal out of his veins and in them, that it is not very improbable that he may have had some imperfect rumour of our experiment without knowing whence it came; and so may, without any dissingenuity, have thence taken a hint to make and publish what now is englished in the Transactions. If it be thought fit that any mention be made of what I related so long since, I think I can send you some other circumstances belonging to it; for I remember I tried it with other liquors (as spirit of wine, oil of tartar, oil of turpentine) and I think also I can send you some remarks upon the colour of the upper part of the blood. And I shall on this occasion add, in reference to anatomical matters in general, that after I saw how favourably *the Usefulness of Experimental Philosophy* was received, I was invited to enlarge it in another edition; and for that I provided divers anatomical as well as other experiments, and designed many more; so that I have by me divers things that would not perhaps be unwelcome to anatomists, &c.





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## NEW EXPERIMENTS

Concerning the Relation between Light and Air, in  
shining Wood and Fish; made by the Author,  
and by him address'd from *Oxford* to the Publisher,  
and so communicated to the Royal Society.

First Printed in the *Philosophical Transactions*, No. XXXI. p. 581.

For *Monday*, January 6, 1667-8.

S I R,

**T**O perform now the promise I made you the other day, I must acquaint you with what will perhaps somewhat surprize you, by giving you an account of what I tried on *Tuesday* night last (*October* 29, 1667) and the two or three following nights, about the relation between air and light, as this is to be found in some bodies.

THE occasion of these trials was this: Having, as you know, long since made some notes, chiefly historical, upon particular qualities, and finding light to be (how justly I now dispute not) reckoned by the generality of philosophers among qualities, I huddled together what observations I had either made myself or received from some ingenious travellers (to whom I recommended my inquiries) about shining bodies; and had also prepared several trials about them, to be made when I should have opportunity and requisite instruments to put them in practice; which, as to some of those designed experiments, have been long denied me. But having at length got hither one of my little engines, and having also procured, after much inquiry, a few small pieces of shining wood, I began on the day above-mentioned to try with them an experiment I found in my list: and though the main experiment be but one; I intend to set down what occurred to me about it but as several phaenomena of it; yet finding it requisite to acquaint you with some trials that are not so properly parts of it, I shall, for distinction sake, propose them as several experiments; the narratives whereof are taken for the most part *verbatim* out of the notes I set down for my own use, when the things to be registered were freshly done. Which advertisement I give you both to excuse the carelessness of the style, and to induce you not to distrust a narrative that was made only to serve my memory, not an hypothesis.

## E X P E R I M E N T I.

To try whether or no a piece of shining wood, being put into a receiver of our pneumattick engine, would, upon the withdrawing and re-admitting of the air, suffer such changes, as I have often observed a live coal, placed there, to do. Having at length procured a piece of such wood, about the bigness of a groat or less, that gave a vivid light (for rotten wood) we put it into a middle-sized receiver, so as it was kept from touching the cement; and the pump being set a work, we observed not during the five or six first exfuctions of the air, that the splendor of the included wood was manifestly lessened (though it never was at all increased); but about the seventh suck it seemed to grow a little more dim, and afterwards answered our expectation, by losing of its light more and more, as the air was still farther pumped out; till at length about the tenth exfuction (though by the removal of the candles out of the room, and by black clothes and hats we made the place as dark as we could) yet we could not perceive any light at all to proceed from the wood.

## E X P E R I M E N T II.

WHEREFORE we let in the outward air by degrees, and had the pleasure to see the seemingly extinguished light revive so fast and perfectly, that it looked to us all, almost like a little flash of lightning, and the splendour of the wood seemed rather greater, than at all less, than before it was put into the receiver. But partly for greater certainty, and partly to enjoy so delightful a spectacle, we repeated the experiment with the like success as at first. Wherefore being desirous to see how soon these changes might be produced, we included the wood in a very small receiver of clear glass, and found that in this the light would begin to grow faint at the second, or at least at the third exfuction of the air, and at the sixth or seventh would quite disappear. And we found, by a minute-watch, that the sending the candles out of the room, the pumping out the air till the wood would shine no more, the re-admitting of the air (upon which in a trice it would recover its light) and the sending in for the candles to consult the watch, did in all take up but six minutes.

## E X P E R I M E N T III.

THE fore-mentioned experiment, without taking notice how long it lasted, being reiterated twice in this new receiver, we had a desire to see whether this luminous wood would more resemble a coal, or the life of a perfect animal, in being totally and finally extinguished, in case the air were kept from it a few minutes; or else the life of insects, which in our exhausted receiver I had observed to lose all appearance of its continuing, and that for a much longer time than a few minutes, and yet afterwards, upon the restitution of air, to recover presently, and shew manifest signs of life? Wherefore having exhausted the receiver till the wood quite disappeared, we stayed somewhat above a quarter of an hour in the dark without perceiving that the wood had regained any thing of light, though about the end of this time we made the place about it as dark as we could; and then it being too late at night to protract the experiment, we let in the air, upon whose admission the wood presently recovered light enough to be conspicuous at a distance;



distance; though it seemed to me somewhat less vivid than before: which yet may be either a weakness in my sight, or an effect of the steams of the cement, unfriendly perhaps to the luminousness of the wood.

*Thus far we proceeded yester-night, to which we this night added these observations.*

We put in a piece of wood bigger than the former (this being above an inch long) and that shone very vigorously. And having by a few sucks quite deprived it of light, we left it in the exhausted receiver for full half an hour, and then coming into the dark room again, we found all had not continued so staunch, but that some small portion of air had insinuated itself into the receiver. This we concluded to be but a small portion of air, because the wood was but visible to an attentive eye: and yet, that it was really some air which was got in that caused the little glimmering light which we perceived, may appear by this, that it did presently (as we expected) vanish at the first or second suck; and then the air being let into the dark receiver, the included wood presently shone again as before; though I suspected I discerned some little diminution of its brightness; which yet, till further trials of the like kind, and for a longer time, have been made, I dare not affirm. Before the receiver was sufficiently emptied at the beginning of the experiment made with this great piece of wood, a small leak accidentally sprung, which, letting in a little air, did, sooner than we intended, recall the almost disappearing light.

#### EXPERIMENT IV.

THERE is an experiment of affinity with the former which we thought it not altogether impertinent to try; for having observed on another occasion, that sometimes the operation which the withdrawing the air hath upon a body included in the receiver, proves more considerable some minutes after we have ceased pumping, than immediately after the exercise is left off; I imagined that even in such cases where the light is not made wholly to disappear (though it be made almost quite to do so) by the emptying of the pneumatical glass, the suffering the body to remain a while there, though without any pumping (unless now and then a very little to remove the air that might have stolen in, in the mean time the remaining light of the body might be further impaired, if not reduced quite to vanish. To examine this conjecture we put in a body that was not wood, which had some parts far more luminous than the rest; and having drawn out the air, all the others disappeared, and even the formerly brighter ones shone but faintly when the pneumatical glass seemed to be exhausted. But keeping the included body a while in that unfriendly place we perceived the parts that had retained light to grow more and more dim, some of them disappearing, and that which was formerly the most conspicuous, being now but just visible to an attentive eye, and that scarce without dispute. For if we had not known beforehand, that a shining matter had been included in the receiver, perhaps we should not have found it out, and he that had the youngest eyes in the company could not at all discern it (the air being let in, the body began to shine again): but this being a single trial, which the lateness of the night hindered us from re-iterating, is to be further prosecuted, and in differing substances, before much be built upon it.

## EXPERIMENT V.

THE rarefaction or expansion of the air having so notable an operation upon our shining wood, I thought it would not be amiss to try what the compression of the air would do to it. For which purpose we included a piece of it in such a little instrument to compress, which you may remember to have been devised and proposed by Mr. Hook. But though we impelled the air forcibly enough into the glass, yet, by reason of the thickness requisite in such glasses, and the opacity thence arising, we were not able then to determine whether or no any change was made in the luminousness of the wood.

WHICH I thought the less strange, because by some experiments purposely devised (at one of which I remember you were present) I had long since observed that even a great pressure, from a fluid body which presseth more uniformly against all the parts it toucheth of the consistent body, does work a far less manifest change, even on soft or tender substances, than one would expect from the force wherewith it compresseth.

AND were it not that one contrary oftentimes minds us of another, I might have forgot that I had divers thoughts about finding some good ways of trying, whether any such change of texture might be discovered to be made in the shining wood by the absence and return of the ambient air, as might with any probability have the loss or recovery of the wood's splendour attributed to it. For I had formerly (if I were not mistaken) found by several circumstances, which I shall not now stay to name, that a slight (so it be an appropriated) variation of the texture of this wood, and which may seem mainly to respect the pores (which perhaps ought to be of a determinate shape and size, and filled with a determinate matter) will have a great operation upon its splendour. And I formerly found by other trials, that even consistent bodies, if soft ones, may have their pores enlarged and vitiated, and their bulk, and consequently their texture (at least as to their pores) manifestly enough altered by having the air withdrawn from about them (whereby the aerial particles within them were able to expand themselves) and let in again; whereby, as to sense, they seemed pretty well restored to their former state. But the success of my endeavours, either with microscopes (through which a vivid piece of wood will shine by its own light) or otherwise, was not considerable enough to deserve a particular account; especially in this paper, where I am not to venture at matter of theory.

## EXPERIMENT VI.

THINKING fit to try whether a small quantity of air, without being ventilated or renewed, might not suffice to maintain this cold fire, though it will not that of a live coal, or a piece of match, we caused a piece of shining wood to be hermetically sealed up in a pipe of clear and thin glass: but though, carrying it into the dark, we found it had quite lost its light, yet imagining that that might proceed from its having been overheated (being sealed up in a pipe not long enough to afford it a due distance from the flame of the lamp we employed to seal it); we caused two or three pieces of fresh wood, amounting all of them to the length of about two inches, to be sealed up in a slender pipe between four or five inches in length, which being warily done, the wood retained its light very well when the operation was over: and afterwards laying it by my bedside,



side, when the candles were carried away out of the room, I considered it a while before I fell asleep, and found it to shine vividly.

THE next morning when I awaked, though the sun was risen, yet forbearing to draw open the curtains of my bed till I had looked upon the sealed glass, which I had fenced with a piece of cloth held between it and the window, my eyes having not yet been exposed to the day-light, since the darkness they had been accustomed to during the night, made me think the wood shined brighter than ever. And this night, after ten of the clock, looking on it in a dark place, it appeared luminous all its length, though not so much as in the morning.

THE morning after, and the night after that, the same wood did likewise manifestly, though not vigorously shine; especially one piece whose light was much more vivid than the rest: and, for aught I know, I might have observed them to shine longer, if one of the sealed ends of the glass had not been accidentally broken.

#### EXPERIMENT VII.

WHILST the former trials were making, I was wishing for a good *Bolonian* stone to try what effect the withdrawing of the air would have upon it. For though I knew it might be objected that the experiments of light performable in our engine must be made in the night, whereas the *Bolonian* stone gains its light by being exposed to the sun-beams; yet that objection did not hinder my wish, since the better sort of *Bolonian* stones may be induced with a luminousness by the flame of fire or of large candles.

I ALSO wished for such a shining diamond as is now in the hands that best deserve such a rarity, our Royal Founder's. For you may remember, that in the observations I made of that stone, and annexed to the conclusion of the book of colours, I shew how it may several ways be brought to shine; so that by one or other of those ways, especially that of external heat, I thought it very likely I should be able to make the light continue four or five minutes, which would be long enough to try in a very small receiver, exhaustible at a suck or two, whether the withdrawing and restoring the air would have any visible operation on it?

I ALSO wished for some of the glow-worms with which I formerly made other trials. For though I forgot not what operation the withdrawing of the air, by our engine, is wont to have upon living creatures, yet that made me not forbear my wish; not only because of the different effect I have found the engine to have on insects in respect of other animals, but because I am not of the opinion of those modern writers who will have the light of the glow-worms depend altogether upon their life, and end with it. But being not likely by my wishes to procure any new subject to make trials on, I thought fit at least to do what was in my power; and accordingly (to gratify them, who, I presumed, would, if present, propose such a trial) caused a piece of iron to be forged whose top was of the bigness of a nut-meg; the rest being a stem of an inch, or an inch and half long, for which we provided a little candle-stick of tobacco-pipe clay, which would not yield any smoke to fill and darken the receiver: then having heated the iron red-hot, and placed it in this clay, so that the round part was clearly protuberant, we conveyed it into a receiver of white glass, which was so placed as to keep the sides at as good a distance as we could from the iron, lest the excessive heat should (as we much feared it would) break the glass; then sending away the candles, and making the room dark, we hastily pumped out the air, but could not perceive the withdrawing

of it had any operation on the glowing iron. And though it continued shining long enough to give us opportunity to pump out and let in the air three several times, yet we could not observe that the air had any manifest operation one way or other. For though, upon the withdrawing of the air, the iron grew dimmer and dimmer, yet that I attributed to the cooling of it; and the rather, because, having (to examine the conjecture) let in two or three times the air when the receiver had been exhausted, there appeared no manifest increase of light upon the sudden admission of it.

## EXPERIMENT VIII.

HAVING formerly, in our *Physico-mechanical* experiments about the spring of the air, observed that the air is thus far a vehicle of sound, that a body but faintly sounding, being placed in our receiver, gave a yet weaker sound when the air was withdrawn from about it, than when the receiver was full of air; I presumed some curious persons would, if they had been present, desire to have a trial made, whether or no a small piece of shining wood being so included in the receiver, as that the pumping out of the air should have no injurious operation upon the body of it, its light would upon the withdrawing of the air be manifestly diminished. And this I was the less backward to try, because (not to mention the relation which the former experiments shew there may be in some cases between light and air) it did not readily occur to my memory, that by any manifest experiment (for I know there are probable reasons to prove it) it appeared that a body more thin than air will or can transmit light, as well as other diaphanous mediums. And those modern atomists that think there is in our exhausted receiver very many times more vacuum than body, would, I presumed, be glad to be supplied with an argument against the Peripateticks, to shew that the motion of bodies, *viz.* the corpules of light may be freely made *in vacuo*, and proceed without the assistance of a vehicle.

WHEREFORE, having hermetically sealed up a small piece of shining wood in a slender pipe, and placed it in a small receiver that was likewise made of clear glass, we exhausted it of air, and afterwards let in again that which we had excluded; but by neither of the operations could we perceive any sensible decrement or increase of the light of the wood; though by that very observation it appeared that the glass had been well sealed, since otherwise the included air would have got out of the pipe into the receiver, and have left the wood without light.

## EXPERIMENT IX.

I HAD also a mind to try both what degree of rarefaction of the air would deprive the wood of its splendour in such and such measures, and whether or no the self-same air, which, when rarefied, would not suffer the wood to shine, would, when reduced to its former density, allow it to shine as much as before.

THIS I proposed to do by putting some shining wood into a clear and conveniently shaped glass, that the long stem or pipe being so far filled with quicksilver, as that there might be about half a spoonful of air left at the closed end where the wood was placed, it might be inverted into a little glass of stagnant quicksilver, and therewith conveyed into a slender receiver, out of which, as the air should come to be pumped, that included in the glass which held the wood might be rarefied, and afterwards, upon  
the



the admission of the outward air (which must impel up the quicksilver to its former height) might be restored to its former state. But when we came to trial of this, we had no receiver conveniently shaped that was so clear and thin as that we could see the wood shine through both the glasses; and though we would for an expedient have substituted a fine thin bladder, wherein the wood was to be put, and a convenient quantity of air strongly tied up with it, yet for want of a bladder fine enough for our turn, that expedient also proved useless to us. But being desirous to make what trial we could by the least unfit means we had in our power, we got an old but thin glass, sealed at one end, whose shape was pretty cylindrical, and whose bore was about the bigness of a man's little finger, and whose length was about a foot or more; into this pipe, near the sealed end, we put a piece of shining wood, wedged in with a piece of cork, to keep it from falling; and having inverted the nose of it into another slender glass, but not cylindrical, wherein was pretty store of quicksilver, we put them both into a long receiver shaped almost like a glass churn; and having pumped a while, that the air included in the pipe, expanding itself, might depress the quicksilver, and so make escapes into the receiver as long as we thought fit; we then let in the outward air that the stagnant quicksilver might be impelled into the cavity of the pipe, now freed from much of the air, to the height requisite for our purpose.

THIS done, we plied the pump again, and observed that, as the air in the pipe did by its own spring expand itself more and more and grow thinner and thinner, the shining wood grew dimmer and dimmer, till at length it ceased to shine, the internal air being then got a good way lower than the surface of the external quicksilver; whereupon opening the commerce between the cavity of the receiver and the atmosphere, the quicksilver was driven up again, and consequently the air above it was restored to its former density; upon which the rotten wood also recovered its light:—what the greatest expansion of this air was we could not certainly determine, because the expansion raised the external quicksilver so high as to hinder us to see and measure it; but we guessed that the air reached to about a foot or more from the top of the pipe to the surface of the quicksilver near the bottom of it. But when that rarefied air was impelled into its former dimensions, we measured it, and found that the upper part of the tube, unpossessed by the quicksilver, was about three inches; and the wood being about an inch long, there remained two inches, or somewhat better, for the air. But this experiment is to be repeated when exacter instruments can be procured.

## EXPERIMENT X.

THINKING it fit to try, as well whether sinking fish that shines be of the same nature, as to luminousness, with rotten wood that shines too; as whether the withdrawing of the air will extinguish or eclipse the light of a considerable bulk of luminous matter, as in the experiments hitherto made we found it would do to a small one; we took a fish that we had kept and caused to be watched till it was almost all over luminous; though much more in the belly and some parts of the head than elsewhere; and having suspended him in a conveniently-shaped receiver, we found him to give so great a light, that we suspected beforehand that the withdrawing of the air would hardly have its full operation upon a body whose bulk was considerable, as well as its light very vivid, and which had many luminous parts retired to a pretty distance from the air. Accordingly, having exhausted the receiver as much as we were wont, it appeared, indeed, especially towards the latter end of the operation, that the absence of the air did

considerably lessen, and in some places eclipse the light of those parts that shone less strongly; but the belly appeared not much less luminous than before: wherefore, supposing that, upon the turning of the stop-cock the air coming in much more hastily than it could be drawn out, we should have the best advantage to discern what interest it had in the luminousness of the fish, we re-admitted it; and, upon its rushing in, perceived the light to be, as it were, revived and increased; those parts of the fish that were scarce visible before or shone but dimly, receiving presently their former splendour.

AND not to leave un-prosecuted the remaining part of the experiment, which was to try whether it was the kind of the luminous body, or only the greatness of the bulk, and the vividness of light, and, if I may so speak, the tenacity of the substance it resided in that made the difference between the fish and the wood, we put part of the fish of another kind that shone much more faintly than that hitherto spoken of, and but in some places; and by the withdrawing the air, we made some of the luminous parts disappear, and the others so dim as scarce to be discerned; and yet both the one and the other regained their former light upon the return of the air.

AND to pursue the experiment a little further, we put in such a piece of the first fish, as though it were bright, was yet but thin, and not considerably great; and upon pumping out the air we found it, according to our expectation, quite eclipsed, though it recovered its light upon the air's re-entry.

THESE, Sir, are the experiments I have lately made about the shining bodies in our engine. More I would have tried, notwithstanding the trouble we found in managing the engine in the dark, if rotten wood had not failed us, and I were not in a place where the glass-men's shops are not near so well furnished as the stationers.

I SCARCE doubt but these experiments will occasion among the virtuosi several queries and conjectures, according to the differing hypotheses and inquiries to which men are inclined: and, particularly, it is probable that some will make use of this discourse to countenance their opinion, that notwithstanding the coldness (at least as to sense) of fishes and other animals, there may be in the heart and blood a vital kind of fire which needs air, as well as those fires that are sensibly hot; which may lessen the wonder that animals should not be able to live when robbed of air: and if I had now time, I could possibly furnish you with some other trials that seem much to favour the comparison, though, as to the opinion itself of a vital flame, I shall not now tell you my thoughts about it. And though not only the Cartesians will perhaps draw an argument from the past phenomena in favour of their theory of light, but divers others will discourse upon them, and propose further questions, and perhaps inquiries, suitable to their several hypotheses; yet I shall content myself at present to have faithfully delivered the historical part of these appearances, without making, at least at this time, any reflections on them. And the rather, indeed, because I enjoyed so little health when I was making the experiments, that it was not fit for me to engage in speculations that would much exercise my thoughts; which, I doubt, have been more gratified than my health hath been by the bare trials, which are most seasonably made at hours unseasonable for one that is not well.

*Postscript*



*Postscript sent by the same noble Author from the same place, December 6, 1667.*

*My condition, in point of health, being not much improved since I writ to you in October last, when I shall have added, that I have not these five or six weeks been able to procure any shining wood (except one single piece, which though large, was so ill conditioned that it afforded me but one trial) you will not, I hope, expect that I should add much to the experiments I formerly sent you about the relation betwixt light and air. But however, since the subject is new and noble, and since your curiosity about other matters has been so welcome and useful to the Virtuosi, I shall not decline, even on this occasion, to comply with it; and the rather, because I had promised you some additions a good while since, and because too, that though what I shall acquaint you with may seem to be but a confirmation of two or three of the former experiments, yet, besides that it is of them which most needed a confirmation, these trials will also afford some circumstances that will not, I think, be unwelcome.*

#### EXPERIMENT XI.

To examine then the conjecture mentioned in the last experiment, that the durability of the light in the shining fish, in spite of the withdrawing the air, might proceed in great part from the vividness of it, and the beauty of the matter it resided in, rather than from the extent of the luminous body in comparison of the small pieces of shining wood I hitherto had made my trials with; I put in the above-mentioned piece of wood, whose luminous superficies might be perhaps ten or twelve times as great as that which the eye saw at once, of the surface of such fragments of shining wood as I was wont to employ; and though some parts of this large superficies shined vividly (for the light was usually enough for rotten wood inferior to that of our fish) yet this great piece, being put into a convenient receiver, was, upon the withdrawing of the air, deprived of light, as the smaller ones had been formerly; the returning air restoring its light to the one, as it had done to the other.

#### EXPERIMENT XII.

BUT this is not the chief thing I intended to acquaint you with; that being the success of some trials which we made in prosecution of these two neighbouring experiments.

IN the first of these I told you I had been able to try but for half an hour, or a little more, that a shining piece of wood deprived in our engine of light, would yet retain a disposition to be, as it were, rekindled upon the fresh access of the air; wherefore, though I could have wished to have made a further trial with the same kind of bodies, yet being able to procure none, I substituted in their room small pieces of rotten fish that shone, some of them more faintly, and some of them more vividly, in reference to one another, but none as strongly as some that I could have employed: and having, in a very small and clear receiver, so far drawn off the air as to make the included body disappear, we so ordered the matter, that we kept out the air for about 24 hours; and

and then allowing the air to re-enter in a dark place, and late at night, upon its first admittance the fish regained its light.

### EXPERIMENT XIII.

THIS, compared with some of my former observations about putrefaction, put me upon a trial, which, though it miscarried, I shall here make mention of, that in case you, who are better furnished with glasses, think it worth while, you may get reiterated by the Society's operator. Considering then, how great an interest putrefaction hath in the shining of fishes, and air in the phenomena of putrefaction, I thought it might be somewhat to the purpose to take a fish that was, according to the common course I had observed in animals, not far from the state at which it would begin to shine; and having cut out a piece of it, I caused the rest to be hung up again in a cellar, and the expected piece to be put into a small and transparent receiver, that we might observe, if a day or two, or more, after the fish in the cellar should begin to shine, that in the exhausted receiver would either also shine, or (because that seemed not likely) would, notwithstanding the check which the absence of the air might be presumed to give the putrefaction, be found to shine too, either immediately upon the admission of the air, or not long after it.

BUT this experiment, as I lately intimated, was only designed and attempted, not completed, the receiver being so thin, that upon the exhaustion of the internal air the weight of the external broke it; and we could ill spare another of that kind from trials we were more concerned to make: notwithstanding which we made one trial more, which succeeded no better than the former, but miscarried upon a quite differing account, *viz.* because neither the included piece of fish, nor the remaining, though it were of the same sort with the fishes I usually employed, would shine at all, though kept a pretty while beyond the usual time at which such fishes were wont to grow luminous.

If this experiment had succeeded, I had some others to try in prosecution of it, which I shall not now trouble you with the mention of. But that this paragraph may not be useless to you, I will take this occasion to give you a couple of Advertisements that may relate not only to this experiment, but also more generally to those, whether precedent or subsequent, where shining fish are employed.

### ADVERTISEMENT I.

IN the first place then, I will not undertake that all the experiments you shall make with rotten fish shall have just the same success with these I have related. For, as I elsewhere observed (in a discourse written purposely on that subject) that the event of divers other experiments is not always certain; so I have had occasion to observe the like about shining of fishes. And besides what I lately took notice of at the close of the tenth Experiment, I remember, that having once designed to make observations about the light of rotten fishes, and having in order thereunto caused a competent number of them to be bought, not one of them all would shine, though they were bought by the same person I was wont to employ, and hung up in the same place where I used to have them put, and kept not only till they began to putrefy, but beyond the time that others used to continue to shine; although a parcel of the same kind of fishes bought the week before, and another of the same kind bought not many days after, shined according to expectation.



expectation. What the reason of this disappointment was, I could not determine; only I remember that at the time it happened, the weather was variable, and not without some days of frost and snow. Nor is this the oddest observation I could relate to you about the uncertain shining of fishes, if I thought it necessary to add it in this place.

## ADVERTISEMENT II.

NOTICE must also be taken in making experiments with shining fish, that their luminousness is not wont to continue very many days. Which advertisement may be therefore useful, because without it we may be apt sometimes to make trials that cannot be soon enough brought to an issue; and so we may mistake the loss of light in the fish, to be a deprivation of it caused by the experiment; which indeed is but a cessation, according to the usual course of nature.

## EXPERIMENT XIV.

I KNOW not whether you will think it worth while to be told of a trial that we made to save those criticks a labour, that else might perhaps demand why it was not made. We put therefore a piece of shining fish into a wide-mouthed glass, about half filled with fair water, and having placed this glass in a receiver, we exhausted the air for a good while, to observe whether when the pressure of the air was removed, and yet (by reason of the water that did before keep the air from immediately touching the fish) the exhaustion of the receiver did not deprive the fish of that contact of air, which it had lost before; whether, I say, in this case the absence of the air would have the same influence on the shining body, as in the former experiments; and here, as far as the numerous bubbles excited in the water would give us leave to discern it (for they did, though not unexpectedly, somewhat disturb the experiment, which inconvenience we might have prevented, if we had thought it worth while) we could not perceive, that either the absence or return of the air had any great operation upon the light of the immersed body: which yet did not keep me from intending to make a somewhat like trial with shining wood (when I can get any) fastened to the lower part of a clear glass, and covered over, but not very deep, with quicksilver. Of which practice I shall not now stay to give you the reasons, having elsewhere fully enough expressed them.

AND that this Section may acquaint you with something besides the (seemingly) insignificant experiment related in it, I shall here inform you (since I perceive I did not in the first papers I sent you) that though when I formerly put together some notes about luminous bodies, I confined not my observations to one or two sorts of fishes, yet the experiments sent you since *October* last, were all of them (except a collateral one or two) made with whittings, which, among the fishes I have had occasion to take notice of, is (except one sort that I cannot procure) the fittest for such trials, and consequently fit to be named to you, to facilitate their future ones, in case you think it requisite to make any upon such subjects.

EXPE-

## EXPERIMENT XV.

THE other of the two neighbouring experiments I lately mentioned (*viz.* the ninth) I told you, when I sent it you, needed a reiteration to confirm it, since we had but once tried it (and that without all the conveniency we desired) that a shining body, which upon the first withdrawing the air loseth much, but not all its light, may be deprived of the rest by continuing in that unfriendly place, though the air be no farther exhausted. To prosecute therefore both the experiments in one trial, we took somewhat late at night a piece of rotten fish, which we judged to shine too strongly to be quickly deprived of all its light, and having put it into a small and clear receiver, we found (as we had foreseen) that the light was much impaired, but nothing near suppressed by the withdrawing of the air. Wherefore, having removed the receiver into a convenient place, I caused it to be brought to me about midnight (after I was a-bed) and having by close drawing the curtains, and other means, made the place pretty dark, I perceived the included body to continue to shine more vividly than one would have expected (and, if I mistake not, I saw it shining in the morning, whilst it was dark); but the night after, coming to look upon it again, its light appeared no more: notwithstanding which I made a shift to keep out the air about 24 hours longer, and so after 48 hours in all, we opened the receiver in a dark place, and presently upon the ingress of the air were pleasingly saluted with so vivid an apparition of light, that the included body continued to shine when carried into a room where there was both fire and candle, if it were but by a hat screened from their beams.

BEING encouraged as well as pleased with this success, we forthwith exhausted the air once more out of the same receiver, and having kept it about four hours longer, we looked upon it again in a dark place, and finding no appearance of light, let the air in upon it, whereby it was made to shine again, and that vigorously enough, so that I caused the receiver to be exhausted once more; but that it being *Sunday* night, I was unwilling to scandalize any, by putting my servants upon a laborious and not necessary work.

THE suddenness with which the included body appeared to be, as it were, re-kindled upon the first contact of the air, revived in me some suspicions I have had about the possible causes of these short-lived apparitions of light (for I speak not now of real lamps found in tombs, for a reason to be told you another time) which disclosing themselves upon men's coming in, and consequently letting in fresh air into vaults, that had been very long close, did soon after vanish. These thoughts, as I was saying, occurred to me upon what I had been relating, by reason of the sudden operation of the fresh air upon a body, that but a minute before disclosed no light. For though the lights reported to have been seen in caves quickly disappeared, which that of our fish did not; yet that difference might possibly proceed from the tenacity, or some other disposition of the matter wherein the luminousness of the fish resides. For I remembered that I had more than once observed a certain glimmering and small light to be produced in a sort of bodies upon putting them out of their former rest, and taking them into the air, which sparks would vanish themselves sometimes within one minute, sometimes within a few minutes. But as these thoughts were but transient conjectures, so I shall not entertain you any longer about them, but rather contenting myself with the hint already given, take notice of what may be more certainly deduced from our experiment; which is, that the air may have a much greater interest in divers odd phenomena of nature, than we are hitherto aware of.

AND



And for confirmation of our experiment I shall add, that, having in another receiver eclipsed a piece of fish that shone, when it was put in, more languidly than divers others that we had tried, I kept it about three days and three nights in a receiver, which (receiver) being somewhat like another, at first suggested to me, when I came to take it, some scruple; but afterwards, upon farther examination, concluded it to be the same: wherefore I opened it in the dark, and upon letting in the air on this body, that shined but faintly at first, it immediately recovered its long suppressed light. And having included another piece, that was yet more faint than this when it was put into the receiver, I thought fit to try at once the experiment hitherto confirmed, and the converse of it. And therefore having kept this piece also three days and three nights in the exhausted glass, I let in the air upon it, and notwithstanding the darkness of the place, nothing of light was thereupon revived. But this being little other than I expected from a body that shined so faintly when it was put into the receiver, and had been kept there so long, I resolved to exercise my patience a while as well as my curiosity, and try whether the appulse and contact of the air would have that operation after some time, that it had not at first; and accordingly, after having waited a while, I observed the fish to disclose a light, which, though but dim, was manifest enough; but having considered it for some time, I had not leisure to watch whether it would increase, or how long it would continue.

I know not, Sir, whether you are weary with reading, but I am sure I am quite tired with making so many experiments upon one subject; and therefore I shall here conclude this paper, as soon I have added this confirmation, as well of what I last related, as of something that I observed before, that having included in some receivers two pieces of rotten whittings, whereof the one, before it was put in, scarce shone so vividly, as did the other after the receiver was exhausted; and having ordered the matter so, that we were able to keep out the air for some days, at the end of about 48 hours we found, that the more strongly shining body retained yet a deal of light. But afterwards looking upon them both in a dark place, we could not perceive in either any show of light. Wherefore having let in the air into that receiver, whereinto the body that at first shined the fainter had been put, there did not ensue any glimmering of light for a pretty while; nay, upon the rushing in of the air into the other glass (then also made accessible to the atmosphere) the body that at first shone so strongly, and that continued to shine so long, shewed no glimmering of light. But being resolved to expect the issue a while longer, our patience was rewarded in less than a quarter of an hour with the sight of a manifest light in the body last named; and a while after the other became visible, but by a light very dim. The more luminous of these bodies I observed to retain some light twenty-four hours after: and the hitherto recited experiment had this peculiar circumstance in it, that the two receivers were uninterruptedly kept exhausted no less than four days, and as many nights.\*

\* What method the noble author of these experiments used in keeping out the air for so long a time, will probably be made known ere long by himself.

## OBSERVATIONS and TRIALS about the Resemblances and Differences between a BURNING COAL and SHINING WOOD.

First Printed in the *Philosophical Transactions*, No. XXXII. p. 605.

For Monday, February 10, 1667.

*THESE particulars were already in our hands, when we published the experiments made on shining wood and fyb, in the last papers, imparted then by the same noble author that those were; but wanted then room enough to contain these, which now follow, as they were sent in a letter from Oxford, viz.*

AND now, Sir, seeing the want of shining wood hath kept me ever since I sent you the former experiments from making any new ones on that subject, I shall, by way of amends, subjoin some of the observations that I heretofore intimated to you I had made of the resemblances and differences between a live coal and a piece of shining wood; in perusing of which you will easily discern, that to those particulars, which my memory and the former observations, I had noted down about light and luminous bodies, had suggested to me, I have added some that have been afforded me by those late trials made in my engine, whereof I sent you an account.

### R E S E M B L A N C E S.

THE things wherein I observed a piece of wood and a burning coal to agree or resemble each other, are principally these five:

1. *Both of them are luminaries, that is, give light, as having it (if I may so speak) residing in them; and not like looking-glasses or white bodies, which are conspicuous only by the incident beams of the sun, or some other luminous body which they reflect.*

THIS is evident, because both shining wood and a burning coal shine the more vividly, by how much the place, wherein they are put, is made the darker by the careful exclusion of the adventitious light. It is true that the moon and Venus appear brightest at or about midnight, and yet have but a borrowed light; but the difference between those planets and the bodies we treat of, in reference to the difficulty we are considering, is obvious enough. For though the beholder's eye, that looks upon those stars, be advantaged by being in the dark, which enlarges the pupil of the eye, yet the object itself is freely exposed to the beams of the sun; which, if they were intercepted, those planets would quickly be darkened, as experience manifests in eclipses.

2. *Both*



2. *Both shining wood and a burning coal need the presence of the air, and are too of such a density, to make them continue shining.*

THIS has been proved as to a coal, by what I long since published in my *Physico-mechanical Experiments*, where I relate how quickly a coal would be extinguished upon the withdrawing the air from about it: and as to shining wood, the experiments I lately sent you, make it needless for me to add any other proof of the requisiteness, not only of air, but of air of such a thickness, to make its light continue. How far this is applicable to flame, it is not necessary here to determine; though when I have the satisfaction of seeing you again, I may tell you something about that question, which perhaps you do not expect.

3. *Both shining wood and a burning coal, having been deprived for a time of their light, by the withdrawing of the contiguous air, may presently recover it by letting in fresh air upon them.*

THE former part of this particular, trials have often shown you to be true, when kindled coals, that seem to be extinguished in our exhausted receivers, were presently revived when the air was restored to them: and the latter part is abundantly manifest by the experiments, to which this paper is an appendance.

4. *Both a quick coal and shining wood will be easily quenched by water and many other liquors.*

THE truth of this, as to coals, is too obvious to need a proof; and therefore I shall confirm it only as to wood. For which purpose you may be pleased to take the following transcript of some of my notes about light.

I TOOK a piece of shining wood, and having wetted it with a little common water in a clear glass, it presently lost all its light\*.

THE like experiment I tried with strong spirit of salt, and also with weak spirit of sal armoniac; but in both the light did, upon the wood's imbibing of the liquor, presently disappear.

AND lest you should think that in the words, *many other liquors*, I intended not to comprise any that consist of soft and unctuous parts, or that are highly inflammable, I shall subjoin a couple of notes that I find next to those just now transcribed.

I MADE the like trial with rectified oil of turpentine, with a not unlike success. The same experiment I tried more than once with high rectified spirit of wine, which did immediately destroy all the light of the wood that was immersed in it; and having put a little of that liquor with my finger upon a part of the whole piece of wood, that shone very vigorously, it quickly did, as it were, quench the coal, as far as the liquor reached; nor did it in a pretty while regain its luminousness (which whether it recovered at all, I know not; for this trial being made upon my bed, I fell asleep before I had waited long enough to finish the observation).

5. *As a quick coal is not to be extinguished by the coldness of the air, when that is greater than ordinary, so neither is a piece of shining wood to be deprived of its light by the same quality of air.*

As much of this observation as concerns the coal will be readily granted; and for proof of the other part of it, I could relate to you more trials than one, but that I suppose one may suffice, circumstanced like that which I shall now relate.

\* From hence you will easily gather the reason why, when I lately told you of the trial I made with a piece of shining fish under water in the unexhausted receiver, I did not propose to have the like trial made with shining wood and water, but for this liquor substituted mercury.

## *Observations about the Differences between*

I took a small piece of shining wood, and put it into a slender glass-pipe, sealed at one end, and open at the other, and placed this pipe in a glass-vessel, where I caused to be put a strongly frigorifick mixture of ice and salt; and having kept it there full as long as I thought would be requisite to freeze an aqueous body, I afterwards took it out, and perceived not any sensible diminution of its light. But to be sure the frigorifick mixture should not deceive me, I had placed by this pipe another almost filled with water, which I found to be turned into ice; and though I suffered the wood to remain a pretty while after, exposed to so intense a cold, yet when I took it out, it continued shining, and, if I mistake not, it ceased not to do so, when I looked on it, twenty-four hours after. But though the light of shining fish be usually (as far as I have observed) more vigorous and durable than that of shining wood; yet I cannot say that it will hold out against cold so well as the other. For having ordered one of my servants to cut off a good large piece of the luminous whiting, and bury it in ice and salt, when I called for it in less than half an hour after, I found it much stiffened by the cold, and to have no light, that I could discern in a place dark enough. And for fear that this effect may have proceeded not barely from the operation of the cold, but also from that of the salt (for which suspicion you would see reason enough, if I could shew you my trials about shining fish) I caused another time a piece of whiting to be put in a pipe of glass sealed at one end, and having seen it shine there, I looked upon it again, after it had stayed but a quarter of an hour, by my estimate, in a frigorifick mixture, which the glass kept from touching the fish; and yet neither I, nor a youth that I employed to look on it, could perceive in a dark place, that it retained any light; which whether the cold had deprived it of by that great change of texture, that the congelation of the aqueous juice of the fish (which I have several times observed to be luminous) may be supposed to have made in the body invaded by it; or whether the effect depend more principally on some other cause, I shall not now examine.

### D I F F E R E N C E S.

1. *The first difference I observed betwixt a live coal and shining wood, is that whereas the light of the former is readily extinguishable by compression (as is obvious in the practice of suddenly extinguishing a piece of coal by treading upon it) I could not find that such a compression as I could conveniently give, without losing sight of its operation, would put out or much injure the light even of small fragments of shining wood: one of my trials about which I find thus set down among my notes about light.*

I took a piece of shining wood, and having pressed it between two pieces of clear glass (whereof the one was pretty flat, and the other convex) so that I could clearly see the wood through the glass, I could not perceive that the compression, though it sometimes broke the wood into several fragments, did either destroy or considerably alter the light.

This experiment I repeated with the same success. But what a stronger or more lasting compression may do in this case, I had not opportunity to try.

2. *The next unlikeness to be taken notice of betwixt rotten wood and a kindled coal is, that the latter will in a very few minutes be totally extinguished by the withdrawing of the air; whereas a piece of shining wood, being eclipsed by the absence of the air, and kept so for a time, will immediately recover its light, if the air be let in upon it again within an hour after it was first withdrawn.*



THE former part of this observation is easily proved by the experiments that have been often made upon quick coals in the pneumatical engine; and the truth of the latter part appears by an experiment about shining wood made by us in *October* last. Neither is it improbable that if I had had the conveniency to try it, I should have found that a piece of shining wood, deprived of its light by the removal of the ambient air, would retain a disposition to recover it upon the return of the air, not only for half an hour (which is all that I lately asserted) but for half a day, and perhaps a longer time.

3. *The next difference to be mentioned is, that a live coal being put into a small close glass will not continue to burn for very many minutes, but a piece of shining wood will continue to shine for some whole days.*

THE first part of the assertion I know you readily grant; and the rather, because it contains matter of fact, without at all determining, whether the coal's not continuing to burn proceeds from its being, as it were, stifled by its own smoak and exhalations (which can have no vent in a small close glass) or from the want of fresh air, or from any particular cause, which I must not here debate; though I have sometimes made experiments somewhat odd, to facilitate that enquiry. The other part of our observation may be easily made out by what I tried upon shining wood, sealed up hermetically in very small glasses, where the wood did for several days (though I remember not precisely how many) retain its light.

4. *A fourth difference may be this; that whereas a coal, as it burns, sends forth store of smoke or exhalations, luminous wood does not so.*

5. *A fifth, flowing from the former, is, that whereas a coal in shining wastes itself at a great rate, shining wood does not.*

THESE two unlikelinesses I mention together, not only because of their affinity, but because what concerns the coal in both, will need no proof; and as for what concerns rotten wood, it may be verified by an observation, that, I find by my notes, I made in a small clear glass; where, after it had continued luminous some days, I looked on it in the day-time to perceive if any store of spirits or other steams had, during all that while, exhaled from the wood; but could not find any on the inside of the glass, save that in one place there appeared a kind of a dew, but consisting of such very small drops (if at least their size were not below that name) that a multitude of them would go to the making up of one ordinary drop. But in pieces of shining fish I found the case much otherwise, as was to be expected.

6. *The last difference I shall take notice of betwixt the bodies hitherto compared, is, that a quick coal is actually and vehemently hot; whereas I have not observed shining wood to be so much as sensibly lukewarm.*

WHAT is said of the coal's heat being as manifest as its light, I shall need only to make out what relates to the shining wood. To assist me wherein, I meet among my notes that, whose transcript I shall subjoin, when I have premised that (if my memory do not deceive me) the piece of wood to be mentioned was one that shone so vividly, that waking in the night some hours before I tried it, and perceiving, as it lay near me on the bed, how luminous it was, I was invited to reach out to a place near the bed's-head, where there stood several books, and laying the wood on that which came to hand, I could discern by the light of it, that the book was an Hebrew bible, and that of the page I lighted on, the wrong end was turned upwards: to which intimation having added, that the little glass instrument, mentioned in the note, is such an one as you may find described in my preliminaries to the history of cold, save that part of this was a little bending inward at the basis, that it may sometimes stand by itself, and sometimes receive a small body into the dimple at its basis; having, I say, premised this,

this, and that as shining wood did not feel at all warm to me, so I also found shining fish palpably cold, I shall conclude your trouble with the premised note, which speaks thus :

[I PUT upon a large piece of wood which was partly shining, and, as near as I could, upon one of the most luminous parts of it, one of those thermoscopes that I make with a pendulous drop of water. But as I had formerly tried, that by laying the tip of my nose or finger upon it, when it shone vividly enough to enable me to discern both the one and the other, at the time of contact I could not perceive the least of heat, but rather an actual coldness ; so by this trial I could not satisfy myself that it did visibly raise the pendulous drop, though the instrument were so tender, that by approaching one finger near it, yet without actually touching of it, it would manifestly be impelled up, and upon the removal of my finger, would presently descend again.]

AND I remember, that having put such an instrument upon a shining fish that was pretty large, I could not perceive that it had any degree of heat, but rather the contrary. For having divers times taken off the glass to apply it with the more advantage to several parts of the luminous fish, I divers times (for I remember not whether it were always) took notice that upon the removal of the glass into the air, the pendulous drop would manifestly rise a little, and subside again, when the glass was applied to the fish. But whether this part of the experiment will hold in all temperatures of the air, I had not opportunity to try.





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A  
CONTINUATION  
OF  
NEW EXPERIMENTS  
PHYSICO-MECHANICAL,  
TOUCHING THE  
SPRING and WEIGHT of the AIR, and their EFFECTS.  
THE FIRST PART.

Written by way of LETTER, to the Right Honourable the  
Lord CLIFFORD AND DUNGARVAN.

WHERE TO IS ANNEXED

A Short Discourse of the ATMOSPHERES of CONSISTENT BODIES.

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The P R E F A C E.

HAVING at the beginning of the treatise, whereof this is a continuation, acquainted my readers with several things that belong, in common, as well to the following experiments, as to those there published; it will not be necessary for me to trouble the reader with a repetition of what he may have met with there already, nor to acquaint him in this address with any other particulars than those that concern the experiments I am now about to present him.

I doubt not but it will be remembered by some, that I seemed in the above-mentioned book to have promised a second part to it, or a large appendix to it; but intima-

mations of that kind do many times respect only the thing itself, leaving the giver of them free in point of time; and I wanted not sufficient inducements to delay a while to perform my promise, if I made any. I had, indeed, partly before the book already referred to came from the press, and partly some time after, made divers other trials, in order to a supplement of it: but being obliged to make some journeys and removes, which allowed me no opportunity to prosecute the experiments, I had made no very great progress in my design before the convening of an illustrious assembly of virtuosi, which has since made itself sufficiently known under the title of *THE ROYAL SOCIETY*. And having then thought fit to make a present, to persons so like to employ it well, of the great engine I had then made use of in the physico-mechanical experiments about the air; and being unable afterwards to procure another so good, I applied my studies to other subjects, and gave over, for a great while, the care of making more experiments of that kind; and the rather, because that finding by the very favourable reception those I had published had met with among the curious in several parts of *Europe*, that they were like to be considered and perused, I thought I might safely leave the prosecution of them to others, who would probably come more fresh and untired to such an exercise of their curiosity.

BUT observing that the great difficulties men met with in making an engine that would exhaust and keep out a body so subtle as air, and so ponderous as the atmosphere (besides, perhaps, some other impediments) were such, that in five or six years I could hear but of one or two engines that were brought to be fit to work, and of but one or two new experiments that had been added by the ingenious owners of them; I began to listen to the persuasions of those that suggested that unless I resumed this work myself, there would scarce be much done in it. And therefore having (by the help of other workmen than those I had unsuccessfully employed before) procured a new engine, less than the other, and differing in some circumstances from it, we did (though not without trouble enough) bring it to work as well as the other, and, as to some purposes, better; and having once got this, I made haste to try with it those experiments that belonged to the designed continuation, and do now make up this book.

I HOPE that to such readers as the following papers are principally intended for, I shall not need to make an apology either for the plainness of my style (wherein I aimed at perspicuity, not eloquence) or for my not having adorned or stuffed this treatise with authorities, or sentences of classic authors, which I had neither the leisure to seek, nor thought I had any great need to employ, though it had been far more easy, than perhaps it would have proved, to borrow from them things that would have been very proper to a treatise, where my main design was to make out, by practical experiments, divers things, among others, that have not hitherto been advantaged by that way of probation, nor perchance thought very capable of it; so that I shall have obtained a great part of what I aimed at, if I have shewn that those very phenomena, which the school-philosophers and their party urge, and sometimes triumph in, as clear proofs of nature's abhorrence of a vacuum, may be not only explicated, but actually exhibited, some by the gravity, and some also by the bare spring of the air; which latter I now mention as a distinct thing from the other; not that I think it is actually separated in these trials (since the weight of the upper parts of the air does, if I may so speak, bend the springs of the lower) but because that having in the already published experiments, and even in some of these, manifested the efficacy of the air's gravitation on bodies, I thought fit to make it my task in many of these, to shew that most of the same things that are done by the pressure of all the superincumbent atmosphere acting as a weight,

may



may be likewise performed by the pressure of a small portion of air, included indeed (but without any new compression) acting as a spring.

THE present first part of our continuation might, I confess, have been, not inconveniently, divided into two parts. For first, it contains some experiments that are already related in the printed book, though they be here so repeated as to be confirmed, illustrated, or improved, by being reiterated either with better instruments, or with better success, than when they were made in my large receiver, which holding (if I misremember not) about eight gallons, could not easily be so well exhausted as those small receivers I often since employed. And secondly, the other, and far more numerous sort of experiments, related in this first part, are new, and superadded. And yet I forbear to assign each of these two sorts a place by itself, because I could not conveniently set down my trials otherwise than as they came to hand among my notes; and I considered that in divers places the new ones and the old ones being mentioned together, might serve by their neighbourhood to illustrate or confirm each other. And however, at another edition of our Continuation, it will be a very easy task, if it appear to be a requisite one, to give the improvements of the former experiments, and the super-added new ones, distinct titles and places.

As for the mechanical contrivances I employed in making the following experiments, though most of them have had the good fortune to meet with an approbation, and some of them with more than that, from no mean virtuosi and mathematicians, yet as I expect that critical readers will judge that in some experiments more artificial instruments might have been made use of, so I hope that they will not look upon those I was reduced to employ, as always the best that ever I could have directed, since it sufficiently appears by divers passages of the following experiments, that they were not made at *London*, but in places where the want of a glass-house, and other accommodations reduced me to make my trials not after the best manner I could devise, but in the best way I could then and there put in practice. And let me add on this occasion, to what I have elsewhere said to the like purpose, that it is both a great discouragement to many ingenious men, and no small hindrance to the advancement of natural philosophy, that some nice criticks are so censorious in exacting from attempters the very best contrivances, and many that would be attempters, stand too much in awe of such mens judgments; for though in very nice experiments the exactness of instruments is not only desirable and useful, but, in some cases, necessary; yet in many others, where the production of a new phenomenon is the thing aimed at, they are to be looked upon as benefactors to the history of nature, that perform the substantial part of a discovery, though they do it not by the most easy and compendious ways deviseable, or attain not to the utmost preciseness that might be wished, and is possible. For such performances, notwithstanding their being short of perfection, make discoveries to the world of new and useful things; which though others, that are more lucky at contrivances, and have better accommodations, may compass by more compendious ways, or with greater preciseness, yet still the world is beholden to the first discovery for the improvement of it, as we are to *Archimedes* for the first devising a way to find, by weighing bodies in water, how much gold or how much silver a mixture of those metals does contain, though (if historians have not injured that great man in the relation) he went a more laborious and less accurate way to work than modern hydrostaticians, who (as I elsewhere shew) may perform the same thing by a far better way, which yet, probably, we should not have thought of, if that attributed to *Archimedes* had not preceded and afforded us a fundamental notion. And that the not being so dexterous at contriving the ways to effect a thing, is no sure argument that a man has not a true and

solid knowledge of it, we may easily learn from *Euclid*, whom our geometricians generally and justly acknowledge to be their master, and to have enriched the world with many useful truths, and solidly demonstrated all his propositions, though divers of his modern commentators have found out more compendious ways for effecting several of his problems, as well as of demonstrating divers of his theorems, especially since the excellent invention of specious algebra, by whose help that accurate mathematician Dr. *Wallis* has, besides other specimens upon intricate propositions, clearly demonstrated the ten first and for the most part perplexing theorems of the second element, in little more than as few lines. In sum, in experiments that are very nice, accurate contrivances and instruments are industriously to be sought, and highly to be valued; and even in such other experiments, as are frequently to be reiterated, the most commodious and easy ways of performing them are very desirable: but those practical compendiums, though very welcome to them that would repeat trials, are not so important to the generality of readers, as being but useful to save pains, not necessary to discover truths, to which men may oftentimes do good service without any peculiar gift at mechanical contrivances, since in most cases they may be looked upon as promoters of natural philosophy, who devise experiments fit to discover a new truth, if the attempt succeeds, and propose ways of bringing it to trial, which, though perhaps not the most skilful or expeditious, are yet sufficient and practicable, the increase of physical knowledge being the product of the things themselves that are discovered, whatever were the instruments men employed about making the discoveries.

As for the cuts I endeavoured to make their relations and descriptions of most of the experiments so full and plain, as to need as few schemes as might be to illustrate them: but though I hope that they who either were versed in such kind of studies, or have any peculiar facility of imagining, would well enough conceive my meaning only by words; yet lest my own accustomance to devise such trials, and to see these made, should make me think them more easily intelligible than most readers will find them, I advised with a learned friend or two, fit to be consulted on such an occasion, what experiments were requisite to be illustrated with diagrams, and to such I took care they should be annexed. Only I forbore to add to the figure of each instrument alphabetical explanations of its parts, as judging that troublesome work less easy for me, than it would be for such readers as this tract is designed for, to understand what is delivered by the help of a little attention in conferring the schemes of the instruments with the verbal accounts of the experiments they relate to. But there is one particular about the cuts may require both to be given notice of and excused; which is, that having occasion to alter the method of my experiments, when I began to foresee that I should be obliged to reserve divers things for another opportunity; and being myself absent from the engraver for a good part of the time he was at work, some of the cuts were misplaced, and not graven in the plates, in which, according to the present series of experiments, they might most properly have been put.

BUT perhaps I may (for I am not sure of it) more need the reader's pardon for (unknowingly) troubling him in this continuation with some passages, that he may have already met with in the book it refers to; which, though I had not read over for some years before, I chanced not to have at hand, when divers of the following papers were written; and though afterwards I recovered it, yet the indisposition of my eyes made me think it unfit rather to tire them by reading over the whole book, than to trust to the reader's good nature (in case I should need it) for the pardon of a few unintended repetitions.



I DOUBT not many readers will be inquisitive to know why this treatise is stiled the first part of a Continuation. To give these some account of the title, I must put them in mind, that in the already published experiments I intimated that two sorts of trials might be made by the help of our engine: the one, such as needed but a short absence of the air, and the other such as required that the air should not only be withdrawn for a while, but kept out for a considerable time, from the bodies whereupon the trial is made. Of the former sort of experiments are these this present book does (as well as that heretofore published did) consist of. And though I have been so much called upon, and troubled for certain writings, whereof I have made such mention in those that passed the press, as some readers interpreted to be an engagement, that it made me think fit, when I satisfied their demands, to be thenceforward very shy of making the publick any promise; yet I was induced not to alter the title of this treatise, partly because it may intimate to the curious, that there are yet a great many things to be performed by our engine, besides the productions of it I have hitherto presented them; and partly because though I still persist in my former averfeness to make promises to the world; yet it is very possible, that if God grant me life and health, I may, in due time, present my friends with what may serve for a second part of our Continuation, consisting of experiments that require a longer absence of the air from the bodies to be wrought upon: and I shall think if this first part prove not unacceptable to the curious, that the latter will be not unwelcome to them, as being designed to consist of sets of experiments, which by their being most of them new, and some of them odd enough, may perchance afford some not despicable hints to the speculative. But the very nature of these experiments requiring that some of them should be long in making, my friends could not reasonably expect a quick dispatch of work of this kind, though I should not meet for the future with such intervening impediments, as have hitherto disturbed it (as want of instruments, of health, of leisure, and of the liberty, which is so requisite in this case, of staying long enough in one place): notwithstanding all which difficulties I have by snatches been able, through God's blessing, to make forty or fifty of designed trials, being such as require the least of time to be performed in, though I now think not fit to mention any of them, as well for other reasons, as because though they be made by the help of our engine, yet they require a peculiar apparatus of instruments, very differing from those we have hitherto mentioned, and not to be intelligibly described without many words and divers figures. In the mean time, lest the industrious should be discouraged by a surmise, that there is nothing left for them to do by the help of our engine, at least as to the first sort of experiments, I shall inform them, that I had thoughts to have added divers others of that kind to these that now come forth, and particularly two clusters of pneumatical trials, the one about respiration, and the other about fire and flames; but several of my notes and observations being at present out of the way, my having neither health nor leisure to repair these inconveniencies, and prosecute trials of that sort with any assiduity, makes me chuse rather to reserve them for an appendix, than to make those that now come abroad stay for them; which will not, I presume, be the more disliked, because by taking this course I may, in delivering of the phaenomena of nature, imitate nature herself, of whom it is the Roman philosopher's saying, *rerum natura sacra sua non simul tradit.*

Seneca  
quest nat.  
lib. 7.  
c. 31.

*Some ADVERTISEMENTS touching the ENGINE itself.*

**T**HOUGH the engine already published, and that which I employed in the following trials, have the same ules, and agree both in the ground and the main part of their construction, yet they differ in some particulars fit to be taken notice of: for after I had presented the great engine I formerly made use of to the Royal Society, partly the difficulty of procuring such another of that size and make, and partly the desire of making some improvements, invited me to make some alterations in the structure; some of them suggested by others (especially by the ingenious Mr. *Hook*) and some of them that I added myself, as finding that without them I could not do my work. Wherefore it will not be amiss to point at the chief differences between the former and the latter engine, and to intimate some of the conveniences and inconveniences that attend them.

As for the construction of the second engine itself, since it is presumed that the readers of this book have already perused that of which this is a Continuation, and understood the contrivance of the instrument that belongs to it, it was presumed sufficient to exhibit in the first plate the delineation of the entire engine ready to be set at work; and in the second, the figure of the several metalline parts, that compose it, before they are set together. For though these have not verbal and alphabetical explications annexed to them, yet the sight of them may suffice to make those that have an imagination fitted to conceive mechanical contrivances, and are acquainted with the former engine, comprehend the structure of this; which alphabetical explications would scarce make such readers do, as are not so qualified: only two things there are, which being of some difficulty, as well as of importance to be conceived, I shall here particularly take notice of. The first of them is, that in regard the sucker is to be always under water, and the perforation *p q*, that passes perpendicularly quite through it, and serves together with the stick *r s* for a valve, is to be stoppt at the bottom of the cylinder, as at *n o*; when it is full of water, it was requisite to make the stick *r p* of a considerable length, as two or three feet. The other and chief thing is, that in the second plate, the pipe *A B*, whose end *B* bends upward, is made to lie in a groove or gutter purposely made in the flat wooden board *c d e f*, on which the receivers are to rest; which square board I caused to be overlaid with very good cement, on which I took care to apply a strong plate of iron, of the bigness and shape of the board, leaving only a small hole for the erected part of the pipe to come out at, which I added, not only to keep the wooden board the better from warping, but because I knew (what will perhaps be thought strange) that the pressure of the atmosphere on one side of the board, when there is no pressure, or but very little, on the other, will enable many aerial particles to strain through the very wood, though of a good thickness, and imbued with oil to choak the pores. To this iron plate we sometimes fit a lip turning up about it, to hinder the water that on some occasions will come from the receiver from falling on the room; and (to add that upon the by) though the stop-cock *g b i k*, that belongs to the hitherto mentioned pipe, may be inserted at *I*, into the barrel or cylinder *l m n o*, by the help of soder, yet we chose as a much better way to have the branch *I*, of the  
stop-



stop-cock, made like a screw, which being once firmly screwed into the barrel, is not apt to be broken off, and may be more easily mended, if any thing happen to be out of order, which the engine is the most liable to be in or about the pipe; partly because it may fall out (though but very rarely, if due care be but taken) that the air will insinuate itself between the wooden board and the iron-plate, and so get up (where the pipe bends upwards) into the cavity of the receiver; and partly because the pipe being for a just reason made but slender, and the part of it that looks upwards very short, it happens not very unfrequently, that when we employ receivers with narrow orifices, where the cement must lie close to the opening of the pipe, it happens, I say, that the cement, especially if it be much softened by heat, is sucked (as they speak) into the pipe, and so choaks it up; or else that some part of the body included in the receiver is drawn to the orifice of the pipe, and lying upon it as a cover hinders the free passage of the air into the barrel; against which inconvenience, to add that upon the by, we use amongst other expedients to place just about the orifice of the pipe a small cover of tin, like that of a little box, which covers it at the top, to hinder any thing from lying immediately upon the pipe, and has a small opening or two in the side, to give the air of the receiver free access to the pipe.

THE square and hollow wooden part of this engine, discernible in the first plate, is so made, that it may contain not only the cylinder, but so much water as will always keep the cylinder quite covered with that liquor; by which means the sucker, lying and playing always under water, is kept still turbid and plump, and the water being ready at hand to fill up any little interval or chink that may happen to be between the sucker and the inside of the barrel, together with the newly mentioned plumpness of the sucker, very much conduce to the exact keeping out of the air. But this advantage is not without some inconvenience; for divers times, if great care be not taken in turning the stop-cock, the water will be impelled into the receiver, and much prejudice sundry experiments, when the included bodies are such that may be spoiled or impaired (at least for the present) by that liquor. The smallness of our cylinder is a convenience in regard of the facility it affords to make and dispatch those many experiments that may be performed in small receivers, though it make those more troublesome and tedious that require the exhaustion of large and capacious ones.

THE flat plate (mentioned a little above) has this great conveniency in many experiments, that the receiver needs no stop cock of its own; for such a vessel being made all of an entire piece of glass, and whelmed on upon the plate well covered with cement, can better keep out the air, than if there were a stop-cock, at which the air does but too frequently get in: but besides that in divers experiments such receivers do usually require to be wide mouthed, whereby a greater compass is to be fenced against the ingress of the air, several experiments cannot so conveniently be tried in this sort of receiver.

BUT because that though this second form of our engine hath as to several purposes its peculiar conveniences and advantages, yet some virtuosi may be furnished with the other already, and some may conceive it the more clearly of the two, or may judge it preferable for their particular designs; I shall here intimate, that for most of the experiments, if not all, that follow in this treatise, they may make use of, or at least make a shift with the first engine, with a few alterations; whereof the chief is to be this, that to the upper part of the great cylinder, on the side opposite to the iron-rack, there is to be fastened such a square board, and suitable iron-plate, as is used in the second engine, betwixt which board and plate is to be lodged such a pipe as was lately described,

described, between either a continuation of the outward branch of the stop-cock, or else firmly fastened to it by fodering or screwing; for by this means when the sucker is depressed, the air will through the cavity of this pipe, and the stop-cock whereto it is annexed, pass freely by virtue of its spring, out of the receiver into the exhausted cylinder; though this and the sucker that moves in it, being not kept, as in the second form of the engine, under water, the greater care will be needed to keep the air from insinuating itself between them. A good cement to fasten the receivers to the often mentioned plate of iron, is a thing of no small moment in making the following experiments, of which we employ differing compositions for differing purposes, some of which are not necessary to be mentioned in that part of this work that now comes forth; but that, which in almost all the following trials we chiefly make use of, is a well wrought mixture of yellow bees-wax and turpentine, which composition, as it serves better than most others to keep out the air, so it has the conveniency, which is no small one, of seldom needing to be heated, and seldomer to be much so; especially if we employ a little more turpentine in winter than in summer, in the former of which seasons, as much, or very near as much of that ingredient as of the wax does well, whereas in summer a mixture of three parts of wax to about two of turpentine is more proper.





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A CONTINUATION OF  
NEW EXPERIMENTS  
PHYSICO-MECHANICAL,

TOUCHING THE  
SPRING AND WEIGHT OF THE AIR, AND THEIR EFFECTS.

MY DEAR LORD,

SINCE I have already in proper places of the physico-mechanical experiments about the air, which I formerly presented your lordship, given you a sufficient account of several things touching the scope, occasion, &c. of my attempt; it will not be necessary to make a solemn preface to the ensuing experiments. And therefore presuming upon an acceptance which the favourable entertainment which your lordship, as well as the publick, was pleased to give my first trials of this kind, encourages me to expect I shall, without troubling you with any further preface, immediately fall upon a continuation; especially since your lordship will perhaps wonder that you have not received it much sooner, as indeed you should have done if I had been befriended with accommodations and leisure.

EXPERIMENT I.

*About the raising of mercury to a great height in an open tube, by the spring of a little included air.*

DIVERS ways have been proposed to shew both the pressure of the air, as the atmosphere is a heavy body, and that the air, especially when compressed by outward force, has a spring that enables it to sustain or resist a pressure equal to that of as much of the atmosphere as can come to bear against it; and also to shew that such air as we live in, and is not condensed by any human or adventitious force, has not only a  
resisting.

resisting spring, but an active spring (if I may so speak) in some measure, as when it distends a flaccid or breaks a full-blown bladder in our exhausted receiver.

BUT observing that there seems to want a visible experiment to convince those that are not so easily satisfied with reasons, though drawn by just consequence from physical or mechanical truths, or even from other experiments; taking notice, I say, hereof, I made the following experiments; not so much to prevent or remove a scruple no better grounded, as to have a new way of making an estimate by some known and determinate measure of the force of the bare spring of the air, both in its natural state (as it is said to be, when not compressed nor rarefied more than the free air we breathe) and according to its several degrees of expansion.

WE took then a phial with a neck not very large; and having filled about a fourth part of it with quick-silver, we so erected and fastened a long and slender pipe of glass, opened at both ends in the neck of the phial, with hard sealing-wax, that the lower end reached almost to the bottom of the quick-silver, and the upper more than a yard above the phial; then having blown in a little air, to try whether the instrument did not leak, (which it is very difficult to keep such instruments from doing) we conveyed it into a long and slender receiver fit for such an use; and having withdrawn the air as well as we could, we found, according to our expectation, that the spring of the air, included in the phial, impelled up the quick-silver into the erected pipe, to the height of 27 inches; and having suffered the external air to return into the receiver, the quick silver subsided in the tube, sometimes almost, and sometimes quite as low as the stagnant quick-silver in the phial.

See Plate  
III. Fig. 1

For the better illustration of this experiment, thus summarily related, but with the like success, as to the main, several times repeated, we will subjoin the following observations and notes:

I. THAT we tried this experiment several times, and the last time in the presence of the famous Savilian geometer, Dr. *Wallis*, who saw the quick-silver in the pipe impelled up to 27 inches, being one himself of the measurers. And though at other times we found it to be much about the same height with the last, yet once it seemed plainly to be a pretty deal higher; which yet we specified not, because a mischance took off the mark which we had made to measure the height by.

II. HAVING once, to try the stanchness of the phial, blown in so much air (without taking out any thing as we use to do in the like case) that the air in the cavity of the phial raised and kept the quick-silver three inches high in the pipe, when we went on with the rest of the experiment, according to the way above described, we found, by emptying the receiver of air, that we were able to raise the quick-silver in the cane 30 inches, or somewhat above that in the phial.

III. SOMETIMES it may happen that the mercury, when taken very soon out of the receiver, will not appear to have subsided to its first lowness, which perhaps it will not sink to in some while after; which is not to be wondered at, since in such a receiver, which contains but little air, the heat of the cement and the iron employed to melt it quite round the receiver, may impart a little warmth to the air in the phial, which will after return to its former temper. But this accident is neither constant nor necessary to the experiment.

IV. IT is very remarkable, that if the receiver be fitly stopped and slender enough, upon the turning of the stop-cock, to let out the air at the first exsuction, the mercury will be impelled up by the spring of the air in the phial, suddenly flying abroad or stretching itself, so that it will be raised several inches above the height it will rest at afterwards, and will make several vibrations up and down before it come to settle, just as  
the



the mercury does in the Torricellian experiment (the bare pressure of the little air doing here to the mercury what the weight of the atmosphere does there); and such motions of the mercury will be made four or five subsequent exsuctions, upon the withdrawing of the air in the receiver. But as these grow lesser and lesser as the spring of the included air grows fainter, so none of them is any thing near so considerable as the vibrations made upon the first suck.

V. AGREEABLE hereunto we observe that at the first exsuction, when the spring of the included air was yet strong, the mercury would be raised by our estimate above half, if not  $\frac{2}{3}$  of the whole height, whereto it will at length be brought (though that must be according to the bigness of the receiver, and other circumstances) and the subsequent exsuctions do still add less and less proportions of height to the mercurial cylinder, and that for two reasons; the one, because the more there is of mercury impelled into the tube, the greater weight of mercury presses upon the included air; and the other, because the air has so much the more room in the phial to expand itself, whereby its spring must be proportionably weakened.

LASTLY, when we made most of these trials, I had the curiosity to observe the height of the mercury in a good barometer, and thereby found that the air was then but light; its greatest height reaching but to 29 inches and  $\frac{1}{8}$ , and its height soon after the trial, whereof Dr. Wallis was a witness, amounting but to 29 inches.

To make an estimate of the quantity of air that had raised the quicksilver to 27 inches, we took the phial that was employed about this experiment, and having counterpoised it, whilst it was empty, we afterwards filled it with water, and found the liquor to weigh 5 ounces, 2 drachms, and about 20 grains; and then having poured out the water till it was sunk to a mark which we had made on the outside of the glass, to take notice how high the quicksilver reached that we poured in; and lastly, weighing the remaining water equal in bulk to the quicksilver, we found it to amount to 1 ounce, 2 drachms, 14 grains; so that the air that had raised up the mercury, possessed (before its expansion) in the phial the place but of  $\frac{1}{4}$  ounces, and a few odd grains, i. e. of about  $\frac{1}{8}$  of a pint of water. And as for the pipe also, employed about the same experiment, we found its cavity to have about  $\frac{1}{4}$  part of an inch in diameter.

It was one of the uses I hoped to make of this experiment, that by comparing the several degrees of expansion of air included in the phial with the respective and increasing heights of the mercury that was impelled up into the pipe, some estimate might be made of the force of the spring of the air weakened by several degrees of dilatation; but for want of conveniencies I forbore to venture upon such nice observations, especially because the pressure of the dilated air that remains in the receiver, and is external to the air included in the phial, must also be taken into consideration.

ANOTHER use of our experiment may be this; that it may supply us with a considerable argument against some learned men, who attribute the suspension of the quicksilver in the Torricellian experiment to a certain rarefied matter which some call a funiculus, and whereto others give other names; which rarefied substance they suppose to draw up and sustain the quicksilver, in compliance of nature's abhorrency of a vacuum. For in the experiment under consideration, the quicksilver, being not only sustained at the height of 27 inches in the tube, but elevated thither; if the cause of this be demanded, it will be answered, according to their hypothesis, that the air in the receiver, external to that of the phial, being, by reason of the sucking out of some of it by the pump, more rarefied than that in the phial, it draws up to it the quicksilver in the cane; and the more it is rarefied the higher it is enabled to draw it. But then I demand, whence it comes to pass, that though we can, by persevering to pump, more and more rarify the little remaining

maining air, or the æreal substance in the receiver, that in the phial not appearing to be also rarefied, yet the air in the receiver does not, by virtue of its superadded rarefaction, whereby it exceeds that of the air in the phial, pull up the quicksilver to a greater height in the tube than 27 inches; for, that this is not the greatest height to which mercury may be raised by this rarefied substance, our adversaries must not deny, who tell us, that in the Torricellian experiment it sustains a mercurial cylinder of 29 inches and  $\frac{1}{2}$ , and can raise a cylinder of 29 inches to 29 $\frac{1}{2}$ , or higher, in case that the cylinder be made to vibrate up and down in the tube.

See the  
latter part  
of the fol-  
lowing  
Experi-  
ment.

AND as for those that will in such cases, as our experiment suggests, have recourse only to that which they call the *fuga vacui*, they may please also to consider, that since the quicksilver remains the same, its ascension in the tube will not be available for what they think to be nature's purpose; for, whether it reach higher or lower in the tube, it will adequately fill no more space in one posture, or in one figure, than in another, in what part soever of the cavity of the receiver it be placed.

## EXPERIMENT II.

*Shewing that much included air raised mercury in an open tube no bigger than the weight of the atmosphere may in a baroscope.*

IN the former experiment, by reason of the smallness of the phial that was employed about it, there was so little air included, that the expansion of it, so far as was requisite to impel up the mercury in the pipe to the above-mentioned height of 27 inches, may be probably suspected to have very much weakened its spring, and therefore it may be thought that (especially considering the great force that several of our experiments manifest imprisoned air to have) if there were a greater quantity of air included in the vessel, so that the expansion, sufficient to raise the mercury to the former height, would not need to be considerable (because that the capacity of the tube being but the same, the whole included air will be so much the less expanded, by how much the more of it there is); it seemed probable that the spring of the air, being but a little weakened by so small a dilatation, would remain strong enough to raise a much taller cylinder of mercury in the tube, and perhaps make the liquor run over into the receiver.

BUT though this suggestion seem probable enough, yet when I considered that the weight of the atmosphere is able to sustain a cylinder of quicksilver but of 30 inches or thereabouts (in perpendicular height) and consequently that the pressure of such a mercurial cylinder is equivalent to that of an atmospherical cylinder of the same bore; it was not difficult to conclude, that since the air in a phial, before the mouth is closed, has a spring but equal in strength to the weight of the atmospherical pillar that leans upon it (for if the spring were too strong for the weight that leans on it, some of the air would get out of the phial) a greater phial, and consequently a greater quantity of included air would not be able by its spring to elevate and sustain a longer cylinder of mercury, than the weight of the atmosphere is able to do; nor indeed altogether so much, because of some little (though but little) diminution of the spring by some (though but a small) expansion that the included air suffers by succeeding in the place of mercury that is impelled up.

TO clear, therefore, this matter by an experiment, we took a strong glass-bottle, capable of holding about a quart of liquor, and having put into it a convenient quantity of quicksilver, we erected in it a very long and slender pipe of glass open at both the

ends,



ends, and reaching at the lower end beneath the surface of the stagnant mercury; and having fastened this pipe in the neck of the bottle, by choaking up that neck very accurately with good cement, that none of the included air might be able to get out, we conveyed the whole into a receiver, like that employed about the first experiment in shape, but much larger, that it might be able to contain so great a vessel; and then the engine being set at work, we quickly raised the quicksilver to a greater height than formerly; and when we saw it come to a stand, we did by the help of some marks, made before-hand on the pipe, and by the help of a very long and well divided ruler, measure, with as much care and accurateness as the figure of the vessels would allow us to do, the height of the mercurial cylinder, which we found to be 29 inches and about  $\frac{7}{8}$ , to which abating half an inch, which was raised before the pump was employed, by some air that had been blown into the bottle, to try whether it were flanch; deducting, I say, this half inch of quicksilver which remained in the tube after the external air was let in (as well as it had been there before the receiver was exhausted) out of the newly mentioned number there remained 29 inches, and near  $\frac{3}{8}$ , for the height of the mercury, raised by the spring of the air shut up in the bottle; and then consulting with the above-mentioned baroscope which stood in a window in another part of the house, I found that the weight of the atmosphere did bear a mercurial cylinder of about 29 inches and  $\frac{1}{2}$ , which was higher by  $\frac{1}{2}$  than that to which the spring had raised the quicksilver in the exhausted receiver; and the difference perhaps would have been greater, if the place where the experiment was made, had not by its warmth added some little matter to the spring of the air; and if also we could have kept the mercury so long elevated as to give it leave to discharge itself of those small bubbles, which it is almost impossible, in such experiments as this, to free quicksilver from, without some help from time.]

LASTLY, though we caused the pump to be plied, to try whether we could not, by the more diligent exsuction of the receiver, raise the quicksilver above the height of that which the atmosphere kept sustained in the baroscope; yet our labour gave us a confirmation that the spring of the air would not raise the mercury higher than did the weight of the atmosphere, which may not a little confirm the second observation.

N. B. THIS was not the only nor the first experiment we made of this kind; but this being carried on without mischances (with which divers others were attended) and made with much care, I thought fit to set down this instead of all, intimating generally about the rest, that they seemed to agree well for the main with that which is here recited. Only there is one thing relating to those other experiments that seems not altogether unworthy to be taken notice of; which is, that when our trials were made in vessels that contained a considerable quantity of air, though upon the exhaustion of the receiver the spring of the included air could not raise the quicksilver to the top of the pipe, yet sometimes by other effects it manifested itself to be very strong, as once or twice by the blowing out or breaking the cork or cement and other matter that was employed to stop the glass it was shut in; and once by an accident too memorable to be here passed over in silence.

I HAD one day invited Dr. Wallis to see such an experiment as I have been relating made with (not a phial, but) a bottle of green glass (such as we use now for wine) and four or five pounds of mercury. After this learned person and I had continued spectators as long as we thought fit, we withdrew into another room, where we had not sat long by the fire before we were surprized by a sudden noise, which the person that occasioned it presently came running in to give us an account of, by which it appeared that this ingenious young man (whom I often employ about pneumatical experiments, and whom I mentioned to your Lordship, because J. M. has the honour to be somewhat known

to you) being desirous in our absence to satisfy the curiosity he had to know whether the quicksilver could not be raised higher in the pipe than I had foretold, plyed the pump so obstinately, that at length the bottle being not, it seems, every where equally strong, the imprisoned air found it more difficult to make the quicksilver run over at the top of the pipe, than to break the bottle in the weakest place; and accordingly did not only throw off a piece of the bottle, but threw it with such violence against the large and strong receiver, as broke that also, and rendered it unserviceable for the future. But the doctor and I laying together the pipe, which happened to be broken into but few pieces, concluded by the place, to which we were told it reached when this accident happened, that it had not exceeded nor indeed fully equalled the height to which the weight of the atmosphere might have raised it.

### EXPERIMENT III.

*Shewing that the spring of the included air will raise mercury to almost equal heights in very unequal tubes.*

HAVING shewn in the two former experiments that the active strength of the air's spring is very considerable, I thought good also to examine whether or no to the other resemblances in operation between the weight of the free air and the pressure of the included air, this also may be added, that as the gravitation of the atmosphere is able (as we shall hereafter prove) to sustain the mercury at the same height in lesser and greater tubes sealed at the top; so the pressure of the included air may be able to sustain the mercury at the same height in slender and in larger tubes, though in the latter it must sustain a far greater weight of mercury than in the former; provided allowance be made for the weakening, which the spring of the included air must be subject to, by reason that, to succeed in the place of a large cylinder of mercury impelled up into the greater tube, it must expand itself more, and consequently have its spring more weakened than if the tube were slender.

To prosecute this experiment, I thought on a peculiar shape of vessels, which, if I had been where there is a glass-house, I would have caused to be blown for the more convenient trying of two pipes of different bores at the same time: but though I wanted this accommodation, I thought it might well enough shew what I intended by employing successively two tubes of very differing sizes, provided the vessel for the including of the air were the same.

WHEREFORE, taking the glass bottle made use of to try the former experiment, and erecting in it, after the manner above described, a cylindrical pipe of glass a good deal larger than the former (if not as large again) we prosecuted the experiment as we had made it, with the slender tube above-mentioned, and found that we were able, by the spring of the air in the bottle, to raise the quicksilver to a considerable height, which, measuring as well as the vessel would allow us, was, by the least estimate that was made of it (which was mine) 28 inches and  $\frac{1}{2}$ , by which it appeared to want somewhat above an inch of the height of the mercurial cylinder, which the weight of the atmosphere could have sustained, as appeared by the barometer, wherein the quicksilver at that time was about 29 inches and  $\frac{1}{2}$  high; which difference was no more than I expected, considering, that, whereas the weight of the atmosphere is still the same, when the mercury is at its full height (and that whether the pipe be great or small) in a sealed tube; the spring of our included air must needs be weakened the larger the tube is, and the higher



higher the liquid metal is impelled in it; so that it seemed a considerable phenomenon that the spring of so little air should be able to raise the mercury as high within an inch or thereabouts in a wider as in a slenderer tube, since the diameter of the cavity of the former being, by our estimate, double to that of the latter (into which the slender pipe could easily be put as into a case too big for it); the greater mercurial cylinder may be supposed to have weighed near four times as much as the lesser; I say, near, because there was an inch difference in their heights, but in case these had been equal, then the solidities of the cylinders would have been to one another as their bases; and since these, being circular, are in duplicate proportion to their diameters, that is, as the squares of their diameters; it is plain that if the diameters be as one to two, the squares of them must be as one to four; and these cylinders consisting of the same mercury, their weights will have the same proportions with their solidities, and consequently would be as one to four, making the abatement formerly intimated for the inch and a little more of mercury, by which the larger cylinder came short of the height of the former.

N. B. 1. This and the two former experiments tried by us with quicksilver, may be also tried with water; but besides that we could hardly procure tubes long enough for such trials, we were not very solicitous about it: for if we attentively enough consider what has been already delivered, and the proportion in specific gravity betwixt water and quicksilver (whereof the latter is near 14 times as heavy, bulk for bulk, as the former) it will not be difficult to foresee the event of such experiments, which he that has a mind to make should be furnished not only with long tubes, but with capacious vessels to shut up the air in; else the air will be so far expanded before the water has attained near the height to which the weight of the atmosphere may raise it, that the experiments will not seem to succeed near so well with water as ours did with quicksilver.

2. We thought it worth trying, whether, when the included air had raised the great cylinder of mercury to the utmost height it could elevate it to by the spring it then had, it would not be brought to raise the quicksilver yet higher, if, notwithstanding the expansion it had already, there were an agitation made by the heated corpuscles of the same air. And, in pursuance of this curiosity, having caused an hot iron and a shovel of kindled coals to be held near the opposite parts of the receiver, we perceived after a while that the mercury ascended  $\frac{1}{8}$  of an inch or better above the greatest height it had reached before: but conjecturing that it would have risen higher, were it not, that whilst the application of the hot bodies was making, some particles of air had unperceivably stolen into the receiver, I caused the pump to be plied again to withdraw the air I suspected to have got in, by which means the mercury was quickly raised  $\frac{2}{3}$  of an inch, or better, by virtue of this adventitious spring (if I may so call it) which the included air acquired by heat, and I made no doubt that it might have been raised much higher; but I was unwilling, by applying a less moderate heat, to hazard the breaking of my glasses, in the place I then was in, where such a mischance could scarce have been repaired.

## EXPERIMENT IV.

*About a new hydraulo-pneumatical fountain, made by the spring of uncompressed air.*

I SHALL NOW add such an application of the principle whereon the former experiment was grounded, as I should scarce think worth mentioning in this place, were it not, that besides that divers virtuosi seem not a little delighted with it, it may, for aught I know, prove to be of some philosophical use (to be pointed at hereafter).

See Plate IV. Fig. 2. This figure was designed only to make some representation of the difference, that would appear, if instead of making the fourth experiment with water, as of the pipe in the foregoing figure, the trial was made with quicksilver.

WE took a glass bottle with a convenient quantity of water in it, and fitted this bottle with a slender glass-pipe open at both ends, and about three foot long, which was so placed that the lower orifice was a good way beneath the surface of the water, and the pipe itself passed perpendicularly upwards through the neck of the bottle, which neck was, by the pipe and by good hard cement, employed to fill the space betwixt the pipe and the inside, so well and firmly closed that no water or air could get out of the bottle, nor no external air could get into it, but by passing through the pipe. This instrument was conveyed into a large receiver shaped like a pear, of which a good part of the blunt end, and a small part of the sharp end are cut off by sections parallel to the horizon, and consequently to one another. And because this receiver was not (nor ought to be) long enough to receive the whole pipe, there was cemented on to the upper part of it a smaller receiver of white glass, of such a length and bigness, that the upper end of the pipe might reach to the middle of its cavity, or thereabouts, and that the motions of the springing water might have a convenient scope, and so be the better taken notice of.

THIS double receiver being cemented on to the engine, a little of the air was by one suck of the pump drawn out from it, by which the pressure of the remaining air being weakened, it was necessary, that since the air included in the bottle had not its spring likewise weakened it should expand itself, and consequently impel up the water in the same bottle through the pipe, which it did so vigorously, as to make it strike briskly at first against that part of the top of the smaller receiver, which was just over the orifice of the pipe. But after it had a while made the water thus shoot up in a perpendicular line, as the spring of the air in the bottle grew by that air's dilatation to be weakened, the water would be impelled up less strongly and less directly, till the air in the bottle being as much expanded as that in the receiver, the ascent of the water would quite cease, unless by pumping a little more air out of the receiver we renewed it again.

ABOUT the making of this experiment these particulars may be noted.

I. It is convenient that the upper part of the pipe be made (as it easily may be at the flame of a lamp) very slender, that the water having but a very small orifice to issue out at, may be spent but slowly, and thereby make the experiment last so much the longer.

II. You may, if you please, instead of making the upper part of the pipe slender, as was just now directed, cement on to it a top either of glass or brass, consisting of three or more very slender pipes, with a pin-hole at the end of each, that one of these pointing directly upwards, and the others to the right hand and to the left, the water may spout out several ways at once, by which kind of branched pipes we have sometimes imitated the *Jets d'eau* (as the French call them) and artificial fountains of gardens and groves.



III. In regard that so short a cylinder of water, as exceeded not the length of our glass pipe, could not make any considerable resistance to the expansion of the included air, it was thought and found safe enough to employ, instead of a strong glass-bottle, a much larger phial, without being solicitous about its shape, or that it should be very strong, and by this means we could make this pleasant spectacle last a great while, especially if we also made use of the expedient to be mentioned in the following note.

4. If you find that the included air has by expanding itself too much weakened its spring, whilst there yet remains with it a good quantity of water in the bottle or phial, you may reinforce the pressure of the air by only turning the stop-cock, and letting in what air you think fit to the exhausted receiver: for upon the admission of this new air, the air in the receiver will press upon the water in the pipe, and having driven it into the bottle again, will follow it thither till the air in the bottle and that in the receiver have attained an equal spring, and then by pumping out a convenient quantity of the air contained in the latter, the air shut up in the former will be able to impel up the water as before, till the stagnant liquor be depressed to the lower orifice of the pipe, at which, when the air of the bottle can get out, the course of the water upwards must cease.

THE uses I made of this new hydraulo-pneumatical fountain (for in it I aim not only at a ludicrous experiment) were principally these.

THE first was to make it the more probable, that if we had had convenient vessels we might, by the pressure of the air included in the bottle, have raised water about fourteen times as high as we did quicksilver in the former experiment, since upon but a little weakening of the pressure of the air in the double receiver, the air in the bottle was able to impel the water forcibly enough, and for a pretty while, to the top of a pipe of about a yard long, and a good deal higher (but this is but a slight use).

THE next thing therefore we designed to shew by this experiment was, that in those hydraulo-pneumatical engines, where water is placed between two parcels of air, the water may be set a moving as well by the mere dilatation of one of the parcels of the air, as by giving a new force, by heat or compression, to the other; and whether this mechanical principle of motion may hereafter prove not altogether useless in engines, we refer to further consideration.

ANOTHER use we made of this experiment was to shew somewhat relating to the spring of the air, which may be worth considering, though we shall now but barely mention it. If then, when some of the air had been pumped out of the receiver, we removed that double vessel from the bottle, the external air would, by its weight, hastily depress the water in the pipe, till having driven it to the very bottom it got up in numerous bubbles through the water, and joined itself with the air incumbent on that liquor; but that which was here observable was, that all the external air that was able to get into the bottle did not do it suddenly, but after the first irruption we could perceive that from time to time there would new portions of air leisurely insinuate themselves through the pipe into the bottle, and emerge through the stagnant water in bubbles, that succeeded one another so slowly as to beget some wonder, as if the spring of the included air having been once put out of its wonted constitution by its late expansion, could not be reduced to it, but by degrees, by the weight of the atmosphere, which was still the same; or, rather, as if between the spring of the included and the pressure of the external air counterbalancing each other, there happened some such thing as is observed in an ordinary pair of scales, of which one is too much depressed, where the motion (which was swift enough at first) becomes so much slower, by how much the weights come nearer to the equilibrium which their equality disposes them to rest in.

BUT

BUT the chief use designed in this experiment was to observe, whether the lines made by the water in its effluxions, would be of the same figure, notwithstanding the rarefaction of the air in the upper part of the receiver, as if the air had not been at all rarefied; and for this purpose it is best to make one's observations towards the latter end of the experiment, because then the receiver being most exhausted, and consequently having the least of air left in it, the difference made by the change of the density of the medium, in which the beams of water (if I may so call them) move, is like (in case there be any) to be best discerned. And this convenience we had by our way of experimenting, that we could take notice of the lines described by the salient water, as the ejaculation of that liquor grew still fainter and fainter. But though I afterwards invited Dr. Wallis to favour me with his opinion about the curve lines of the salient water, yet for want of an upper receiver large enough, even he professed himself (as I had done) not satisfied about them. Only he sometimes (as I also did) observed the salient water to describe part of a line perfectly enough parabolical, with which sort of curves he has been particularly conversant.

THIS made me resolve, for further satisfaction, to attempt by another contrivance (of whose success, if I can procure the implements I need, your lordship may expect an account) what the figures will be not only of salient water, but mercury, and other liquors; and that when the receiver is much better exhausted, than it was necessary it should be in the foregoing experiment.

## EXPERIMENT V.

*About a way of speedily breaking flat glasses, by the weight of the atmosphere.*

For the more easy understanding of some of the subsequent trials, it will be requisite in this place to mention, among experiments about the spring of the air, the following phenomenon belonging to its weight.

THIS is one of those that is the most usually shewn to strangers, as a plain and easy proof, both that the weight of the incumbent air is considerable, and that the round figure of a receiver doth much more conduce to make an exhausted glass support that weight, than if the upper part of the receiver were flat.

To make this experiment we provided a hoop or ring of brass of a considerable thickness, whose height was  $2\frac{1}{4}$  or 3 inches, and the diameter of whose cavity as well at the upper as lower orifice (should have been just 3 inches, but through the error of the workman) was 3 inches and  $\frac{1}{16}$ . To this hoop we successively fastened with cement divers round pieces of glass, such as is used by glaziers (to whose shops we sent for it) to make panes for windows, and thereby made the brass-ring with its glass-cover a kind of receiver, whose open orifice we carefully cemented on to the engine; and then we found, as we had conjectured, that usually at the first exsuction (though sometimes not till the second) the glass-plate would be broken inwards with such violence as to be shattered into a great multitude of small fragments, and (which was remarkable) the irruption of the external air driving the glass inwards did constantly make a loud clap, almost like the report of a pistol: which phenomenon, whether it may help us to discover the cause of that great noise that is made upon the discharging of guns (for the recoil seems to depend upon the dilatation and impulse of the powder) I must not stay to consider.

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## EXPERIMENT VI.

*Shewing, that the breaking of glass-plates in the foregoing experiment, need not to be ascribed to the fuga vacui.*

THOUGH I long since informed you, that in the experiments I then presented your lordship, it was not my purpose to deliver my own opinion, whether there be a vacuum, or no; and though I do not in this tract intend to declare myself either way; yet, that I may on this occasion also shew, that the pressure of the air may suffice to account for divers phenomena, which, according to the vulgar philosophers, must be referred to nature's abhorrency of a vacuum, I will illustrate the foregoing experiment by another, the substance whereof is this.

THAT if, instead of the above-mentioned brass-hoop, both whose orifices are of equal breadth, you may employ a hollow (but taller) piece of brass, or (which is more easily made) of latten, shaped like a conus truncatus, or a sugar-loaf, whose upper part is taken off parallel to the bottom; and if you make the two orifices of a breadth sufficiently unequal, as if the larger being made as wide as that of our brass hoop, the straiter were less than an inch in diameter; you will find, that if this piece of metal be made use of, as the other was in the foregoing experiment, the flat glass cemented on to the orifice will be easily broken, as formerly when it is fastened to the wider orifice; but if the straiter orifice be turned upward, the glass that covers it, if it be of a due thickness (though no thicker than the former) will remain entire, notwithstanding the withdrawing of the air from beneath it: which seems sufficiently to argue, that it is not precisely nature's abhorrency of a vacuum that is the cause why glasses are usually broken in such experiments, since, whether the wider or the narrower orifice be uppermost, and covered (the metalline part of the vessel being the same, and only varying its posture) the capacity of the exhausted vessel will be equal; and therefore nature ought to break the glass as well in one case as the other, which yet the experiment shews she does not.

WHEREFORE this diversity seems much better explicable by saying, that when the wider orifice is uppermost, the glass that covers it must serve for the basis of a large atmospherical pillar, which by its great weight may easily force the resistance of the glass; whereas when the smaller orifice is uppermost, there leans upon its cover but so slender a pillar of the atmosphere, that the natural tenacity of mutual cohesion of parts in the glass is not to be surmounted by a weight that is no greater.

## EXPERIMENT VII.

*About a convenient way of breaking blown bladders by the spring of the air included in them.*

THE foregoing experiments having sufficiently manifested the strength of the air's spring upon fluid bodies, I next thought fit to try whether the force of a little included air would also upon consistent and even solid bodies emulate the operations of the weight of the atmosphere. In the prosecution of which enquiry we thought fit to make two sorts of trials: the one, where the air is included in the bodies on which its spring does work; and the other, where it is external to them. Of the first sort are this seventh

and the two following experiments; and of the second sort are some other trials, to be comprehended under the tenth experiment.

HAVING formerly mentioned to your lordship, that we were several times able (though sometimes not without much difficulty) to make a blown bladder break with the spring of its own air; I should not think it worth while to say any thing here about the same phenomenon, but that (besides that it seems odd enough, and is not unpleasant to many spectators) it may deserve not to be wholly neglected, because a good way to break bladders in the much exhausted receiver may sometimes prove an useful expedient, especially in such cases, where the experimenter (who sometimes either is not skilful enough, or well enough furnished with accommodations to regulate the ingress of the air) would very suddenly supply the receiver with fresh air, when it has been much emptied, without danger of letting in too much air from without. Not to mention, that the air included in the bladder to be broken may be so mingled with steams, or imbued with divers qualities, as to be much fitter than common air for some particular purposes.

WE shall then, for the affinity's sake between this trial and the former, subjoin now the way, by which we seldom failed of breaking bladders in our emptied receivers. For this purpose the blown bladder that was to be burst, having the neck very closely and strongly tied, was kept a pretty while in the receiver, whilst the air was pumping out, and then taken out again, that now the fibres were stretched and relaxed, the capacity being lessened by a new ligature that I ordered to be strongly made near the neck, the bladder might be lessened though the air were but the same, and the membrane being not so capable of yielding as before, upon the second exhaustion of the receiver the bladder in it would break far more easily than otherwise, and perhaps be oddly enough lacerated.

WE sometimes also varied this way of disposing bladders to be burst, by omitting the preparatory putting in of the bladder into the receiver, and only taking it in a little near the neck, that, the bladder having not been blown very full at first, the tension of the included air might be greater. But this last way is to be made use of, when the thing we desire is, that the bladder by breaking at a certain time may part with its air, and not when it is only to give an instance of the force of the spring of uncompress'd air against the sides of the vessel that contain it.

## EXPERIMENT VIII.

*About the lifting up a considerable weight by the bare spring of a little air included in a bladder.*

You will easily believe, that the force employed (in the following experiment) by the air, to break the well blown bladders it is included in, is considerable, if I here add, that a small quantity of air which will not fill  $\frac{1}{4}$  of a bladder, will not only serve to blow it quite up, but will manifestly swell it, though that effect be opposed not only by the resistance of the bladder itself, but by a considerable weight tied to the bottom of it, as in the following experiment.

WE took a middle sized bladder (of a hog or sheep) and having pressed out the air, till there remained but a fourth or fifth part (by guess) we caused the neck to be very strongly tied up again; also round about the opposite part of the bladder, within about an inch of the bottom, we so strongly tied another string, that it would not be made to  
slip



slip off by a not inconsiderable weight we hung at it; then fastening the neck of the bladder to the turning key, we conveyed the bladder and the weight hanging at it into a large receiver, in which when it began to be pretty well exhausted, the air within the bladder being freed from the wonted pressure of the air without it, did by its own spring manifestly swell, and thereby notably shorten the bladder that contained it, and by consequence visibly lifted up the weight (that resisted that change of figure) which exceeded fifteen pounds of sixteen ounces to the pound.

AFTER that we took a large bladder, and having let out so much air, that it was left lank enough, we fastened the two ends of it to the upper part of the receiver (for which else it would have been too long) and tied a weight (but not the same) so as that it hung down from the middle of the bladder; then exhausting the receiver as before, though the bladder, and this new weight which stretched it, reached so low as that for a while we could scarce see whether it hung in the air or no, yet at length we perceived the bladder to swell, and concluded that it had lifted up its clog about an inch; which was confirmed by the return we permitted of the air into the receiver, upon which the bladder became more wrinkled than before, and the weight descended, which being taken off, and weighed in a statera, amounted to about 28 pounds. We would have reiterated the experiment, but so heavy a weight having broken the bladder, we were discouraged from proceeding any further, especially in regard of the difficulty of bringing by this contrivance the strength of the air's spring to any exact computation; though it sufficiently shews what I designed it should, namely, that the spring of a little included air may be able even in so slight a contrivance to raise a great weight.

WHETHER this experiment may any way illustrate the motion of muscles made by inflation, contraction, &c. it belongs not to this place to consider.

## EXPERIMENT IX.

*About the breaking of hermetically sealed bubbles of glass by the bare spring of their own air.*

I SHALL premise to the following trials an experiment, wherein uncompressed air is made by its own bare spring to break the solid body itself it is shut up in. And this I the rather set down before the subsequent trials, because in our already published physico-mechanical experiments mention has been made of this trial, as of one that we could not then make to succeed; we have since, employing smaller receivers, made it often enough prosperously, somewhat to the wonder of eminent virtuosi, who confessed to me they had made frequent and divers attempts to perform the same thing, without ever succeeding in any of them.

BUT it will not be requisite to multiply relations about this particular, and therefore I shall set down but this one, which I meet with among my loose notes.

A large glass bubble hermetically sealed being put into the receiver, and the air drawn out as much as in usual operations, and somewhat more, though I told the company beforehand, that I had several times observed that such bubbles would not break immediately, but some while after the withdrawing the air from about them; yet this continued so long entire after we had left off pumping, that presuming it had been blown too strong, I began to despair of the experiments succeeding; when, whilst we were providing something else to put into the receiver, and, as I guessed, four minutes after the pump had been let alone, the bubble surprized us with its being broken with such

violence by the spring of the included air, that the fragments of it were dashed every way against the sides of the receiver, and broken so very small, that when we came to take it up, the powder was by the by-standers compared to the small sand wont to be employed to dry papers that have been newly writ upon with ink. The reason why the bubble broke so slowly I cannot now stay to propose, no more than to examine whether the difficulty of breaking vessels of glass, no thicker than these bubbles, proceed from some weakening of the spring of imprisoned air, by its stretching a little the including glass (for in another case we have observed this glass to be stretchable by the pressure of air) or from hence, that it was very hard, as I have elsewhere mentioned, to avoid rarefying the air a little, and consequently weakening its spring, by the heat that was necessary to be employed about the sealing up the bubble.

## EXPERIMENT X.

*Containing two or three trials of the force of the spring of our air uncompressed upon stable and even solid bodies (where to it is external).*

In prosecution of the inquiry proposed in the title, we made (among others) the following trials.

### THE FIRST TRIAL.

I. WE took the brass-hoop, mentioned in the fifth experiment (whose diameter is somewhat above three inches) and having caused a glazier to cut some plates of glass, such as are used for making the quarrels of windows, till he had brought them to a size and a roundness fit to serve for covers to that brass-hoop, we carefully fastened one of them with cement to the upper orifice of the hoop or ring, and then cementing the lower orifice to the engine, so that the vessel, composed of the metal and glass, served for a small receiver; we whelmed over it a large and strong receiver, which we also fastened on to the engine with cement after the usual manner. By which contrivance it was necessary, that when the pump was set on work, the included receiver (of brass and glass) should have its air withdrawn, and yet the air in the larger receiver should not be pumped out but by breaking through the glass, so that the internal air of the metalline receiver (as we may call it for distinction sake) being pumped out, the glass plate that made part of that receiver, must lie exposed to the pressure of the ambient air shut up in the other receiver without having the former assistance of the now withdrawn air to resist the pressure; wherefore, as we expected, at the first or second exsuction of the air, included in the small metalline receiver, the glass-plate was, by the pressure of the incumbent air contained in the great receiver, broken into an 100 pieces, which were beaten inwards into the cavity of the hoop.

### THE SECOND TRIAL.

THIS done, to shew that there needed not the spring of so great a quantity of included air to break such glasses, we took another roundish one, which, though wide enough at the orifice to cover the brass ring and the new glass-plate that we had cemented on it, was yet so low that we estimated it to hold but a sixth part of what the large receiver formerly employed is able to contain; and having whelmed this smaller vessel, which was shaped



shaped like those cups they call tumblers, over the metalline receiver, and well fastened it to the engine with cement, we found, that though this external receiver had a great part of its cavity filled by the included one, yet when this internal one was exhausted by an exsuction or two, the spring of the little air that remained was able to break the plate into a multitude of fragments.

### THE THIRD TRIAL.

III. BECAUSE the glass-plates hitherto mentioned seemed not so thick but that the pressure of the included air might be able to give considerable instances of its force; instead of the metalline receivers hitherto employed, we took a square bottle of glass, which we judged to be able to contain about a pint (or pound) of water, and which had been provided to keep subtle chymical liquors in, for which use we are not wont to chuse weak ones. This we inverted, and applied to the engine as a receiver, over which we whelmed the large receiver formerly mentioned; and having cemented it on, as in the foregoing experiments, we set the pump on work to empty the internal receiver (or square bottle) by which means the withdrawing of the air, and the figure of the vessel (which was inconvenient for resisting) suffered the pressure of the air included in the external receiver to crush the phial into a great number of pieces.

AND to vary this experiment, as we did that of breaking the metalline receivers, we took another glass of the shape and about the bigness of the former, and having applied it to the engine as before, and covered it with a receiver that was little higher than itself, we found that upon the exhaustion of the air the second square glass was likewise broken into many fragments, some of which were of so great a thickness as moved some wonder, that the bare pressure of the air was able to break such a vessel, though probably the cracks that reached to them were begun in much weaker parts of the glass.

N. B. 1. THE bottoms and the necks of both these square bottles were entire enough; by which it seemed probable that the vessels had been broken by the pressure of the air against the sides, which were not only thinner than the parts above named, but exposed a larger superficies to the lateral pressure of the air, than to the perpendicular.

2. WE observed in one of the two last experiments, that the vessel did not break presently upon the last exsuction that was made of the included air, but a considerable time after, which it seems was requisite to allow the compressed parts of the glass time to change their places; and this phenomenon I therefore mention, because the same thing that here happened in the breaking a glass inwards by the spring of the air, I elsewhere observed to have happened in breaking a glass outwards by the same spring.

3. To confirm that it is the spring of the external receiver's air, that is the agent in those fractures of glasses, and to prevent or remove some scruples, we thought fit to make this variation in the experiment. We applied a plate of glass, just like those formerly mentioned, to the brass-hoop; but in the cementing of it on, we placed in the thickness of the cement a small pipe of glass about an inch long, whose cavity was not so big as that of a straw, and which being left open at both the ends, might serve for a little channel, through which the air might pass from the external receiver to the internal; over this we whelmed one of the small receivers above mentioned, and then, though we set the pump on work much longer than would have needed, if this little pipe had not been made use of, we found, as we expected, that the internal receiver continued entire, because the air, whose spring should have broken it, having liberty to pass through the pipe, and consequently to expand itself into the place deferred by the

air

air pumped out, did by that expansion weaken its spring too much to retain strength enough to break the metalline (or internal) receiver

BUT here it is to be noted, that either the pipe must be made bigger than that lately mentioned, or the exsuction of the air must not be made by the pump as nimbly as we can, or otherwise the plate of glass may be broken, notwithstanding the pipe; because the air contained in the external receiver having a force much greater than is necessary to break such a plate, it may well happen (as I have sometimes found it do) that if the air be hastily drawn out of the internal receiver, that air, which should succeed in its room, cannot get fast enough out of that external receiver through so small a pipe; and the air remaining in that external receiver will yet retain a spring strong enough to break the glass. To illustrate which I shall propose this experiment; that sometimes, when I have at the flame of a lamp caused glass bubbles to be blown with exceeding slender stems, if they were nimbly removed out of the flame whilst they were ignited, they would, according to my conjecture, be either broken, if they cooled too fast, or compressed inward, if they long enough retained the softness they had given them by fusion. For the air in the bubble being exceeding rarefied and expanded, whilst the glass is kept in the flame, and coming to cool hastily when removed from thence, loses upon refrigeration the spring the heat had given it; and so if the external air cannot press in fast enough through the too slender pipe, there will not get in air enough to resist the pressure of the atmosphere; and therefore if this pressure find the bubble yet soft, it will press it a little inwards, and either flatten it, or make a dimple in it, though the orifice of the pipe be left open.

## EXPERIMENT XI.

*Shewing that mercury will in tubes be raised by suction no higher than the weight of the atmosphere is able to impel it up.*

It is sufficiently known that the common opinion of philosophers, and especially of those which follow *Aristotle*, has long been, and still is, that the cause of the ascension of water upon suction, and particularly in those pumps where the water seems of its own accord to follow the rising sucker, is nature's abhorrency of a vacuum. Against this received opinion divers of the modern philosophers have opposed themselves. But as some of them were vacuists, and others plenists, they have explicated the ascension of water in sucking-pumps upon very different grounds; so that many ingenious men continue yet irresolved in this noble controversy. Wherefore though I have formerly made, and now renew a solemn profession, that I do not in this treatise intend to declare either for or against the being of a vacuum; and though I have \* elsewhere occasionally acknowledged myself not to acquiesce fully in what either the ancient or the modern philosophers have taught about the adequate cause of suction (in the assigning of which, I think, I have shewn them to have been somewhat deficient) yet, since I think some experiments, of importance to this controversy, may be better made by the help of our engine, than they have been by any instrument I have yet heard of, I shall now add the trials I made to shew both that whether there be, or may be a vacuum or not, there is no need to have recourse to a *fuga vacui* to explicate suction; and also that whatever other causes have by *Gassendus* and *Cartesius* been ingeniously proposed to explicate

\* The place here meant is a passage in the Author's *Examen* of Mr. *Hobbes's Dialogue* about the air.



suction, it seems to depend clearly upon the weight of the atmosphere, or in some cases upon the spring of the air; though I deny not that other causes may contribute to that pressure of the air, which I take to be the grand and immediate agent in these phenomena.

We took a brass pipe bended like a siphon, and fitted at the bigger end with a stop-cock, &c. as is delineated in the figure (which instrument for brevity sake, I often call an exhausting or sucking siphon) and to the slender end of this we fastned with good cement the upper end of a cylindrical pipe of glass, of about fifty inches long, and open at both ends, and having the lower end open into a glass of stagnant quicksilver, whose upper superficies reached a pretty deal higher than the immersed orifice of the glass cane. These things being thus prepared, we caused the pump to be set on work, whereby the air being by degrees drawn out of the exhausting siphon, and consequently of the glass cane that opened into it; the stagnant mercury was proportionably impelled up into the glass-pipe, until it had attained to its due height, which exceeded not 30 inches; and then, though there remained in the upper part of the pipe above 20 inches unfilled with quicksilver, yet we could not by farther pumping raise that fluid metal any higher.

See Plate III. Fig. 2. and the annotations at the close of this experiment.

By which it seems manifest enough, that whatever many learned men have taught, or others do yet believe, about the unlimited power that nature would exercise to prevent what they call a vacuum; yet this power has its bounds, and those depend not so much upon the exigency of that principle, which the schoolmen call a *fuga vacui*, as upon the specific gravity of the liquor to be raised by suction. For confirmation of which, we substituted instead of the stagnant mercury, a basin of water; and though instead of the many sucks we had fruitlessly employed to raise the quicksilver above the lately mentioned height, we now employed but one exsuction (or less than a full one) which did but in part empty the exhausting siphon: yet the water upon the opening of the stop-cock was not only impelled to the very top of the glass-cane, but likewise continued running for a good while through the exhausting siphon, and thence fell upon the plate of the engine; so that it seemed an odd spectacle to those that knew not the reason of it, to see the water running very briskly of its own accord, as they imagined, out of the shorter leg of a siphon; especially that leg being perhaps not above a quarter so long as the other. And here I must not omit this considerable circumstance, that though sometimes in the Torricellian experiment I have observed the mercury to stand at thirty inches, and now and then above it, yet the height of the mercury elevated in our glass-cane appeared not, when measured, to reach fully 29 inches and a quarter; which I thought it was not difficult to render a reason of, from the varying weight of the atmosphere; and accordingly consulting the baroscope (that stood in another room) I found the atmosphere to be at that time somewhat light, the quicksilver in it being in height but 29 inches and an eighth, which probably would have been the very height of the quicksilver raised by the engine, if it had had time by standing to free itself from bubbles.

From whence we may conclude, that suction will elevate liquors in pumps no higher than the weight of the atmosphere is able to raise them, since the closeness requisite in the pump of our engine to be staunch makes it very unlikely, that by any ordinary pump a more accurate suction can be effected.

I HAVE nothing to add about the related experiment but this one; that it may afford us a noble confirmation of the argument we formerly proposed against them, that ascribed the elevation and sustentation of the quicksilver in the Torricellian experiment to a certain rarefied air, which the more highly it is rarefied, the greater power

it acquires to attract quicksilver, and other contiguous bodies; for in our experiment, though by continuing to pump we can rarefy or distend more and more the air in the exhausting siphon, yet we were not able to raise the mercury above thirty inches (which exceeds not the height to which the atmosphere is able to elevate it) and this, though the stagnant mercury being exposed to the free air, it cannot be pretended (as in some other cases it may, though not satisfactorily, be done) that the mercury cannot be raised higher, without offering violence to the body incumbent on the stagnant mercury: for, in the experiment, we are considering if nature should raise the quicksilver higher and higher in the pipe, to succeed in the room of the air that is withdrawn, the formerly stagnant mercury, that would on this occasion be raised, might be immediately succeeded by the free and undilated air; so that nature would be put to offer violence to the quicksilver only, which if she were scrupulous to do, what ailed her to raise it (as she did in our trial) against the inclinations of so ponderous a body, to above 29 inches high?

#### ANNO TATION.

THOUGH the exhausting siphon, mentioned at the beginning of this experiment, may be easily enough conceived by an attentive inspection of the figure; yet, because I frequently made use of it in pneumatical experiments, it will not be amiss to intimate here once for all these three particulars about it. 1. That though the bending pipe itself may be for some uses more conveniently made of glass than of metal, because the transparency of the former may enable us to discover what passes in it, yet for the most part we chuse to employ pipes of the latter sort, because the others are so very subject to break. 2. That it is convenient to make the longer leg of the siphon a little larger at the bottom than the rest of the pipe usually needs to be, that it may the more commodiously admit the shank of a stop-cock, which is to be very carefully inserted with cement; by seasonably turning and returning of which stop-cock, the passage (for the air) between the engine and the vessel to be exhausted is to be opened and shut. 3. That though we sometimes content ourselves to apply immediately the brass siphon itself to the engine, by fastening with cement the external shank of the stop-cock to the orifice of the little pipe through which the exclusion of the air is made, yet the bended pipe alone, if it be not almost constantly held, is so apt to be loosened by the motion of the engine, and the turning of the stop cock (which frequently occasions leaks, and disturbs the operation) that for the most part we make use of a siphon, consisting of a brass-pipe, and stop-cock, and a glass of 6, 8, or 10 inches in height, and of some such shape (for it need not be the very same) as that represented in the figure: for by this means, though the exhaustion is because of this additional glass somewhat longer in making, yet it is more securely and uninterruptedly carried on by reason of the stability which the breadth of the lower orifice of the glass gives to the whole instrument. Besides which, we have these other conveniencies, that not only the siphon is hereby much lengthened, which in divers trials is very fit; but also that we may commodiously place in the glassy part of this compounded siphon a gage whereby to discern from time to time how much the air is drawn out of the vessel to be exhausted.

See Plate  
111. Fig. 2



EXPERIMENT XII.

*About the differing heights whereto liquors will be elevated by suction, according to their several specifick gravities.*

If when I was making the foregoing experiment I had been able to procure a pipe long enough, I had tried to what height I could raise water by suction, though I would have done it rather to satisfy others than myself, who scarce doubted but that as water is (bulk for bulk) about 14 times lighter than quicksilver, so it would have been raised by suction to about four or five-and-thirty feet (which is 14 times as high as we were able to elevate the quicksilver) and no higher. But being not furnished for the trial I would have made, I thought fit to substitute another, which would carry the former experiment somewhat further. For whereas in that we shewed how high the atmosphere was able by its whole gravitation to raise quicksilver; and whereas likewise that which appears in Monsieur *Paschal's* experiment is, at what height the whole weight of the atmosphere can sustain a cylinder of water: by the way that I thought on, it would appear (which hath not yet that I know of been shewn) how a part of the pressure of the air would in perpendicular pipes raise not only the two mentioned liquors, but others also to heights answerable to the degree of pressure, and proportionable to the specifick gravities of the respective liquors.

To make this trial the more clear and free from exceptions, I caused to be made and inserted to the shorter leg of the above-mentioned exhausting siphon a short pipe, which branched itself equally to the right hand and the left, as the adjoining figure de-  
Plate III.  
Fig. 3.  
 clares. In which contrivance I aimed at these two conveniences; one that I might exhaust two glass-canes at the same time; and the other, to prevent its being furnished that the engine was not equally applied to both the glasses to be exhausted. This additional brass-pipe, being carefully cemented into the sucking siphon, we did to each of its two branches take care to have well fastened with the same cement a cylindrical glass of about 42 inches in length (that being somewhat near the height of our exhausting siphon above the floor) the lower orifice of one of these two glasses being immersed in a vessel of stagnant mercury, and that of the other in a vessel of water, where care was taken by those I employed, that as the tubes were chosen near of a bigness (which yet was not necessary) so the surfaces of the two different liquors should be near of a height. This being done, we began to pump warily and slowly, till the water in one of the pipes was elevated about 42 inches; and then measuring the height of the quicksilver in the other pipe above the surface of the stagnant quicksilver, we found it to be almost three inches; so that the water was about 14 times as high as the quicksilver. And to prosecute the experiment a little further, we very warily let in a little air to the exhausting siphon, and had the pleasure to see the two liquors proportionably descend, till turning the stop-cock when the water was about 14 inches high, we thereby kept them from sinking any lower, till we had measured the height of the quicksilver, which we found to be about one inch.

We tried also the proportion of these two liquors at other heights, but could not easily measure them so well as we did at those newly mentioned; and therefore though there seemed to be some slight variation, yet we looked upon it but as what might be well imputed to the difficulty of making such experiments exactly; and this displeased me not in these trials, that whereas it was observed, and somewhat wondered at, that

the quicksilver for the most part seemed to be somewhat (though but a very little) higher than the proportion of 1 to 14 required, I had long before by particular trials found that though 14 and 1 be the nearest of small integer numbers that express the proportion between the specific gravities of quicksilver and water, yet the former of those fluids (or at least that which I made my trials with) is not quite so heavy as this proportion supposes, though I shall not here stay to determine precisely the difference, having done it in another tract, where the method I employed in the investigation of it is also set down.

THE above-mentioned experiment, made by the help of our engine, as to quicksilver and water being confirmable by trials (to be by and by mentioned) made in other liquors, affords our hypothesis too considerable advantages above the vulgar doctrine of the schools (for I do not apply what follows to all the plenists) who ascribe the ascension of liquors by suction to attraction made *ob fugam vacui*, as they are wont to speak.

FOR first it is manifestly agreeable to our doctrine, that since the air, according to it, is a fluid that is not void of weight, it should raise those liquors that are lighter, as water, higher than those that are ponderous, as quicksilver; and that answerably to the disparity of their weights. And secondly, there is no reason why, if the air be withdrawn by suction from quicksilver and water, there should be less left a vacuum above the one than above the other, in case either of them succeed not in the place deserted by the air; and consequently when the air is withdrawn out of both the forementioned glass-pipes, if there would be no vacuum in case no liquor should succeed it, why does nature needlessly to prevent a vacuum make the water, that is an heavy body, ascend contrary to its own nature, according to which it tends towards the center of the earth. And if the succeeding of a liquor be necessary to prevent a vacuum, how chance that nature does not elevate the quicksilver as well as the water; especially since it is manifest by the foregoing experiment, that she is able to raise that ponderous liquor above 26 inches higher than she did in the experiment we are now discoursing of.

PERHAPS it would not be amiss to take notice on this occasion, that among other applications of this experiment it may be made somewhat useful to estimate the differing gravities of liquors; to which purpose I caused to be put under the bottom of the forementioned glass-pipes two vessels, the one with fresh water, and the other with the like water impregnated with a good proportion of sea-salt that I had caused to be dissolved in it, for want of sea-water, which I would rather have employed. And I found that when the fresh water was raised to about 42 inches, the saline solution had not fully reached to 40.

BUT though this difference were double to that which the proportion and gravity betwixt our sea-water and fresh water would have required; yet to make the disparity more evident, and also because I would be able the better to guess at the proportion of the dissolved salt, by making it as great as I could, I caused an unusual brine to be made, by suffering sea-salt to deliquesce in the moist air; and having applied this liquor and fresh water to the two already mentioned pipes, and proceeded after the former manner, we found that when the pure water was elevated to near 42 inches, the liquor of sea-salt wanted about 7 inches and a quarter of that height; and when the water was made to subside to the middle of its pipe, or thereabouts, the saline liquor in the other pipe was between three and four inches lower than it.

I WOULD have tried the difference between these liquors and oil, but the coldness of the weather was unfavourable to such a trial: but to shew a far greater disparity than that would have done betwixt the height of liquors of unequal gravities, I took fair water, and a liquor made of the salt of pot-ashes suffered to run in a cellar *per deliquum* (this



(this being one of the ponderouſeſt liquors I ever prepared) and having proceeded as in the former trials, I found that when the common water was about 42 inches high, the newly mentioned ſolution wanted ſomewhat of 30 inches; and when the water was made to ſubſide to the middle of its pipe, or thereabouts, the deliquated liquor was between 6 and 7 inches lower than it.

I HAD ſome thoughts, when I applied myſelf to make theſe trials, to examine how well we could by this new way compare the ſaltneſs of the waters of ſeveral ſeas, and thoſe alſo of ſalt-ſprings; and likewiſe whether and (if any thing near) how far we might by this method determine the proportion of the more ſimple liquors that may be mingled in compounded ones, as in the mixture of water and wine, vinegar and water, &c. but being not provided with inſtruments fit for ſuch nice trials, and a miſchance having impaired the glaſſes lately mentioned before the laſt trials were quite ended, and having ſoon after broken one of them, I laid aſide thoſe thoughts.

### EXPERIMENT XIII.

*About the heights to which water and mercury may be raiſed, proportionably to their ſpecifick gravities, by the ſpring of the air.*

IN proſecution of the parallel formerly begun betwixt the effects of the weight of the atmosphere, and the ſpring of included air, we thought fit, after the foregoing, to make the following experiment.

WE took a ſtrong glaſs-bottle, capable to hold above a pint of water, and having in the bottom of it lodged a convenient quantity of mercury, we poured on it a greater quantity of water (becauſe this liquor was to be impelled up many times higher than the other) and having provided two ſlender glaſs-pipes, each open at both ends, we ſo placed and faſtened them by means of the cement, wherewith we choaked the upper part of the neck of the bottle, that the ſhorter of the pipes had its lower orifice immerſed beneath the ſurface of the quickſilver, and the longer pipe reached not quite ſo low as that ſurface, and ſo was immerſed but in the water, by which contrivance we avoided the neceſſity of having two diſtinct veſſels for our two ſtagnant liquors, which would have been inconvenient in regard of the ſlenderneſs of the upper part of our receiver. This done, we conveyed the bottle into a ſitly ſhaped receiver (formerly deſcribed at the firſt experiment, and having begun to pump out the air, we took notice to what heights the quickſilver and water were impelled up in their reſpective tubes, on which we had before made marks from inch to inch with hard wax (that they might not be removed by wet or rubbing) and we obſerved that when the quickſilver was impelled up to two inches, the water was raiſed to about eight-and-twenty; and when the quickſilver was about one inch high, the water was about fourteen. I ſay, about, partly becauſe ſome allowances muſt be made for the ſinking of the ſuperficies of the ſtagnant quickſilver, and the greater ſubſidence of that of the ſtagnant water, by reaſon of the liquors impelled into the two pipes; partly becauſe that the breadth of the mark of wax was conſiderable, when the quickſilver was but about an inch high, and ſo made it difficult to diſcern the exact height of the metal, when the water was fallen down to fourteen inches; eſpecially in regard that the quickſilver never aſcending ſo high as the neck of the bottle (which the water left far beneath it) the thickneſs of the receiver, and that of ſo ſtrong a bottle, made it difficult to diſcern ſo clearly the ſtation of the quickſilver as I could have wiſhed.

## EXPERIMENT XIV.

*About the heights answerable to their respective gravities, to which mercury and water will subside, upon the withdrawing of the spring of the air.*

FOR the further illustration of the doctrine proposed in the last, and some of the foregoing experiments, about the raising and sustentation of liquors in pipes by the pressure of the air, I thought it not unfit to make the following trial, though it were easy to foresee in this peculiar experiment a peculiar difficulty.

WE caused then to be conveyed into a fitly shaped receiver two pipes of glass very uneven in length, but each of them sealed at one end: the shorter tube was filled with mercury, and inverted into a small glass jar, wherein a sufficient quantity of that liquor had been before lodged; the longer pipe was filled with common water, and inverted into a larger glass, wherein likewise a fit proportion of the same liquor had been put.

THEN the receiver being closely cemented on to the engine, the air was pumped out for a pretty while before the mercury began to subside; but when it was so far withdrawn, that its pressure was no longer able to keep up a mercurial cylinder of that height, that liquid metal began to sink; the water in the other tube, though this were three times as long, still retaining its full height. But when the quicksilver was fallen so low, as to be but between three and four inches above the surface of the stagnant quicksilver, the water also began to subside, but sooner than according to the laws of mere statics it ought to have done, because many aerial particles emerging from the body of the water to the upper part of the glass, did by their spring concur with the gravity of the water to depress this liquor. And so when the quicksilver was three inches above the stagnant mercury, the water in the other pipe was fallen divers inches beneath 42, and several inches beneath 28, when the mercury had subsided an inch lower. But this being no more than was to be expected, after we had caused the pumping to be a while continued, to free the water the better from the latent air, we let in the external air; and having thereby impelled up again both the liquors into their pipes, and removed the receiver, we took out those pipes, and inverting each of them again to let out the air (for even that which held the quicksilver had got a small bubble, though inconsiderable in comparison of the air that had got up out of the water) we filled each of them with a little of the stagnant liquor belonging to it; and inverting each tube once more into its proper liquor, we repeated the experiment, and found it, as it seemed, to require more pumping than before to make the liquors begin to subside; so that when the mercury was fallen to three inches, or two or one, the water subsided so near to the heights of 42, 28, or 14 inches, that we saw no sufficient cause to hinder us from supposing that the little differences that appeared between the several heights of the quicksilver, and fourteen times as great heights of the water (which fell somewhat lower than its proportion in gravity required) proceeded from some aerial corpuscles yet remaining, in spite of all we had done, in the water, and by their spring, though but faint, when once they had emerged to the upper part of the glass, furthering a little the depression of it: not now to mention lesser circumstances, particularly the surface of the stagnant water did not inconsiderably rise by the accession of the water lately in the pipe; whereby the cylinder of water, raised above that surface, became by so much the shorter. However your lordship may, if you think fit, cause the experiment to be reiterated, which I could not well do, by reason of a mischance that befel the receiver.

EXPE-



# EXPERIMENT XV.

*About the greatest height to which water can be raised by attraction or sucking pumps.*

SINCE the making and the writing of the foregoing experiments, having met with an opportunity to borrow a place somewhat convenient to make a trial to what height water may be raised by pumping, I thought not fit to neglect it. For though both by the consideration of our hypothesis, to whose truth so many phenomena bear witness; and though particularly by the consequences deducible from the three last recited experiments I were kept from doubting what the event would be, yet I thought it worth while to make the trial.

I KNOW what is said to have been the complaint of some pump-makers: but I confess the phenomenon it was grounded on, seemed not to me to be certainly enough delivered by a writer or two, that mention what they complained of; and their observation seems not to have been made determinately or carefully enough for a matter of this moment; since that which they complain of seems to have been in general, that they could not by pumping raise water to what height they please, as the common opinion of philosophers about nature's *fuga vacui* made them expect they might. And it may well have happened, that as they endeavoured only to raise it to the height their occasions required, so all that their disappointment manifested was, that they could not raise it to that particular height; which did not determine, whether, if the pump had been a foot or a yard shorter, the water would then have been elevated to the upper part of it or no: but that which I chiefly consider is, that these being but tradesmen that did not work according to the dictates of, or with design to satisfy a philosophical curiosity, we may justly suspect that their pumps were not sufficiently stanch, nor the operation critically enough performed and taken notice of.

WHEREFORE, partly because a trial of such moment seemed not to have yet been duly made by any, and partly because the varying weight of the atmosphere was not (that appears) known, nor (consequently) taken into consideration by the ingenious Monsieur Pascal in his famous experiment, which yet is but analogous to this; and partly because some very late, as well as learned writers, have not acquiesced in his experiment, but do adhere to the old doctrine of the schools, which would have water raiseable in pumps to any height, *ob fugam vacui* (as they speak) I thought to make the best shift I could to make the trial, of which I now proceed to give your lordship an account.

THE place I borrowed for this purpose was a flat roof about 30 feet high from the ground, and with rails along the edges of it. The tube we made use of should have been of glass, if we could have procured one long and strong enough; but that being exceeding difficult, especially for me who was not near a glass-house, we were fain to cause a tin-man to make several pipes of above an inch bore (for of a great length it was alleged they could not be made slenderer) and as long as he could, of tin or latten, as they call thin plates of iron tinned over; and these being very carefully soldered together made up one pipe of about one or two-and-thirty feet long, which being tied to a pole, we tried with water whether it was stanch, and by the effluxions of that liquor finding where the leaks were, we caused them to be stopped with solder; and then for greater security, the whole pipe, especially at the commissures, was diligently cased over with our close black cement, upon which plaster of Paris was strewed

to keep it from sticking to their hands or cloaths that should manage the pipe; at the upper part of which was very carefully fastened with the like cement a strong pipe of glass, of between 2 and 3 feet in length, that we might see what should happen at the top of the water; and to the upper part of this pipe was (with cement, and by the means of a short elbow of tin) very closely fastened another pipe of the same metal, consisting of two pieces, making a right angle with one another, whereof the upper part was parallel to the horizon, and the other, which was parallel to the glass-pipe, reached down to the engine, which was placed on the flat roof, and was to be with good cement solicitously fastened to the lower end of this descending part of the pipe, whose horizontal leg was supported by a piece of wood nailed to the above mentioned rails; as the tube also was kept from overmuch shaking by a board fastened to the same rails, and having a deep notch cut in it, for the tube to be inserted into.

THIS apparatus being made, and the whole tube with its pole erected along the wall, and fastened with strings and other helps, and the descending pipe being carefully cemented on to the engine, there was placed under the bottom of the long tube a convenient vessel, wherinto so much water was poured, as reached a great way above the orifice of the pipe, and one was appointed to stand by to pour in more as need should require, that the vessel might be still kept competently full.

See Plate  
V. Fig. 1.

AFTER all this, the pump was set on work; but when the water had been raised to great height, and consequently had a great pressure against the sides of the tube, a small leak or two was either discovered or made, which, without moving the tube, we caused to be well stopped by one that was sent up a ladder to apply store of cement where it was requisite.

WHEREFORE, at length we were able, after a pretty number of exsuctions, to raise the water to the middle of the glass-pipe abovementioned, but not without great store of bubbles, made by the air formerly concealed in the pores of the water, and now emerging, which for a pretty while kept a kind of foam upon the surface of it (fresh ones continually succeeding those that broke). And finding the engine and tube as staunch as could be well expected, I thought it a fit season to try what was the utmost height to which water could by suction be elevated; and therefore, though the pump seemed to have been plied enough already, yet for further satisfaction, when the water was within few inches of the top of the glass, I caused twenty exsuctions more to be nimbly made, to be sure that the water should be raised as high as by our pump it could be possibly. And having taken notice where the surface rested, and caused a piece of cement to be stuck near it (for we could not then come to reach it exactly) and descending to the ground where the stagnant water stood, we caused a string to be let down, with a weight hanging at the end of it, which we applied to a mark that had been purposely made at that part of the metalline tube, which the superficies of the stagnant water had rested at, when the water was elevated to its full height: and the other end of the string being by him that let it down applied to that part of the glass, as near as he could guess, where the upper part of the water reached, the weight was pulled up; and the length of the string, and consequently the height of the cylinder of water was measured, which amounted to 33 feet, and about 6 inches. Which done, I returned to my lodging, which was not far off, to look upon the baroscope, to be informed of the present weight of the atmosphere, which I found to be but moderate, the quicksilver standing at 29 inches; and between 2 and 3 eighths of an inch. This being taken notice of, it was not difficult to compare the success of the experiment with our hypothesis. For if we suppose the most received proportion in bulk between cylinders of quicksilver and of water of the same weight, namely that of 1 to 14, the height



height of the water ought to have been 34 feet and about 2 inches, which is about 8 inches greater than we found it. But then your lordship may be pleased to remember that I formerly noted, before ever I made this experiment, that I did not allow the proportion betwixt mercury and water (at least such water as I made my trials with) to be altogether so great; and though in ordinary experiments we may with very little inconvenience make use of that proportion to avoid fractions, yet in so tall a cylinder of water as ours was, the difference is too considerable to be neglected. If therefore, instead of making an inch of quicksilver equivalent to 14 inches of water, we abate but a quarter of an inch, which is but a 56th part of the height of the water, this abatement being repeated 29 times and a quarter, will amount to 7 inches and above a quarter; which added to the former height of the water, namely 33 feet and 6 inches, will make up 34 feet and above an inch; so that the difference between the height of the mercury sustained by the weight of the atmosphere in the baroscope, and that of the water raised and sustained by the pressure of the same atmosphere in the long tube, did not appear to differ more than an inch or two from the proportion they ought to have had, according to the difference of their specific gravities. And though in our experiment the difference had been greater, provided it exceeded not 8 or 10 inches, it would not have been strange; partly, because of the difficulty of measuring all things so exactly in such an experiment; partly, because as waters are not all of the same weight, so a little disparity of it in so long a cylinder may be considerable; and partly, and perhaps chiefly, because the air flying out of the bubbles that rose out of so great a quantity of water, and breaking at the top of it, and so near that of the tube, might, by its spring, though but very weak, assisting the weight of so much water, somewhat (though not much) hinder the utmost elevation of that liquor. But our experiment did not make it needful for me to insist on these considerations; and the inconsiderable difference that was betwixt the height of the water we found, and that which might have been wished, did rather countenance than at all disavow the thing to be made out by our experiment, since by no pumping we could raise the water quite so high (though I confess it wanted but very little) as the weight of the atmosphere was able to keep up a cylinder of mercury proportionable to it in height, and equivalent in weight: and yet I presume your lordship will easily grant that there was at least as much care used in this experiment to keep the things about it tight, as has been wont to be used by tradesmen in their pumps, where it is not so easy either to prevent a little insinuation of the air, or to discern it.

It is not that I am sure, that even all our care would have kept the water for any long time at its full height; but that the air was sufficiently exhausted for our purpose, when we determined the height of the water, I was induced to conclude by these circumstances.

I. As well the construction of the engine, as the many formerly related experiments that have been successfully tried with it, shew, that it is not like it should be inferior in closeness to the great water-pumps made by ordinary tradesmen; and particularly the XIth experiment foregoing manifests, that by this pump quicksilver was raised to as great a height as the atmosphere is able to support in the Torricellian experiment.

II. The stanchness of the pipe appeared by the diminution (as to number) of bubbles that appeared at the top of the water, and by their size too; for when there was a leak (though but so very small, that the water could not get out at in the tube) it might usually be taken notice of by the attentive ear of him that stood to watch upon the ladder erected by the side of the tube; and the air that got in did easily discover itself to the eye by large bubbles, manifestly differing from those that came from the aerial particles belonging

belonging to the water; and if the leak were not so very small, the air that got in would suddenly lift up the water above it, and perhaps fill with it the descending pipe.

III. THOUGH there had been some imperceptible leak, yet that would not have hindered the success of the experiment for the main; for in leaks that have been but small, though manifest enough, we have often, by causing the pump to be plied less nimbly than it now was, been able to prosecute our trials; because the pump carried off still more air than could get in at a leak that was no greater.

IV. AND that little or no intruding air was left in the upper part of our tube, was evident by those marks, whereby it was easy for them that are well acquainted with the pump, to estimate what air is left in the vessel it should exhaust; and particularly towards the end of our operation I observed, that when the sucker was depressed, there came out of the water that covered the pump so very few bubbles, that they might be imputed to the air afforded by the bubbles springing from the water in the tube; whereas if any adventitious air had got into that cylinder of water, it would have appeared in the water that covered the pump.

V. LASTLY, it were very strange, that if the water was but casually hindered by some leak from ascending any higher, it should be so easy to raise it to the very number of feet that our hypothesis requires, and yet we should be unable by obstinate pumping to raise it one foot higher.

N. B. 1. As soon as we had made our experiment, and thereby found that what was requisite to it was in order; I sent to give notice of it to Dr. *Wallis* and Dr. *Wren*, as persons whose curiosity makes them as well delighted with such trials, as their deep knowledge makes them most competent judges of them. But before they could be found, and come, it being grown somewhat late and windy, I, that was not very well, and had tired myself with going up and down, could not stay with them so long as I intended, but leaving the rest of the repeated experiments to be shewn them by J. M. (who had been very industrious in fitting and erecting the tube) they and their learned friend (whom they brought with them) Dr. *Millington* told me a while after, that they also had found the greatest height to which they could raise the water to be 33 feet and an half.

2. WHEN the water began first to appear in the glass, the bubbles would be, as I had foretold, exceeding numerous so as to make a froth of near a foot high, if the water were newly brought, and had never been raised in the tube before. But if the pumping were long continued, the number and height (or at least one of the two) of the aggregate of bubbles would (as there remained fewer and fewer aerial particles in the water) be lesser and lesser; but their emerging did never, that I remember, wholly cease.

3. AT the beginning also there would appear great vibrations of the water in the upper part of the tube; the rising and the falling amounting sometimes to a foot, or near half a yard: but these grew lesser and lesser, as those of the quicksilver in the Torricellian experiment use to do.

4. ONE may use an ordinary pail to hold the stagnant water; but we rather employed a vessel of earth, made for another purpose, somewhat slender and of a cylindrical shape, because in a narrow vessel it is more easy to guess by the rising and falling of the liquor, how the pump is plied, and to perceive even smaller leaks.

5. I MUST not forget to take notice, that though the newly named gentlemen came to me (when they had seen the experiment tried) within less than an hour after the time I had looked upon the baroscope, and observed the quicksilver to stand somewhat beneath 29 inches and three eighths; yet, when presently upon their return I consulted



the same instrument again, the mercury appeared to be sensibly risen, being somewhat (though but very little) above nine and twenty inches and three eighths; and five or six hours after (at bed-time) I found it to be yet more considerably risen. Which may keep your lordship from wondering at what I intimated a little above, touching Monsieur *Pascal*'s experiment, as well as touching the disappointment of the pump-makers endeavours. For it is not only possible, that (as I have elsewhere noted) water may be raised in the same pump, though we suppose it still equally stanch, higher at one time than at another: but it was contingent, that, in Monsieur *Pascal*'s noble attempt to imitate the Torricellian experiment with water instead of quicksilver, the proportion betwixt the heights of those two liquors in their respective tubes answered so well to their specific gravities; for the varying weight of the atmosphere being not then, that appears, known, or consequently taken into consideration; if Monsieur *Pascal*, having tried the Torricellian experiment when the air was, for instance, very heavy, had tried his own experiment when the atmosphere had been as light as I have often enough observed it to be, he might have found his cylinder of water to have been half a yard or two feet shorter than the formerly measured height of the quicksilver would have required.

I HAVE now no more to add about this fifteenth experiment, but that it may serve for a sufficient confirmation of what I note in another treatise, against those hydraulical and pneumatical writers who pretend to teach ways of making water pass by inflected pipes, and by the help of suction, from one side of a mountain to the other, be the mountain never so high. For if the water be to ascend as it were spontaneously above 35 or 36 feet, a sucking pump will not ordinarily, at least here in *England*, be able to raise it.

AND now I speak of mountains, it will not be altogether impertinent to add, that if it had not been for unseasonable weather, I had thought fit to make the foregoing eleventh experiment (of elevating mercury by suction) to be tried at the top of an hill not far from the place I then was at. For by what has been already delivered, it appears, that we might have estimated the height to which the water may be there elevated by suction, without repeating the experiment with a thirty-five feet tube (which we could not hope for conveniency to do) by the utmost height to which our engine could have raised mercury; and it may be of some use to be able from experiments to make some estimate (for it can scarce be an accurate one) how much it may be expected that pumps shall (*cæteris paribus*) lose of their power of elevating water by suction, by being employed at the top of an hill, instead of being so at the bottom, or on a plain. Remembering always what I lately intimated, that even in the same place liquors will be brought to ascend by suction to a greater or less height at one time than another, according to the varying gravity of the atmosphere.

## EXPERIMENT XVI.

*About the bending of a springy body in the exhausted receiver.*

THE cause of the motion of restitution in bodies, and consequently of that which makes some of them springy, which far the greater part of them are not, has been ingeniously attempted by some modern corpuscularians, and especially Cartesians. But since divers learned and judicious men do still look upon the cause of elasticity as a thing that needs to be yet farther inquired into; and because I am not myself so well satisfied as to

blame their curiosity, I held it not unfit to examine by the help of our engine their conjecture, who imagine that the air may have a great stroke in the making of bodies springy; and this I the rather did, because I had \* elsewhere shewn that there is no need to assert, that in all bodies that have it, the elastical power flows immediately from the form, but that in divers of them it depends upon the mechanical structure of the body.

To make some trial, therefore, whether the air have any great interest in the motion of restitution, we took a piece of whalebone of a convenient bigness and length; and having fastened one end of it in a hole made in a thick and heavy trencher, to be placed on the plate of the engine, we tied to the other end a weight, whereby the whalebone was moderately bent, the weight reaching down so near to a body placed in a level position under it, that if the spring were but a little weakened, the weight must either lean upon, or at least touch the horizontal plain; or if on the other side the spring should grow sensibly stronger, it might be easily perceived by the distance of the weight, which was so near the plain, that a little increase of it must be visible.

This done, we conveyed these things into the receiver, and ordered those that pumped to shake it as little as they could, that the weight might not knock against the body that lay under it, or so shake it, as to hinder us from discerning whether or no it were depressed by the bare withdrawing of the air.

AND when the air had been well pumped out I watched attentively, whether any notable change in the distance of the weight from the almost contiguous plain would be produced upon its being let in again; for the weight was then at rest, and the returning air flowing in much more speedily than it could before be drawn out, I thought this the likeliest time to discover whether the absence of the air had sensibly altered the spring of the whalebone: but though the experiment were made more than once, I could satisfy myself only in this, that the depression or elevation of the weight that was due to the true and mere change of the spring, was not very considerable, since I did not think myself sure that I perceived any at all: for though it be true that sometimes, when the receiver was well exhausted, the weight seemed to be a little depressed, yet that I thought was very little, if any thing more than what might be ascribed to the absence of the air, not considered as a body that had any thing to do directly with the spring, but as a body that had some (though but a little) weight; upon which account it made the medium, wherein the experiment was tried, contribute to support the weight that bent the spring; which weight, when the air was absent, must (being now in a lighter medium) have its gravitation increased by as much weight as a quantity of the exhausted air, equal to it in bulk, could amount to: but this experiment being tried only with whalebone, and in a receiver not very great, may deserve to be further tried in taller glasses, with springs of other kinds, and by the motions of a watch, and other more artificial contrivances.

\* In notes about the history of Elasticity.



## EXPERIMENT XVII.

*About the making of mercurial and other gages, whereby to estimate how the receiver is exhausted.*

BECAUSE the air being invisible, it is not always easy to know whether it be sufficiently pumped out of the receiver that was to be exhausted, we thought it would be very convenient to have some instrument within the receiver that might serve for a gage or standard, whereby to judge whether or no it were sufficiently exhausted.

To this purpose divers expedients were thought on, and some of them put in practice; which, though not equally commodious, may yet all of them be usefully employed, one on this occasion, and another on that.

THE first (if I misremember not) that I proposed was a bladder (which may be greater or less, according to the size of the vessel it is to serve for) to be very strongly tied at the neck, after having had only so much air left in the folds of it as may serve to blow up the bladder to its full dimensions, when the receiver is very well exhausted, and not before. But though your lordship will hereafter find that I yet make use of small bladders on certain occasions, in which they are peculiarly convenient, yet in many cases they do, when the glasses are well exhausted, take up too much room in them, and hinder the objects included in the receiver from being observed from all the sides of it.

ANOTHER sort of gage was made with quicksilver poured into a very short pipe, which was afterwards inverted into a little glass of stagnant quicksilver, according to the manner of the Torricellian experiment. For this pipe being but a very few inches long the mercury in it would not begin to descend till a very great proportion of air was pumped out of the receiver; because till then the spring of the remaining air would be strong enough to be able to keep up so short a cylinder of mercury. And this kind of gage is no bad one. But because, to omit some other little inconveniencies, it cannot easily be suspended (which in divers experiments it is fit the gage should be) and the mercury in it is apt to be too much shaken by the motion of the engine, there was another kind of gage by some ingenious man (whoever he were) substituted in its place, consisting of a kind of siphon whose shorter leg hath belonging to it a large bubble of glass, most commonly made use of at an illustrious meeting of virtuosi; where your lordship having seen it, I shall not need to describe it more particularly.

BUT none of the gages I had formerly used, nor even this last, having the conveniences that some of my experiments require, I was fain to devise another, which is that I most make use of, as having advantages, some or other of which each of the gages already mentioned wants; for even that with spirit of wine, not to mention lesser disadvantages, hath a bubble too great to let it be useful in vessels so slender, as for some purposes I divers times employ; and this short cylinder of so light a liquor as spirit of wine makes the subsidence of the liquor be indeed a good sign, that the receiver is well exhausted, but gives us not an account what quantity of air may be in the receiver, till it be arrived at that great measure of rarefaction; and the same liquor, being upon a very small leak (such as would not be prejudicial to many experiments) impelled up to the top of the gage, we cannot afterwards by this instrument take any measure of the air that gets in at the leak: but now there are divers experiments where I desire to see the phenomena that will happen, not only (or perhaps not at all) upon the uttermost ex-

haustion of the air, but when the pressure of it is withdrawn to such or such a measure, and also when the air is gradually re-admitted.

See Plate To make the gage we are speaking of, take a very slender and cylindrical pipe of  
III. Fig. 4 glass of six, eight, ten, or more inches in length, and not so big as a goose-quill (but such as we employ for the stems of sealed weather-glasses) and having at the flame of a lamp melted it, but not too near the middle, to make of it by bending it a siphon, whose two legs are to be not only parallel to one another, but as little distant any where from one another as conveniently may be. In one (which is usually the longer of these legs, there is to be left at the top either half an inch or a whole inch, or more or less than either (according to the length of the gage or the scope of the experimenter) of air in its natural state, neither rarefied nor condensed; the rest of the longer leg, and as great a part of the shorter as shall be thought fit, being to be filled with quicksilver. This done there may be marks placed at the outside of the longer or sealed leg, whereby to measure the expansion of the air included in the same leg; and these marks may be either little glass knobs about the bigness of pins heads, fastened by the help of a lamp at certain distances to the longer leg of the siphon, or else the divisions of an inch made on a list of paper and pasted on, either to the siphon itself, or to the slender frame which on some occasions we fasten the gage to.

This instrument being conveyed into a receiver (which for expedition sake we choose as small as will serve the turn) the air is to be very diligently pumped out, and then notice is to be taken to what part of the gage the mercury is depressed, that we may know, when we shall afterwards see the mercury driven so far, that the receiver the gage is placed in, is well exhausted. And if it be much desired to know more accurately (for one may arrive pretty near the truth by guess) what stations of the mercury in the gage are answerable to the degrees of the rarefaction of the air in the receiver, that may be compassed either by calculation (which is not so easy, and supposes some hypotheses) or, though not without some trouble, by letting in the water as often as is necessary into a receiver, whose entire capacity is first measured, and in which there may be marks made to shew, when the water to be let in shall fill a fourth part, or half, or three quarters, &c. of the cavity. For if (for instance) when the quicksilver in the gage is depressed to such a mark you let in the water, and that liquor appears to fill a fourth part of the receiver, you may conclude that about a fourth part of the air was pumped out, or that a fourth part of the spring that the whole included air had was lost by the exhaustion, when the quicksilver in the gage was at the mark above-mentioned. And if the admitted water do considerably either fall short of, or exceed the quantity you expected, you may the next time let in the water either after the mercury has a little passed the former mark, or a little before it is arrived at it. And when once you have this way obtained one pretty long and accurate gage, you will not need to take so much pains to make others, since you may divide them by the help of that one; for this being placed with any other in a small receiver, when the mercury in the standard-gage (if I may so call it) is depressed to any of the determinate divisions obtained by observation, you may thence conclude how much the air in the receiver is rarefied, and consequently by taking notice of the place where the mercury rests in the other gage, you may determine what degree of exhaustion in a receiver is denoted by that station of the mercury in this gage.

PERHAPS I need not tell your lordship that the ground of this contrivance was, that whereas in divers other gages, when the pump came to be obstinately plied, the expansion of the included air would be so great, that it would either drive out the liquor, especially if it were light, or in part make an escape through it; I judged that in such an instrument as that newly described, those inconveniencies would be avoided, because  
that



that the more the air should come to be dilated, the greater weight of quicksilver it would in the shorter leg have to raise, which would sufficiently hinder it from making that heavy liquor run over; and the same ponderousness of the liquor, together with the slenderness of the pipe, would likewise hinder the included air from getting through in bubbles.

N. B. 1. For most experiments where exact measures are not required, it will not be so necessary to mark the gage at any other station of the quicksilver than that which it is brought to by the exhaustion of the receiver; for by that alone we may know, when the air is well pumped out of the receiver wherein the gage is included: and when one is a little used to some particular gage, one may by the subsidence of the mercury guess at the degree of the air's rarefaction, so near as may serve the turn in such experiments. But when this instrument is to be used about nice trials, where it may be thought requisite to have it divided according to one of the ways formerly proposed, it will on divers occasions be more secure (in case the maker of the gage has skill to do it) to put to the divisions rather by little knobs of glass than by paper; because this will on such occasions be in danger either to be rubbed off or wetted. And if glass-marks be used, it will be convenient that every fifth, or tenth, or such ordinal number as shall be judged fit, be made of glass of a differing colour, for distinction sake, and the more easy reckoning: we sometimes for a need apply, instead of these glass-knobs, little marks of hard sealing-wax, which will not be injured by moisture, as those papers will that are pasted on; but these of wax, though in many cases useful, are not comparable to the other in all; since if they be very small, they are easily rubbed off; and if large, they make not the division exact enough, and often hide the true place of the quicksilver.

2. I SHALL here, about the mercurial gages, add only this hint, that what I proposed to myself in that contrivance was not only to estimate the air pumped out of the receiver, or that remaining in it; but also, by the help of this instrument (as elsewhere by another experiment) to measure (somewhat near) the strength of the spring of rarefied air, according to its several degrees of rarefaction; and by this observation, in concurrence with other things, I hoped we might (according to what I have elsewhere insinuated) be assisted to estimate, by the cylinder of mercury raised in the open leg, the expansion of the air included in the sealed leg: but of these things I designed in this place to give but an intimation.

3. THAT leg of the gage that includes the air, may be sealed up either at the beginning, before the pipe be bent into a siphon, or (which is much better) after the following manner. Before you bend the pipe, draw out the end of it, which you mean to seal, to a short and very slender thread; then having made the pipe a siphon, pour into the leg, which is to remain open, as much quicksilver as you shall judge convenient, which will rise to an equal height in the other leg; out of which, by gently inclining the siphon, you may pour out the superfluous mercury (if there be any) and when you see that there is an inch, or half an inch (or what part you designed to leave for air) unfilled with mercury next to the end that is to be closed, and that the rest of that leg, and as much (as you think fit) of the other is full of quicksilver, you may, by keeping the siphon in the same posture, and warily applying the slender apex above-mentioned to the upper part of the flame of a lamp, blown horizontal, easily seal up that apex without cracking or prejudicing the open leg, or considerably injuring the air-hole that was to be sealed up in the other. And this sealing of one leg must (as it is evident) keep the mercury suspended in it, though it be higher by divers inches than that in the open leg, till the withdrawing of the external air enable the included, by expanding itself, to depress the mercury in the sealed leg, and raise it in the open.

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4. How the length of these mercurial gages is to be varied, according to the bigness and shape of the slender receivers they are to be employed in; and how they may easily be made either to stand upright at the bottom of the receiver, or be kept hanging in the middle, or near the top of it (as occasion may require); and how the open end may be made to secure the mercury, in cases where that is needful, belongs not so properly to this treatise as to the second part of the Continuation; where, if ever I trouble your lordship with it, the usefulness of this sort of gages, and the circumstances that may advantage them, will best appear.

5. THERE being some experiments, wherein it is not desired that the receiver should be near exhausted, but rather that the degrees of the air's rarefaction, which ought not to be very great, should be well measured; we may in such cases make use of gages shaped like those hitherto described, but made as long as the receiver will well admit, and furnished instead of quicksilver either with spirit of wine coloured with cochineal, or else with the tincture of red rose-leaves, drawn only with common water made sharp by a little either of the oil, or spirit of vitriol, or of common salt. For the lightness of these liquors in comparison of quicksilver will allow the expansions of the air included in the gage to be very manifest and notable enough, though not half or perhaps a quarter of the air be pumped out of the receiver.

6. You may also in such cases as these, where the receiver is large enough, and is not to be quite exhausted, make use of a mercurial gage, differing from those above described only in this, that the shorter leg need not to be above an inch or half an inch long before it expand itself into a bubble of about half an inch or an inch in diameter, and having at the upper part a very short and slender unsealed pipe, at which the air may get in and out; by which contrivance you may have this convenience, that you need not include so much air, as otherwise would be requisite, at the top of the longer leg, because the mercury in the shorter cannot, by reason of the breadth of the bubble, whereinto the expansion of the air drives it, be considerably raised; upon which account it becomes more easy to estimate by the eye the degrees of the included air's rarefaction, which may be done almost as easily as if there were water instead of mercury, provided it be remembered that quicksilver, by reason of its ponderousness, does far more assist the dilatation of the air, than so much water would do.

## EXPERIMENT XVIII.

*About an easy way to make the pressure of the air sensible to the touch of those that doubt of it.*

THOUGH several of our experiments sufficiently manifest to the skilful that the pressure of the air is very considerable; yet, because some of them require peculiar glasses and other instruments, which are not always at hand, and because there are many that think it surer to estimate the force of pressure by what they immediately feel than by any other way, I was invited for the sake of such to employ an easy experiment, which usually proved convincing, because it operated on that sense whereon they chiefly relied.

I caused then to be made a hollow (but strong) piece of brass, not above two or three inches high (that it might be in a trice exhausted) and open at both ends, whose orifices were circular and parallel, but not equal (the instrument being made tapering, so that it might be represented by an excavated *conus truncatus*, or a glegg, with the lower  
part



part cut transversely off. This piece of brass being cemented on, as if it were a small receiver to the engine, the person, that would not believe the pressure of the air to be near so considerable as was represented, was bidden to lay the palm of his hand upon the upper orifice; and being ordered to lean a little upon it, that so the lower part of his hand might prove a close cover to the orifice, one exsuction of the air was made by the help of the pump; and then upon the withdrawing of the greatest part of the pressure of the internal air, that before counterbalanced that of the external, the hand being left alone to support the weight of the ambient air, would be pressed inwards so forcibly, that though the stronger sort of men were able (though not without much ado) to take off their hands, yet the weaker sort of triers could not do it, especially if by a second suck the little receiver were better exhausted, but were fain to stay for the return of the air into the receiver to assist them.

THIS experiment being designed rather to convince than to punish those that were to make it, we took care, not only that the brass should be so thick and the orifices so smooth, that no sharpness nor roughness of the metal should offend the hand; but also that the narrower orifice (which was the ofteneft made use of) should be but about an inch and a quarter in diameter. But if any were desirous of a more sensible conviction, it was very easy to give it him by making the larger orifice the uppermost, which was the reason why the instrument was, as we formerly noted, made tapering; but yet this larger orifice ought not to exceed two inches, or two inches and a half in wideness, lest the great weight of the air endanger the breaking or considerably hurting the hand of the experimenter: which caution I am put in mind of giving, by remembering that I once much endangered my own hand, through the mistake of him that managed the pump, who, unawares to me, set it on work, when for another purpose I had laid my hand upon the orifice of an instrument of too great a diameter.

*The famous experiment of Torricellius, mentioned in the 17th of our already published trials, is of that nobleness and importance, that though divers learned men have (but upon very differing principles) discoursed of it in print, which gives me the less mind to insist long upon it here, yet I shall not scruple to subjoin some notes concerning trials that I made (though for want of opportunity I could not repeat them according to my custom) which I had not met with in others, and which may serve to confirm the hypothesis made use of in this Continuation and the treatise it belongs to.*

## EXPERIMENT XIX.

*About the subsidence of mercury in the tube of the Torricellian experiment to the level of the stagnant mercury.*

A BAROSCOPE being included in a receiver made of a long bolt head with the lower part of the ball cut circularly off, upon the first exsuction of the air the quicksilver that before stood at 29 inches (the atmosphere appearing then by a constant baroscope very light) would fall so low as to rest at 9 or 10 inches (for once I measured the subsidence beneath its former elevation) and in about three sucks more it would be brought quite down to the level of the stagnant quicksilver, and somewhat below (as it is the property of quicksilver, quite contrary to water, to rise less in a slender pipe than in a wide).

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The air being let into the receiver, the quicksilver would be impelled up flowlier or faster, as we pleased, to the former height of 29 inches or thereabouts.

N. B. THAT if the air were suffered to go hastily out of the receiver, the mercury would, by virtue of the accelerated motion acquired in its descent, at the very first suck descend till it reached within an inch or two of the stagnant mercury, though it would presently after a few risings and fallings settle at the height of 9 or 10 inches, till the next brought it down lower.

2. IF when the mercury was reimpelled up to its due height, those that managed the pump did, instead of rarefying the air, a little compress it, the quicksilver would by the compressed air be easily made to rise an inch or more above the former standard of 29 inches; which circumstance I mention, not as a new thing, but to confirm (what some think strange) a passage printed in *New Experiments*, Exper. XVII. where I mention, that if the air in the receiver, instead of being rarefied in the engine, were a little compressed by it, the pressure of the included air being somewhat increased by having its spring thus bent, would sustain the mercury in the Torricellian tube at a greater than the wonted height.

AND to confirm another passage in the same page, where I observed, that if the pressure of the air upon the stagnant mercury be not so great as it is wont to be, the mercury will begin to subside in a (filled and inverted tube, which wants of the usual height; we took a glass cane (sealed at one end) much shorter than the due length, and having filled it with mercury, and inverted it into a glass full of stagnant mercury, we placed all in the former receiver; where the mercurial cylinder, for want of the requisite height, remained totally suspended, but upon the first or second suck it would subside, and in two or three sucks more it would fall to the level of the stagnant mercury, or a little below it. Upon the letting in of the air it would be impelled to the very top of the tube, bating an aerial bubble, which seemed to come from the mercury itself, and was so little as not to be at all discernible, save to a very attentive eye.

Experi-  
ment  
XVII.

THIS experiment I should not think fit here to relate, since I formerly acquainted your lordship with the subsidence of the mercury upon the withdrawing of the air from the receiver, were it not that, in the mention of that trial, I remember I confessed to you that I could not so free the great receiver I then used from air, but that the little that remained or leaked in, made me unable to bring the mercury in the tube totally to subside or fall much nearer than within an inch of the surface of the stagnant mercury, with which in our present trials that in the tube was brought to a level.

## EXPERIMENT XX.

*Shewing that in tubes open at both ends, when no fuga vacui can be pretended, the weight of water will raise quicksilver no higher in slender than in large pipes.*

BECAUSE I find it, even by learned and very late writers, urged as a clear and cogent argument against those that ascribe the phænomena of the Torricellian experiment to the weight of the external air; that it is impossible that the air, though it were granted to be a heavy body, could sustain the quicksilver at the same height in tubes of very differing bigness, since the same air cannot equally counterpoise mercurial cylinders of such unequal weights; and because this objection is wont very much to puzzle those that are not well acquainted with the hydrostatics, I presume your lordship will allow me, till I can shew you some hydrostatical papers, by which the objection may appear



to be but ill grounded upon the true theorems of that art, to annex the transcripts of a couple of experiments (that I once made to remove this supposedly insuperable difficulty) just as I find them registered in my note-books.

#### THE FIRST TRIAL, Sept. 2, 1662.

WE took a very large glass-tube, hermetically sealed at one end, and about two feet and a half in length; into this we poured quicksilver to the height of three or four fingers; then we took a couple of cylindrical pipes of very unequal sizes, the wider being as big again as the slenderer, and open at both ends. The lower ends of these two pipes we thrust into the quicksilver, and fastened them near their upper ends to the tube with strings, that they might not be lifted up nor moved out of their posture, in which the convex surface of the mercury in both the pipes seemed to lie almost in a level, the tube also itself being placed upright in a frame. This done, by the help of a funnel we poured in water by degrees at the top of the tube, and observed, that as the water gravitated more and more upon the stagnant mercury, so the included mercury rose equally in both the pipes, until the tube being almost filled with water, the mercury appeared to be impelled up to, and sustained at as great a height in the big tube as in the lesser, being in either raised about two inches above the surface of the stagnant quicksilver.

N. B. 1. HAVING caused about half the water (having no conveniency to withdraw any more) in the tube to be sucked out at the top, we observed the quicksilver in both the tubes to subside uniformly, and to re-ascend alike upon the re-affusion of the water.

2. WE endeavoured to try the experiment (for their sake, who have not the conveniency to have such tubes purposely made) in a wooden vessel, into which, when it was filled with water, we let down a flat glass furnished with stagnant mercury, wherinto the ends of the two pipes were immered. But the opacousness of the cylinder (which reduced us to see only from the top the reflection of the stagnant mercury) and other impediments, disabled us to perceive the motions and stations of the mercury in the pipe, though we once made use of a candle the better to discern them.

#### THE SECOND TRIAL.

WE took a very wide tube of glass, of about a foot long, and into it poured a convenient quantity of quicksilver; we took also two pipes of about equal length, and of that disparity in bigness that we newly mentioned (those pipes lately described being indeed cut off from these we are now to speak of) and these being filled with quicksilver, after the manner of the Torricellian experiment, were by a certain contrivance let down into the tube, and unstopped under the surface of the stagnant mercury, and then the quicksilver in the pipes falling down to its wonted station, and resting there, we poured into the tube about a foot height (by guess) of water, whereupon the quicksilver, as it before stood, as it were, in a level in both the pipes, so it was, for aught appeared to us, equally impelled up beyond its wonted station, and sustained there, both in the slender and in the bigger pipe, and upon the withdrawing of some of the water it began to subside alike, as to sense, in them both, falling no lower in the bigger than in the slenderer. And water being a second time poured down into the tube, the mercury did in both pipes rise uniformly as before. By which and the former experiment it sufficiently appeared that a gravitating liquor, as air or water, may impel or keep up

mercury to the same height in tubes that are of very differing capacities; and that liquors balance each other according to their altitude, and not barely according to their weight. For in this last experiment, the additional cylinder of one inch of mercury was manifestly raised and kept up by the water incumbent on the stagnant mercury, the other cause, whatever it were, of the mercury's suspension, being able to sustain but a cylinder shorter by an inch. And the same parcel of water did counterpoise in the differing pipes two mercurial cylinders, which though but of the same altitude (namely about an inch) were of very unequal weight.

## EXPERIMENT XXI.

*Of the heights at which pure mercury, and mercury amalgamated with tin, will stand in barometers.*

CONSIDERING with myself, that if the sustentation of the quicksilver in the Torricellian experiment at a certain height depends upon the æquilibrium, which a liquor of that specific gravity does at such a height attain to with the external air; if that peculiar and determinate gravity of the quicksilver be altered, the height of it, requisite to an æquilibrium with the atmosphere, must be altered too; considering this, I say, I thought it might somewhat confirm the hypothesis hitherto made use of, if a phenomenon so agreeable to it were actually exhibited. This I supposed performable two differing ways, namely, by mixing, or as chymists speak, amalgamating mercury either with gold, to make it a mixture more heavy, or with some other metal, that might make it more light than mercury alone is. But the former of those two ways I forbore to prosecute, being, where I then was, unfurnished with a sufficient quantity of refined gold, for that which is coined is generally alloyed with silver, or copper, or both; and therefore amalgamating mercury with a convenient proportion of pure tin, or, as the tradesmen call it, block-tin, that the mixture might not be too thick to be readily poured out into a glass-tube, and to subside in it, we filled with this amalgam a cylindrical pipe sealed at one end, and of a fit length, and then inverted it into a little glass furnished with the like mixture. Of which trial the event was, that the amalgam did not fall down to 29, nor even to 30 inches, but stopped at 31 above the surface of the stagnant mixture.

N. B. 1. THAT though one may expect that the event of the experiment would be the more considerable, the greater the quantity is that is mingled of the light metal; yet care must be taken that the amalgam be not made too thick, lest part of it stick here and there (as we did to our trouble find it apt to do) to the inside of the pipe, by which means some aerial corpuscles will meet with such convenient receptacles, as to make it very difficult, if not almost impossible, to free the tube quite from air.

2. It may perhaps be worth while to try, whether by comparing the height of the amalgam to what it ought to be upon the score of the specific gravities of the mercury, and the tin, mingled in a known proportion in the amalgam, any discovery may be made, whether those two metals do penetrate one another after such a manner (for there is no strict penetration of dimensions among bodies) as copper and tin have, as I elsewhere note, been by some chemists observed to do, when being melted down together, they make up a more close and specifically ponderous body than their respective weights seemed to require.

3. THAT



3. THAT by comparing this 21<sup>st</sup> experiment with the 18<sup>th</sup> of those formerly published it may appear, that the height of the liquor, suspended in the Torricellian experiment, depends so much upon its æquilibrium with the outward air, that it may be varied by a change of gravity in either of the two bodies that counterbalance each other, whether the change be of weight in the atmosphere, or of specifick gravity in the suspended liquor.

## ADVERTISEMENT.

I SHOULD here acquaint your lordship with what I have since tried, in reference to the 18<sup>th</sup> of the printed experiments, where I mention that I observed by long keeping the same instrument, with which I once made the Torricellian experiment, in the same place, that the height of the suspended mercury would vary according as the weight of the atmosphere happened to change. But though about the barometer (as others have by their imitation allowed me to call the instrument hitherto mentioned, put into a frame) I made in the year 1660 several observations that would not perhaps be impertinent in this place, yet having long since left them with a friend who lives far off, and not having them now in my power, I must beg your lordship's permission to reserve them for a part of the appendix, which I doubt I shall be engaged to add to this epistle; and in the mean time I shall not forbear to present your lordship those other papers that I have by me, relating to the barometer; some of which will, I presume, sufficiently confirm my lately mentioned conjecture about the cause of the variation observed in the height of the suspended mercury.

## EXPERIMENT XXII.

*Wherein is proposed a way of making barometers that may be transported even to distant countries.*

THINKING it a desirable thing (as I have elsewhere intimated) to be able to compare together, by the help of barometers, the weight of the atmosphere at the same time, not only in differing parts of the same country, as of *England*, but in differing regions of the world; I could not but foresee that it would be very difficult to accomplish my desire without altering the form of the barometers I had hitherto made use of. For as these be unfit to be transported far, because that stagnant mercury would be so apt to spill; so the procuring them to be made in the places where they are to be used, though it be no bad expedient, and such as I have divers times made use of, is liable to this inconvenience, that, besides that few will take the pains, and have the skill requisite to make baroscopes well, though they be sufficiently furnished with glasses and mercury for that purpose; besides this, I say, except men be more than ordinary diligent and skilful (and perhaps though they be) it will be very difficult to be sure, that the baroscope newly made in a remote country is as good (and but as good) as that which a man makes use of in this; in regard that at the making of the former, they are supposed to have no other baroscope to compare it with; and to be sure they have not the same with which it is to be compared here.

BEING by these considerations invited to attempt the making of portable or travelling baroscopes (if I may so call them) I thought it requisite to endeavour these three things

things: the first, to make the vessel that should contain both the sustained and the stagnant mercury, all of one piece of glass, of a like bigness; the next, to place this vessel when filled, in such a frame as may be easy to be transported, and yet in a reasonable measure defend the glass from external violence, no part of it standing quite out of the frame, as in all other baroscopes; and the third, so to order the vessel, that it may not be subject to be easily broken by the violent motion of the mercury contained in it.

THE first of these will not seem practicable to those that imagine (without any warrant from the hydrostaticks) that it is as well necessary as usual, that the stagnant mercury should have a vessel much wider than the tube, wherein the mercurial cylinder is sustained; but to us the difficulty seemed much less to make the glass part of our tube of one piece, and of a convenient shape, than afterwards to fill it.

BUT to do both, we took a glass cylinder sealed at one end, and of a convenient length (as about four or five feet) and caused it by the flame of a lamp to be so bent, that to those that did not take notice it was sealed at one end, it seemed to be a siphon of very unequal legs, the one being three or four times longer than the other; by virtue of which figure the shorter leg may serve instead of the distinct vessel usually employed to contain the stagnant mercury. To fill this, which is not easy, one may proceed after this manner: take a small funnel of glass with a long and slender shank, so that it may reach three or four inches, or further, into the shorter leg of our barometrical siphon (if I may so call it) and by this funnel pour into this shorter leg as much mercury as may reach about two or three inches in both legs; then stopping the orifice with your finger, and slowly inclining the tube, the mercury in the longer leg will gently fall to the sealed end; and the air that was there before, will pass by it, and so make it room. The mercury in the shorter leg (which leg ought to be held uppermost) will by the same inclination of the tube fall towards the orifice; but being by the finger that stops that, kept from falling out, if you do slowly re-erect the glass, and then make it stoop again as much as before, the mercury will pass out of the shorter leg into the longer, and join with that which was there before; and if all the mercury do not so pass, the orifice is to be stopped again with your finger, and the tube inclined as formerly. This done, the tube is to be erected, and by the help of the funnel more mercury is to be poured in, and the foregoing process of stopping the orifice, inclining the tube, &c. is to be repeated, till all the mercury poured into the shorter leg be brought to join with that in the longer; and then the open leg is to be furnished with fresh mercury, observing this, that the nearer the longer leg comes to the being filled, the less you must raise it from time to time, when you pour mercury into the shorter; as also that when you see the longer leg quite full of mercury (though there be but little in the shorter) you need not pour in any more, if the longer do much exceed a yard; because upon the restoring of the tube to an erected posture, there will subside from the taller leg into the other a pretty quantity of mercury, by reason of the space at the sealed end, which will be deserted by the mercury that was there. But because it is difficult by this way, as well as by that practised already, to fill a tube with mercury without leaving any visible bubbles; to free it from such (if any happen to be) you must once more stop the orifice with your finger, and incline and re-erect the tube divers times, till you have thereby brought most of the smaller bubbles into one greater (which you may if you please increase, by letting in a little air); for by making this great bubble pass leisurely two or three times from one end of the tube to the other, it will in its passage as it were lick up all the small bubbles, and unite them to itself; which

may



may afterwards by one inclination more of the tube be made to pass into the shorter leg, and thence into the free air.

BUT there is another sort of funnels, which if one have the skill and conveniency to make (as *I. M.* easily doth) one may very expeditiously fill the bended tubes of our portable barometers. For if you make the slender part of the funnel not straight, but bended in the form of an obtuse angle, and of such a length, that the part which is to go into the shorter leg of our siphon, may reach to the flexure (of the siphon) then you may, by so holding the tube that the sealed end be somewhat lower than the other, and by pouring in mercury at the obtuse end of the angular funnel, easily make it run over the flexure into the longer leg of the siphon; provided you do now and then, as occasion requires, erect a little and shake the tube, to help the mercury to get by the air, and expel it.

By such ways as these we have found by experience that it is possible (though not easy) to do in such a bended glass as our purpose requires, what, besides a very late learned writer, the diligent *Marsennus* himself admonishes his reader, that it is not a practicable thing to do in the ordinary glasses of the Torricellian experiment, viz. to free the mercury of a straight tube from air and bubbles, so as to be able by inclining the glass to make the liquor ascend to the very top.

THE first of our three above-mentioned scopes being thus attained, it was not difficult to compass the second by the help of a solid piece of wood, which is to be somewhat longer than the tube, and a good deal broader in the lower part than in the upper, that it may receive the shorter leg of the siphon. In such a piece of wood, which was about an inch thick, we caused to be made a gutter or channel of such a depth and shape, that our siphon might be placed in it so deep, that a flat piece of wood (like a plained lath) might be laid upon it, without at all pressing upon, or so much as touching the glass; so that this piece of wood may serve for a cover to defend the glass, to be put on when the instrument is to be transported, and taken off again, when it is to be hung up to make observations with; the channel piece of wood serving both for a part of a case, and for an entire frame; which may for some uses be a little more commodious if the cover be joined (as it may easily be) to the rest of the frame, by two or three little hinges and a hasp, by whose help the case may be readily opened and shut at pleasure.

THE third thing we proposed to ourselves is nothing near so easy as the second; nor have we yet had opportunity to try, whether the way we made use of will hold, if the barometer be transported into very remote parts, though by smaller removes we found cause to hope that it will succeed in greater.

THE grand difficulty to be obviated was this; that though it were easy to hinder the spilling of the mercury, by stopping the orifice of the shorter leg of our siphon, yet that would not serve the turn; for the upper part of the tube being destitute of air, if the mercury be by the motion of the instrument put to vibrate, it will be apt (for want of meeting with any air in the upper part of the tube to check its motions) to hit so violently against the top of the glass as to beat it out, or to crack some of the neighbouring parts.

To obviate this great inconvenience, our way is to incline the tube till the mercury be impelled to the very top of it, and yet there will remain a competent quantity in the shorter leg of the glass, if that be not at first made too short. This done, the remaining part of the shorter leg is to be quite filled up either with water or mercury, and the orifice of it is to be very carefully and firmly stopp'd, for which purpose we use our strong black cement; for by this means the mercury in the longer leg having no

See the  
whole ba-  
roscope.  
Plate V.  
Fig. 2.

room to play, cannot strike with violence as before against the top of the glass. But though by many times successively shaking the baroscope we did not perceive that it was very like to be prejudiced by the shakes it might necessarily endure in transportation to remote places, if due care be had of it by the way, yet till further trial have been made I shall not pretend to be certain of the event. But thus much of convenience we have already found in this contrivance, that we sent it some miles off, to the top of a hill, and had it brought home safe again, the phenomena at the top and bottom of the hill being answerable to what we might have expected if we had employed another baroscope.

WHEN the instrument is to be sent away, the height of the mercurial cylinder (to be measured from the surface of the stagnant mercury in the shorter leg) being taken for that place, day, and hour, and compared (if it may be) with that of another good baroscope, which is to continue in that place; as much of the gutter as is unfilled by the glass may be well stuffed with cotton, or some such thing, to keep the glass the more firm in its posture; and that the tube be not shaken or pressed against the wood, some of the same matter may be put between the rest of the frame and the cover, which ought to be well bound together. And when the instrument is arrived at the remote place where it is to be employed (for if it be to be sent but a little way, it may be carried safely without using any adventitious liquor) the water that is added may be taken off again, by soaking it up with pieces of sponge, linen, &c. but if instead of water you put in mercury, as it ought to have been put in by weight, so it is to be taken out, till you have just the weight that was put in; and it is not difficult to take out the mercury by degrees, by the help of a small glass-pipe, since you may either suck up little by little as much as remains of the additional mercury, when by erecting the barometer, and warily unstopping the orifice of the lower leg, as much mercury as will of itself flow out is effluxed; or else you may take out the superfluous mercury, by thrusting the lower end of the pipe into that liquor, and when it has taken in enough, stopping the upper end close with your finger, to keep it from falling back again when you remove the pipe.

N. B. If it should happen in a long voyage, that by the numerous shakings of the instrument there should, from the additional water or mercury in the shorter leg, get up into the longer any little aerial bubble, which seems the only, but I hope not likely, danger in this contrivance, he that is to use the instrument at the end of the voyage may, if he be skilful, free the mercury from it by the same way that we lately prescribed to free it from air, when the instrument was first filled.

I PRESUME I need not tell your lordship that the chief use of this travelling baroscope is, that he that uses it in a remote part, keeping a diary of the heights of the mercury, by comparing these heights with those at which the mercury stood at the same times in the barometer that was not removed, the agreement or difference of the weight of the atmosphere in distant places may be observed. To which this may be added, the convenience which the structure of these instruments gives them to be securely let down into wells or mines, and to be drawn up to the top of towers, and steeples, and other elevated places; not here to consider whether by a convenient addition, these, as well as some other barometers, may not be made to discover even very minute alterations of the atmosphere's pressure.

WHETHER this travelling baroscope, being furnished at its upper end with a very good ball and socket, and at the lower end with a great weight (which way of keeping things steady in a ship has been happily used by the Royal Society on another occasion) whether, I say, our instrument may by this contrivance, or some other that might be sug-



gested to the same purpose, be made any thing serviceable at sea, notwithstanding the differing motions of the ship, I have had no opportunity to try; but whether it may or may not be useful in spite of the rolling of the ship, it may at least be made use of in flat calms (which divers times happen in long voyages, especially to the *East Indies*, and to *Africk*) and then the instrument which at other times may lie by without being at all cumbersome, may be made use of, as long as the calm lasts, to acquaint the observer with the weight of the atmosphere in the climate where he is, and that upon the sea; which may give some welcome information to the curiosity of speculative naturalists, and perhaps prove either more directly, or in its consequences of some use to navigators themselves, as by enabling them by its sudden changes to foretel the end of the calm. Besides that having one of these instruments ready at hand, wherever they set foot on shore, though it be but upon a small island, or a rock, they can presently and easily take notice of the gravity of the atmosphere in that place; which whether or no, if compared with other observations, it may in time prove not altogether useless to the guessing whereabouts they are, and the foreseeing some approaching changes of weather, I leave to future experience, if it shall be thought worth the making to determine.

BESIDES the ordinary baroscope, and this travelling one, I have employed two or three other instruments of quite differing kinds, to discover the varying gravities of the atmosphere; but though they have hitherto succeeded well for the main, yet being willing to make further observations about them, I reserve one of them for another opportunity, and think fit to leave the other in a tract it belongs to.

#### A POSTSCRIPT ADVERTISEMENT.

SINCE the writing of the foregoing and the following experiments about the travelling baroscope, having had occasion to make one at a place about fifty miles distant from that where I was when I writ them, I took notice that the mercury in the travelling baroscope was not by  $\frac{1}{4}$  of an inch so high as that in another baroscope made the ordinary way; and yet it was not easy to perceive that the former had been less carefully filled than the latter. So that I yet know not well to what cause to impute the difference, unless it should perhaps depend upon this circumstance; that the pipe whereof the travelling baroscope was made, was very slender, and much more so than the tube of the other; and I have already elsewhere observed that mercury, contrary to what happens in water, is less apt to rise in very slender pipes. And though I remember that at the place where I writ the experiment to which this postscript belongs, in the tube I then employed to make the travelling baroscope, the mercury ascended as high as in a noted one made the common way; yet not being in the other place furnished with a tube long and big enough, I think myself obliged, till I can clear the doubt by further trial, to give your lordship this advertisement, lest either the cause already suspected, or some other unheeded thing, may in some cases make these travelling baroscopes somewhat differing from others. But though they should prove to be so, yet it would not follow that they cannot be made serviceable; for keeping a pretty while that instrument which suggested the scruple to me, just by the other with which I had compared it, and carefully taking notice of the respective heights at which the mercury rested in both, I observed, that when it rose or fell in the other barometer, it did also rise and fall in the portable one; and when it rested at its first station in the former, it did so in the latter; and though there seemed to be an inequality in the quantity of the ascent, and subsidence of the mercury in the two instruments, yet that seemed to be accountable

countable for by some circumstances, especially the very unequal breadth of the vessel that contained the stagnant mercury in the other barometer, and that shorter leg which answered to that vessel in the travelling barometer. But till the formerly proposed scruple be by further observations removed, the safest way will be to make the barometer to be sent to remote places as like as may be (in bigness and length of the tube) to another portable one kept at home, that so when they are once adjusted, the collations may be made betwixt two instruments of the same kind, whereof that which is kept at home may also, if it be thought fit, be compared, when the observations are made, with a baroscope made the ordinary way.

### EXPERIMENT XXIII.

*Confirming that mercury in a barometer will be kept suspended lower at the top than at the bottom of a hill. On which occasion something is noted about the height of mountains, especially the pic of Teneriff.*

To give your lordship some instance (till I can present you with a noble one) of the use of our travelling barometer, I shall now add, that when I writ the foregoing experiment, chancing to be within two or three miles of a hill, which, though not high, was the least low in that country, I thought our instrument might be safely, and not altogether uselessly, carried on horseback to the top of it, which was too remote from the bottom to be conveniently reached by me on foot in the midst of winter. This trial therefore I resolved to make, because though I formerly told you of a considerable one that had been made in *France* by some eminent virtuoso of that country, yet I was willing not only to have a proof, how safely our baroscope might be transported, but to confirm to your lordship upon our own observation made in another region, so considerable an argument as these kind of experiments afford to our hypothesis.

And though when I came to try the experiment I happened to have an indisposition that forbid me to do it all myself, yet, having carefully marked on the edge of the frame the height to which the suspended quicksilver reached, and compared it with a good baroscope made the ordinary way, I committed our instrument to a couple of servants that I had often employed about pneumatical and mercurial experiments, giving them particular instructions what to do. And the instrument being such as might be safely carried on horseback, I had in two or three hours an account brought me back, the sum of which was, that they found the suspended mercury fall a little as they ascended the hill, at whose top they gave the liquor leave to settle, and carefully took notice, by a mark, of the place it rested at; which was, as I afterwards found,  $\frac{1}{4}$  of an inch, or somewhat better, beneath the mark I had made; and this notwithstanding the hill was not high, and the air and wind seemed to them to be much colder at the top of it than beneath. But though as they descended more and more, they observed the mercury to rise again higher and higher (as being pressed against by a taller column of the atmosphere) and though consequently the experiment agreed very well with our hypothesis, and may serve for a confirmation of it; yet by reason of the small height of the mountain, the decrement of the height of the mercurial cylinder was not so considerable, but that I should perhaps have omitted the mention of this trial, if it did not shew that our travelling baroscopes may be fit to be employed about such experiments; and therefore, when I can recover some of my scattered papers, I shall by way of appendix



pencil, subjoin to this some other observations that I procured to be made by ingenious men who had the opportunity of living near higher mountains.

SOME further trials I have recommended to be hereafter made by some other inquisitive persons; and to make them the more instructive, I could wish that others would do what I should have done, if opportunity had befriended me; for I designed to make the experiment at the bottom, the top, and the intermediate part of the whole, at three differing constitutions of air, when it should appear by a good ordinary baroscope, that the atmosphere was very heavy, when it should be found to be very light, and when it should have a moderate degree of gravity: and I hoped that if sagacious experiments should make these diversified observations on distant and unequal hills, good hints may result from the collations that may be made of the varying decrements of the mercurial cylinder's height, according to the differing gravities of the atmosphere at several times, and the differing heights of the hills and stations where the observations should be made.

I ALSO endeavoured to get a baroscope carried down to the bottoms of deep mines; partly to try whether the atmospherical pillar being longer there than at the top, the mercury in the tube would not be impelled up higher; and partly in order to other discoveries. But some impediments in the structure of those mines made it not very practicable to employ barometers there; which yet makes me not despair of success in some other mines, where the shafts or pits are sunk more perpendicularly.

PERHAPS I told your lordship already by word of mouth, that I have been solicitously endeavouring to get the Torricellian experiment tried upon the pic of *Teneriff*; but hitherto I have had no account of the success of my endeavours; for which I am the more concerned, because of the eminent (if not matchless) height of that mountain, of which you may receive some satisfaction by what I am going to subjoin about it.

### *An Appendix about the height of Mountains.*

FORASMUCH as on the one hand not only *Kepler*, but divers other modern writers of note, do endeavour to straiten the atmosphere, and make it lower by half than the least height to which, according to our estimation, it should reach; and to countenance their opinion, will not allow the clouds to be often above a mile high, nor even the highest mountains to exceed two miles. And forasmuch as on the other side other learned men seem to make the clouds and the mountains of a stupendous height; we, who take a middle way of estimating the height of the one and the other, hold it not unfit to subjoin on this occasion some uncommon observations in favour of our opinion, that we have obtained from inquisitive travellers.

BUT first I will subjoin a passage I have somewhere met with in *Ricciolus's Almigestum novum*, where he, if I well remember, relates that the rector *Metensis*, as he calls him, of the Jesuits college, affirmed to him some years since that he had measured the height of many clouds, without having found any of them higher than 5000 paces; which argues, that he met with some so high, though indeed the height of clouds must needs be very various, according to the gravity or lightness, density or thinness, rest or agitation of the air, and the condition of the vapours and exhalations they consist of. And if either that be true which we have formerly had occasion to mention concerning *Maignan's* observation; or if it be true that sublunary comets (for I speak not of celestial ones) are generated of exhalations of the terrestrial globe, we may well conjecture

ture that the atmosphere (especially if its height be not uniform) and even clouds, especially those that have most fumes and fewest vapours, may reach much higher than *Cardan*, *Kepler*, and others have defined.

BUT of the height of clouds, which we have sometimes attempted to take geometrically, we may have elsewhere occasion to speak again; and therefore I shall now proceed to what I have to say concerning the height of mountains; which being an enquiry curious and difficult enough in itself, and of some importance in the disquisition about the height of the atmosphere, it being evident that that must reach at least as high as the tops of mountains, upon whose tops men can live; I hope it will not be unacceptable to your lordship, if having a while since, as I was intimating, had the opportunity to discourse with some credible persons that have been upon the top of exceeding high mountains, particularly of the pic of *Teneriff*, and especially with one gentleman, who was a few days before brought to satisfy the curiosity of our inquisitive and discerning monarch, by giving him an account of his journey, I acquaint you with those of the particulars which I learned from thence, that are the most pertinent to our present purpose. First then, whereas divers late mathematicians will not allow above two miles or half a *German* league, and some of them not half so much, to the height of the highest mountain; the mountain we speak of, in the island of *Teneriff*, one of the *Canaries* or *Fortunate Islands*, is so high, that though perhaps I think those travellers I have taken notice of speak with the most when they write, that the top of this mountain is to be seen at sea, four degrees off, i. e. at least threescore *German* leagues; yet having asked the ingenious gentleman lately mentioned, *Mr. Sydenham*, from what distance the top of the sugar-loaf, or highest part of the hill, so called from its figure, could be seen at sea, according to the common opinion of seamen, he answered, that that distance was wont to be reckoned 60 sea-leagues, of three miles to a league; adding, that he himself had seen it about 40 leagues off, and yet it appeared exceeding high, and like a bluish pyramid, manifestly a great deal higher than the clouds. And what he related to me about the distance was afterwards confirmed by the answers I received from observing men of differing nations, who had sailed that way; and particularly by a noble virtuoso, skilled in the mathematicks, who was then admiral of a brave English fleet. And the abovementioned gentleman *Mr. S.* also told me, that sometimes men could from thence see the island of *Madeira*, though distant from it 70 leagues; and that the great *Canary*, though 18 leagues off, seemed to be very near them that were on the top of the sugar-loaf, as if they might leap down upon it. Thus far *Mr. Sydenham*, by whose relation it appears, that this pic must be far higher than *Kepler* and others allow mountains to be; for else it could not be seen at sea from so great a distance. And the learned *Ricciolus* supposing it to be (as some navigators report it to be) discoverable at sea four degrees off, calculates its height measured by a perpendicular line, and allowing too for refraction, to amount to ten miles, which altitude also the accurate *Snellius* assigns it. But I fear this learned man may have been somewhat misinformed by the navigators he relies on, or else that the way of allowing for refractions is not yet reduced to a sufficient certainty; for I do not find by those who have purposely gone to the top of it, that the mountain is so high as his calculation makes it. And whereas the same eminent writer resolutely pronounces that the height of mount *Caucasus*, deduction being made for refraction, is 51 *Bolognian* miles, which are considerably greater than the *Roman* miles; I doubt not here likewise, though I question not his supputations, if you grant him the grounds of them, he makes this mountain far higher than indeed it is. For the passage of *Aristotle*, on which he founds his opinion, is obscure enough; and *Aristotle* himself does sometimes take up reports upon hear-say, without



over strictly examining their truth or probability; whereas all the navigators and travellers I have hitherto met with (and your lordship knows that I have, upon a publick account, the opportunity of meeting often with such men) do almost unanimously agree that the pic of *Teneriff* is the highest mountain hitherto known in the world; and yet that is so far from being 15 leagues high, as some eminent and even late writers would persuade us, that it is scarce a seventh part so high as *Ricciolus* computes mount *Caucasus* to be. For having asked Mr. *Sydenham* and others what was the estimate made by the most knowing persons of the island of the height of the hill, he told me that his guides accounted it to be one-and-twenty miles high from the town called *L'oretava*, seated on the lower part of the hill, from which town to the sea there is three miles of way always descending. But in regard that the way which amounted to 21 miles in length is, as other ways whereby steep places are wont to be ascended, made to wind and turn for the conveniency of travellers; I can scarce deduct less than two thirds for the crookedness of the way; and accordingly having asked him, whether the perpendicular height of it had been accurately taken by any with mathematical instruments, he answered, that he could say nothing to that upon his own knowledge, but that a seaman with great confidence affirmed himself to have accurately enough measured it by observations made in a ship, and to have found the perpendicular height of the hill to be about seven miles. Which estimate agrees well enough with the calculations of *Ricciolus* and *Snellius*, if we lessen the distance from which the top of the hill is to be discovered, from 60 German leagues of four miles to a league, to the like number of common leagues at three miles to a league.

AND because eminent writers have so confidently delivered prodigious things touching the height of this mountain, I will here, to confirm the estimate already made, add these particulars, which I took from the gentleman's own mouth, and which were afterwards confirmed to me by another that went with him, and partly also by a third, who went up to the top at another time of the year, viz. that they begun their journey from *L'oretava* on the 18th of *August*, about 10 of the clock at night, and travelled till five in the afternoon on the *Monday* following, resting two hours by the way, and travelling about 10 miles of the way upon mules, which afterwards they were forced to leave, and betake themselves to their feet. Resting upon *Monday* till midnight, they resumed their journeying, and travelled until about nine the next morning, at which time they arrived at the top of the Sugar-loaf, or highest pile of the mountain; so that they travelled in all but 26 hours, in which, considering the steepness and ruggedness of the ways, and that they were forced to go above half way on foot, to which they were unaccustomed, it is likely enough that the length of the way did not much, if at all, exceed the computation of the guides.

WE have since endeavoured, but without yet knowing what will be the success, to have the height of this mountain carefully taken by skilful men; in the interim I shall not deny, but that if what *Aristotle* and other authors report of mount *Caucasus* be true, there may be far higher mountains than the pic of *Teneriff*; especially since there is one consideration, which perhaps you will not think despicable, that I find not taken notice of by those that have written of the height of mountains, viz. that of two mountains, that measured by geometrical instruments may appear to be of the same height, there may yet be a great inequality; because the measurer measures only from some plain piece of ground at the bottom of the hill to the top, whereas it may be, that the country wherein one of those mountains stands, may be exceedingly much higher than that wherein the other is placed; which difference of heights in the several countries he that is to measure only the height of one of the mountains, is not wont to take any

The like consideration I since found to have been had, before me, by the learned *Ricciolus*.

notice of; and consequently, though in respect of the plains adjacent to the feet of the mountains, their altitudes may be equal yet in respect of the level or superficies of the terraqueous globe, considered as having no mountains at all but those two, the height of the one may far exceed that of the other; and so the pic of *Teneriff* being looked upon from the level of the sea, may be much less high than some other hills, but may appear much higher than some other hills, which yet protuberating above the level part of some country, which is itself generally exceeding high, may have its top more remote from the centre of the earth than that of the pic, and would appear higher than it, if as well the one as the other were looked upon from the same superficies of the sea.

BUT to return to the height of the atmosphere; in order to the making an estimate of what we have considered as to the height of mountains, I shall add, that though by what has been already said touching the height of the pic and other hills, it appears that the atmosphere reaches far higher than many learned men would hitherto allow; yet we are not to think that the atmosphere may not reach almost incomparably higher than the tops of mountains: nor do I suffer myself to be concluded by what many commentators of *Aristotle* and other writers are wont to teach, touching the distinct narrow extent they allow to that sphere, within whose limits they would have the steams of the terrestrial globe to produce meteors. How far the height of mountains may make the air at the tops of them inconvenient for respiration, shall be (God permitting) considered, when I come to acquaint your lordship with my loose trials about respiration.

#### EXPERIMENT XXIV.

*Shewing that the pressure of the atmosphere may be exercised enough to keep up the mercury in the Torricellian experiment, though the air press upon it at a very small orifice.*

By a very slight variation of the foregoing 22d experiment we may both confirm one of the most important, and the least likely truths of the hydrostaticks, and remove an objection, which, for want of the knowledge of this truth, is wont to be urged against our hypothesis even by learned men. For divers of these, when they see the same phenomena happen in the Torricellian experiment, whether it be made in the open air or in a chamber, are forward to object, that if it were, as we say it is, the weight of the air incumbent on the stagnant mercury, which keeps that suspended in the tube from falling down, the mercury would not be sustained at any thing near the same height in the open air, where the pillar that is supposed to lean upon the stagnant mercury, may reach up to the top of the atmosphere, as in a close room, where they imagine that no more air can press upon it than what reaches directly up to the roof or ceiling. And when to this it is answered, that, though, if a room were indeed exactly closed, the sustentation of the mercury ought to be ascribed to some other cause than the weight of the imprisoned air, which other cause I have elsewhere shewn to be its spring, yet in ordinary rooms there is still a communication between the internal and external air, either by the chimney, or, if the room have none, by some crevice in the window, or by some chink between the wall and the door, or at least by the key-hole. And when to this it is objected, that the orifice of the key-hole is much narrower than the superficies of the stagnant mercury, and consequently, though the atmosphere were not reduced to press obliquely on the mercury, yet, entering at so small an orifice, it could not press sufficiently upon it; when I say, in answer to this objection I have alledged that hydro-  
statical



statical theorem, that the pressure, in such cases as ours, is to be estimated by the heights of the liquors, and not the breadths, the assertion has been thought unlikely and precarious.

To confirm, therefore, this hydrostatical truth, one may take the bended tube mentioned in the 22d experiment, and inclining it till the greatest part of the mercury pass from the shorter leg into the longer, the upper end of this shorter leg may by the flame of a lamp be drawn out so slender, that the orifice of it shall not be above an eighth or tenth part (not to say a much less) as big as it was before. For this being done, and the tube erected again, if the tall cylinder of mercury be of the usual or former height, as we have found it, it will appear congruous to our hypothesis that the weight of the external air may exercise as much pressure upon the stagnant mercury through a little hole, as when all the upper superficies of that mercury was directly exposed to it.

And if one have not the conveniency to draw out the shorter leg, as is prescribed, one may nevertheless make the trial, by carefully stopping up the orifice with a cork and cement, leaving only, or afterwards making a very small hole for the air to pass in and out. If I had not wanted a fit instrument, I would have tried to exemplify the truth of what has been delivered, by adding to the glasses we employed to make the fifth experiment such a cover as might be cemented on to the edge of the glass, having only a very small hole in the midst, at which the atmosphere would be reduced to exercise its pressure; and the like cover I would have made use of in the tenth experiment, about the breaking of glass-plates in the unexhausted receiver, by the bare spring of the air.

## EXPERIMENT XXV.

*Shewing that an oblique pressure of the atmosphere may suffice to keep up the mercury at the wanted height in the Torricellian experiment, and that the spring of a little included air may do the same.*

By adding a couple of little circumstances to the trials lately proposed, we may confirm two considerable articles of our hypothesis. For, 1. if, instead of drawing the shorter leg of our barometrical siphon, if I may so call it, directly upwards or parallel to the longer leg, as in the foregoing experiment, you make the slender part bend off so, as that, if it were continued, it would make a right angle with the longer leg of the siphon, or else an acute angle, tending downwards; this being done, I say, if when the tube is erected the mercury rest at its wonted station, it will appear that the pressure of the atmosphere may be exercised upon it as well obliquely, when the pipe that conveys it is either horizontal or opens downwards. See Plate V. Fig 3.

AND, 2. if, instead of bending this slender pipe, one seal it up hermetically, the continuance of the mercurial cylinder at the same height will shew, that the spring of a very little air, shut up with the pressure of the atmosphere upon it (though no more than what the air here below is ordinarily exposed to by the weight of the incumbent air) is able to support as tall a cylinder of mercury as the weight of the whole atmosphere, i. e. of as much of it as can come to exercise its pressure against the mercury. See Plate V. Fig 4.

N. B. If when the shorter leg of the baroscope is sealed up, you move the instrument up and down, the mercury will vibrate, by reason of the somewhat yielding spring of the imprisoned air; but because of the resistance of the spring the motion will be diversified after an odd and pretty manner; which may be easily perceived by the impression

it makes upon the hand, but not so easily defcrib'd. And becaufe that, when the ſhorter leg is drawn out ſlender enough, after the inſtrument is furniſhed with quickſilver, it is eaſy to ſeal it up with the flame of a candle, without the help of any inſtrument at all, I ſhall here take notice to your lordſhip (which I could not reaſonably do before) that it may on ſome occaſions be convenient to ſeal up the barometer before it be tranſported, and, in ſome caſes, to incline the tube beforehand till the quickſilver have quite fill'd the longer leg; by this means the vibrations of the quickſilver will be leſs than otherwiſe they would be, and it will be no trouble at all, when the inſtrument is brought to the deſign'd place, to break off the ſlender apex of the ſhorter leg, and ſo expoſe again the mercury to the preſſure of the atmophere.

As about the former experiments, ſo about theſe two this advertiſement may be given, viz. That the ſame trials, for the main, may be made without conſining one's ſelf to the propoſed ways of making them.

1. For the firſt of theſe new trials may be made by cementing very carefully on to the orifice of the ſhorter leg (which need not be alter'd) a ſhort pipe of glaſs, whoſe upper end may be drawn out very ſlender, and bent either horizontally or downwards, which is far eaſier to be done than to draw out the ſhorter leg, when the glaſs is furniſhed with mercury.

2. AND as for the ſecond trial, that may be well enough made by carefully ſtopping the unalter'd orifice of the ſhorter leg with a good cork, and our cloſe cement, or with the latter only; and when you would afterwards uſe this inſtrument as a baroſcope, you need but heat a pin or ſlender wire red hot, and ſo burn a hole through the ſtoppel.

AND this expedient, which I could not conveniently advertiſe your lordſhip of ſooner, may be of uſe, when a travelling baroſcope is to be often removed; becauſe having once ſtopped the whole orifice well, it is far more eaſy to ſtop and open a pin-hole accurately, than to cloſe and unſtop the whole orifice of the tube.

NOTE, I endeavour'd to confirm more than one of the foregoing particulars by this one experiment. Having cauſed a portable barometer to be made with the ſhorter leg of a ſomewhat more than ordinary length, I afterwards cauſed the upper part of this leg to be drawn out very ſlender, as in this 25th experiment; and laſtly, I cauſed the ſame ſhorter leg to be either about or ſomewhat above the middle bended downwards, ſo that the ſmall orifice of the ſlender apex pointed towards the ground. This done, I was to have meaſured the height of the ſuſpended mercury, but not having a fit ruler at hand, I then deſerred, and afterwards forgot to do it; but I remember, that neither I, nor ſome others verſed in ſuch experiments, to whom I ſhew'd it, took any notice, that the mercury was leſs high than in ordinary barometers; whence it was concluded, that the atmophere could exerciſe his preſſure not only at a very ſmall orifice, which in our experiment did little, if at all exceed a pin-hole, but when the air muſt at this little orifice preſs upwards to be able to preſs upon the ſurface of the ſtagnant mercury.

## EXPERIMENT XXVI.

*About the making of a baroſcope (but of little practical uſe) that ſerves but at certain times.*

To ſhew ſome ingenious men by a medium that has not hitherto (that I know of) been made uſe of, that the not ſubſiding of quickſilver in an inverted tube, that is a  
little



little shorter than thirty inches or thereabouts, does not proceed from such a *fuga vacui* as the schools ascribe to nature, but from the gravity of the external air, I devised the following experiment.

HAVING made choice of a time, when it appeared by a good baroscope, which I had frequently consulted for that purpose, that the atmosphere was considerably heavy, I caused a glass-pipe, hermetically sealed at one end, and in length about two feet and a half, to be filled with quicksilver, save a very little, wherein some drops of water were put, that we might the better discern the bubbles, if any should be left after the inversion of the tube into an open glass with stagnant mercury in it. Having by this means, though not without difficulty, freed the tube from bubbles, we so ordered the matter, that the quicksilver and the little water that was about it filled the tube exactly, without leaving any interval, that we could discern at the top, and yet the mercurial cylinder was but very little higher than that of our baroscope was at that time.

This done, the newly filled pipe was left erected in a quiet place, where the liquors retained their former height for divers days. But though an ordinary school-philosopher would confidently have attributed this sustentation of so heavy a body to nature's fear of admitting a vacuum; yet it seems, that either she is not always equally subject to that fear, or some other cause of the phenomenon must be assigned; for when (a pretty while after) I had observed by the baroscope, that the atmosphere was grown much lighter than before, repairing to my short tube, I found, that according to my expectation the quicksilver was not inconsiderably subsided, and had left a cavity at the top, which afterwards grew lesser according as the atmosphere grew heavier.

N. B. 1. THE tube employed about this experiment may be brought to the requisite shortness, either by wearing off a little of the glass at the orifice of it, or by increasing the height of the stagnant mercury into which it hath been inverted.

2. WHEN the quicksilver in our short tube was much subsided, there appeared in the water that swam upon it a little bubble, about the bigness of a small pin's head; but, considering how careful we had been to free the tube from bubbles before we set it to rest, it may very well be, that this so small a bubble was not produced till after the subsiding of the quicksilver, whenupon the aerial particles in the water became less compressed than before; not to mention that the bubble (such as it was) appeared very much greater than it would have done, if the pressure of the atmosphere had not been kept from it by the weight of the subjacent pillar of mercury.

## EXPERIMENT XXVII.

*About the ascension of liquors in very slender pipes in an exhausted receiver.*

WHAT I related to your lordship, in the 35th of the published experiments, about the seemingly spontaneous ascension of water in slender pipes, has occasioned the making of many trials by the curious, whereby that experiment has been not a little diversified. But because among those I have yet heard of, none have been made in our engine, it may not be amiss to add the following trial, which may be of use in the examen of one or two of the chief conjectures that have hitherto been proposed about the cause of that odd phenomenon.

WE tinged some spirit of wine with cochineal, which being put into the receiver and the air withdrawn, did exceedingly bubble for a pretty while; then little hollow pipes  
of.

of different sizes being put into it, the red liquor ascended higher in the slenderer than the others; but upon the withdrawing of the air there scarce appeared any sensible difference in the heights of the liquor, nor yet upon the letting it in again.

AFTERWARDS two such pipes of differing sizes being fastened together (at a distance) with cement, were let down into the same spirit of wine, when the receiver was well exhausted, notwithstanding which the liquor ascended in them, for aught we could plainly see, after the ordinary manner; only when the air was let in again, there seemed to be some little (and but very little) rising, at least, in one of the pipes. In this trial, this phenomenon was noted, that though there appeared no bubbles at all in the vesseled spirit of wine (notwithstanding that we continued to pump) yet there did for a pretty while arise bubbles in that part of the liquor that was got into the slender pipes; which I guessed to proceed from the sustentation (in part) of the spirit of wine, made by the inside of the pipe whereto it adhered.

### EXPERIMENT XXVIII.

*About the great and seemingly spontaneous ascension of water in a pipe filled with a compact body, whose particles are thought incapable of imbibing it.*

UPON occasion of the (seemingly) spontaneous ascension of water in slender pipes of glass, I considered, that it would be easy by another way to make it rise to a far greater height than hitherto had been done; for since we had found by observation, that, *ceteris paribus*, the slenderer the little pipes were that we employed, the higher the liquor would rise in them; and since the hydrostaticks had taught us, that oftentimes, even in very crooked pipes, water would be made to ascend by the same ways (of raising it) to the same perpendicular height (or thereabouts) as in straight ones; I thought, that I might well substitute a powder, consisting of solid corpuscles heaped upon one another and included in a glass-cane, instead of the little pipes I had hitherto used. For I considered the little intervals that would necessarily be left between these differently shaped and confusedly placed corpuscles, would allow passage to the water, as did the cavities of the little pipes, and yet would in many places be straighter than the slenderest pipes I had used: and though beaten glass, or fine sand, &c. might have been employed about this experiment, yet I judged it far more convenient to make use of some metalline calx, because the operation of the fire making a more exquisite comminution of solid bodies than our pestles are wont to do, is fit to supply us with exceeding minute grains, that intercept proportionable cavities between them.

UPON this consideration, therefore (besides others to be hereafter hinted) I took a straight pipe of glass open at both ends, and of a moderate wideness (for it need not be very slender) and having tied a linen-rag to one end of it, that the water might have free passage in, and the powder not be able to fall out, we carefully, and as exactly as we could, filled the cavity with minium (which is lead calcined, without addition to redness); and then having erected the tube, so that the bottom of it rested upon that of a somewhat shallow and open-mouthed glass, containing water enough to swim an

This was  
(if I forget  
not) about  
the latter  
end of the  
year 1662.

inch or two above the bottom of the tube, into whose cavity it did, as I expected, insinuate itself by degrees, as appeared by a little change of colour in that part of the minium which it reached, till (the open glass being from time to time supplied with fresh liquor) it attained to the height of about 30 inches; and then, our Society expressing



pressing a curiosity to see it and have it placed among better things, I was hindered from making any further observations with that particular glass.

WHEREFORE, taking afterwards another tube and some minium carefully prepared, I prosecuted the experiment, so as to make the water rise in the pipe about 40 inches above the surface of the stagnant water. I guessed it had risen higher, but by reason that, at the upper part of the minium, the difference of colour was so small, as not to be easily distinguishable with certainty, I forbore to allow a greater height to the ascension of the water; nor could I, where I then was, much promote the experiment, for want of such accommodations as I desired; but about the experiment, as I tried it, I shall take notice of the following particulars.

I TRIED some other powders besides red lead (as beaten glass, pieces of fine sponge, putty, &c.) but did not find any of them do so well; which success was yet perhaps but accidental, and therefore the trial may be repeated, especially with putty, because that being a metalline calx as well as minium, consists of very small grains, and, by reason of its great whiteness, receives a greater change of colour by wetting than minium does; in which, especially if it be very fine, the discoloration that water makes towards the upper part of the tube, is sometimes not so easy to be clearly discerned.

2. I DID indeed endeavour to remedy this inconvenience by using, instead of mere water, tinted liquors, as ink, tincture of saffron, &c. but they seemed not to rise near so high as water alone, as if the dissolved ingredients did by degrees choke the pores of the minium.

3. To have the grains of our powder more minute, and the smaller intervals between them, I chose not only to use the finest sort of minium I could procure, but also to sift it through a very fine sieve, and to put it but by little and little into the tube, that by ramming it from time to time it might be made to lie the closer; which expedients succeeded not ill.

4. It seemed by a trial or two (for I am not sure the observation will always hold) that if the tube were very slender (as about the bigness of a swan's quill) the experiment succeeded not well.

5. It may be worth while to observe in what times the water ascends to such and such heights; for at the beginning it will ascend much faster than afterwards, and sometimes it will continue rising 24 or 30 hours, and sometimes perhaps much longer.

6. ONE of the scopes I proposed to myself in this experiment was to discover a mistake in the explication, that some learned modern writers have given us of the cause of filtration; for, whereas they teach, that the parts of filter that touch the water, being swelled by the ingress of it to their pores, are thereby made to lift up the water till it touch the superior parts of the filter that are almost contiguous to them; by which means, these being also wetted and swelled, raise the water to the other neighbouring parts of the filter till it have reached to the top of it, whence its own gravity will make it descend: but in our case we have a filter made of solid metalline corpuscles, where it will be very hard to shew that any such intumescence is produced as the recited explication requires.

7. WATER ascends so few inches even in very slender pipes, as to seem much to favour their judgment, who disallow the conjecture lately entertained by some ingenious men (particularly Mr. H.) about the raising of the sap in trees, after the like manner that water is raised in slender pipes. But without fully delivering yet my thoughts of that speculation, I may take notice, that in the last trial above recited, I made water to ascend near if not above 3 feet  $\frac{1}{2}$ ; and if, by so slight an expedient, water may be made to rise as high as is necessary for the nutrition of some thousand of plants (for such a

number there is, that exceed not 3 feet  $\frac{1}{2}$  in height) one may without absurdity ask, Why it is not possible that nature, or rather the most wise author of it, may have made such contrivances in plants, as to make liquors ascend in them to the tops of the tallest trees; especially since, besides divers things that we may already suspect (as heat, and something equivalent to well placed valves) many others that perhaps are not yet dreamt of, may probably concur to the effect.

8. As I formerly made, by bending the slender pipes we have been talking of, short siphons, through which the water runs, without being at first assisted by suction, so I thought fit to try, whether I could not in larger pipes, by the help of minium, make much longer siphons. But though when the orifices were turned upwards, fine minium were rammed into both the legs, and the orifices were both of them closed; yet when they came to be again turned downwards the weight of the minium would somewhere or other (and, for the most part, at or near the flexure) make some such chink or discontinuation, as to hinder the farther progress of the water. Which impediment, though I judged it superable enough (especially by making at the flexure a little pipe or socket, by which both legs might be closely filled) yet for want of accommodations and leisure it was left unfurmounted: upon which account also, I did not satisfy myself about the success of some former trials, as of the ascension of water into pieces of wood of differing sorts, the operation of the vicissitudes of the sun's beams, and the absence of them, upon liquors ascending in tubes filled with minium, &c.

9. WHETHER the pressure of the outward air be the cause of the ascension of liquors in our tubes furnished with minium is a problem, in order to whose solution I could acquaint your lordship with a contrivance wherewith to make some trials in our engine. But since it can scarce be well described without many words, unless you express a particular curiosity to know it, I shall not trouble you with it; and the rather, because the best way I know of examining this difficulty belongs to the second part of this Continuation, where mention is made of an attempt about it, which did not, I confess, dispense me.

## EXPERIMENT XXIX.

*Of the seemingly spontaneous ascension of salts along the sides of glasses, with a conjecture at the cause of it.*

To the same cause, or the like, with that of the ascension of water in slender pipes, may be probably referred an odd phenomenon, which, though I remember not to have been mentioned by any chymical or other writer, I have not unfrequently observed, as well by chance as in trials purposely made to satisfy myself, and others, about the truth of it.

THE phenomenon, in short, was this: that having, in wide-mouthed glasses (which should not be very deep) exposed to the air a strong solution of common sea-salt, or of vitriol, which reached not, by some inches, to the top of the glass; and having suffered much of the aqueous part to exhale away very slowly, the coagulated salt would, at length, appear to have lined the inside of the glass, and to have ascended much higher, not only than the place where the surface of the remaining water then rested at, but than the place to which the liquor reached when it was first poured in; and if the experiment were continued long enough, I sometimes observed this ascension of the salt to amount to some inches, and that the salt did not only line the inside of the glass, but, getting  
over



over the brim of it, covered the outside of it with a saline crust; which made them that saw how little liquor remained in the glass, admire how it could possibly get thither.

AND though I have mentioned but the solution of vitriol and sea-salt, because they are much easier than others to be procured, and yet the experiment succeeds better in them than in some other far less parable salts; yet they are not the only ones, by whose solutions the recited phenomenon may be exhibited.

As for the cause of this odd effect, though I shall not propose any thing about it with confidence till I have further inquired into it, and especially till I have tried whether the phenomenon may be produced in an exhausted receiver; yet, by what I have hitherto observed, I am inclined to conjecture that it may be referred to such a cause as that of the ascension of liquors in pipes, after some such manner as this.

FIRST, I observed, that in water and aqueous liquors, that part of the surface which is next the sides of the glass, is (whatever the reason of it be) sensibly more elevated than the rest of the superficies; and, if very little clippings of straw or other such minute and light bodies, floating upon the water, chance to approach near enough to the sides of the glass, they will be apt (which one would not expect) to run up, as it were, this ascent of water, and rest against the sides of the glass.

NEXT we may take notice, with the salt-boilers and chymists, that sea-salt is usually wont to coagulate at the top of the water in small and oblong corpuscles, so that as to these, it is easy to conceive to them that have considered the first observation, how numbers of them may fasten themselves round about to the inside of the glass. And besides sea-salt, I have found by trial divers others, if their solutions be slowly enough evaporated, that will, whilst yet there remains a good proportion of liquor, afford saline concretions at the top of the water; and the fastening of saline particles to the sides of the glass may perhaps be promoted by the coldness that may be communicated to the corpuscles contiguous to the glass, by reason of the coldness which the glass may be supposed to have, upon the score of its density, in comparison of water. But to proceed: I consider, that by the evaporation of the aqueous parts of the solution, the surface of the remaining liquor must necessarily subside, and those saline particles that were contiguous to the inside of the glass and the more elevated part of the water, having no longer enough of liquor to keep them dissolved, will be apt to remain sticking to the sides of the glass, and upon the least farther evaporation of the water will be a little higher than the greater part of the superficies of that liquor; by which means it will come to pass, that, by reason of the little inequalities that will be on the internal surface of the adhering corpuscles of the salt, and perhaps also on the internal superficies of the glass, there will be intercepted between the salt and the glass little cavities, into which the water contiguous to the bottom will ascend or be impelled, upon such an account as that whereon it is raised in slender pipes. And when the liquor is thus got to the top of the salt, and comes to be exposed to the air, the saline part may, by the evaporation of the aqueous, be brought to coagulate there, and consequently to increase the height of the saline film, if I may so call it; which, by the like means, may be at length brought to reach to the very top of the glass, whence it may easily be brought over to the outside of the vessel, where the natural weight of the solution will facilitate its progress downwards; and the skin of salt, together with the contiguous surface of the glass, may at length constitute a kind of siphon.

To this explication it agrees well, that I have usually observed the saline film hitherto mentioned to be with great ease separable from the glass in large fleaks; which argues

that they did not stick close to one another, except in some few places, but had a thin cavity intercepted between them through which the water might ascend:

NOR is it repugnant to this explication, that in case the water ascended, it should, as it seems, dissolve the salt; for the liquor being already upon the point of concretion, is so glutted with salt that it can dissolve no more. Whence we may also render a reason, why, when the saline film chanceth to reach to the outside of the glass, the liquor divers times does not run down to the bottom, but is coagulated by the way: and I have also had a suspicion (though I could not seasonably take notice of it before now) that when the concretion is once begun, the film may be raised and propagated, not only by the motion of the liquor between the inside of it and the glass, but by the same liquor's insinuating itself on the outside of the film into the small chinks and crevices intercepted between the saline corpuscles, as ink (especially if somewhat thin) rises into the slit, and along the sides of the nib of a pen, though nothing but its very point be dipt in the surface of the liquor; and by this means the impregnated solution may, as it were, climb up to the top of the saline concretion, and by coagulating there add to its height.

SOME other circumstances I have noted of our phenomenon that agree with the proposed explication; but perhaps it would not be worth while to spend more time about it. Not to examine here, whether what has been related, so as to make it probable, that ascending water may carry up wherewithal to heighten and increase the pipes or vessels through which it rises, may contribute any thing more than was suggested in the former 28th experiment, towards the explication of the rising and diffusing of the sap in trees.

### EXPERIMENT XXX.

*About an attempt to measure the gravity of cylinders of the atmosphere, so as that it may be expressed by known and common weights.*

WHILST I was making the former experiments, it was more than once my wish, that by knowing the just weight of a cylinder of quicksilver of a determinate diameter, and of 29 or 30 inches high, which is near the height that the air does usually counterbalance, I might the better estimate the weight of a cylinder of the atmosphere of that diameter, and consequently make the better guesses how near the effects of the spring of the air, as well as of its weight, produced by the help of our engine, approached to the utmost of what might have been expected, in case all the instruments employed had been perfect, and all concurrent circumstances had been favourable. And upon this account, I several times regretted my want of a long instrument of steel or hardened iron, wherewith I many years since made an observation that was more carefully registered than preserved, of the weight of a mercurial cylinder of a determinate height, as well as diameter; which weight I did not think so safe to determine by the help of glass tubes, because it is very difficult to have them uniformly cylindrical, and to know that they are so, in regard that they are formed but by blowing and drawing out; and, besides the inequality that may happen to the cavity upon other accounts, it is very difficult to make the sides of the glass equally thick, and to examine whether they be so or no.

BUT at length lighting upon (what I had too often wanted in the foregoing experiments) a dexterous artificer that chanced to come for a while to the place where I then  
was.



was, I endeavoured to repair my loss, as well as he could help me to do it, by causing him to turn very carefully a cylindrical piece of brass of an inch in diameter, and three inches in length, and open (that it might be the better wrought) at both ends, to one of which was exactly fitted a flat bottom of the same metal fastened very close to it with little screws on the outside, this being judged a better way than if it had been turned all of a piece.

THIS instrument being diligently counterpoised in a trusty pair of scales, was carefully filled with mercury which (for greater caution) we took out of a new parcel, that we had not yet employed about other experiments, and finding it to weigh 17 ounces, 1 drachm, 45 grains, Troy weight (or 137 drachms 45 grains) multiplying that by 10, there will come for the weight of a mercurial cylinder of one inch in diameter and 30 inches in height (and so high I have divers times seen the mercury to be in a good barometer) about 14.2 lb. (i. e. 14 lb. 2 ounces, and above three drachms, Troy weight); and almost 11.8 lb. Avoirdupoise weight (i. e. 11 lb. 12 ounces, and above 6 drachms) which is a greater weight than, without such a trial, one would easily imagine, that so short a cylinder of mercury, and much less that a cylinder of so light a body as air, being neither of them above an inch diameter, could amount to.

NOTE first, to examine at the same time the weight of the mercury, and its proportion to water, we did, before the mercury was poured into the brass-veffel, fill it with water (after which we wiped it dry before the mercury was put into it) and this liquor weighing 10 drachms and 15 grains, the proportion between the mercury and the water appeared to be that of  $13\frac{3}{4}$  to 1; which, though it seem somewhat of the least, yet your lordship may remember that I formerly told you I had several times found the received proportion of 14 to 1, between mercury and water, to be somewhat too great; and besides that, in a vessel whose orifice was no less than an inch in diameter, it is exceeding difficult to be sure when it is precisely full, either of water or mercury, because the former has a superficies considerably concave, and the other one that is notably convex; and though we used some little artifices (which would be troublesome here to mention) to estimate the protuberance of the one liquor and the deficiency of the other, as near the truth as could be, yet I am not sure but there may have been a few mercurial corpuscles more than there should have been, and that consequently some small abatement may have been made of the weight newly attributed to the whole mercurial cylinder of 30 inches.

2. I HAD thoughts of making use of the barrel of a gun, of a convenient length, to find the weight of a mercurial cylinder of 2 feet and  $\frac{1}{2}$ ; but I preferred the instrument already made use of (especially not being where I could have one bored after a peculiar way) not only because I could not meet with one whose diameter was just an inch, and consequently as convenient for calculations, and because that the barrels of guns are often bored a little tapering; but because a skilful artificer confessed to me that they scarce ever bore such barrels but with a four-square bit (as they call it) which leaves the cavity too angular, or too imperfectly round; whereas if an hexahedral bit be employed, it will, as he affirmed, make the cavity almost as cylindrical as can be reasonably desired. I say nothing here of making use, for our purpose, of a trunk, as they call a hollow cylinder of wood, because I elsewhere shew that wood (at least such as the trunks of shoot pellets with are wont to be made of) is not of a texture close enough for such an use.

3. BECAUSE in cylinders of mercury, 30 inches is a height which the atmosphere is seldom heavy enough to be able to counterpoise; and because 29 inches is somewhat nearer the middle between the greatest and the least heights, at which I have observed the

the mercury at differing times to stand in good barometers; your lordship may, if you please, abate a thirtieth part of the weight assigned above to a mercurial cylinder of 30 inches (though I take 29 and  $\frac{1}{4}$ , or thereabouts, to be somewhat a more usual height of the mercury than precisely nine-and-twenty).

4. THE weight of a mercurial cylinder in an æquilibrium with the atmosphere, and of one inch in diameter, being thus settled, we may, by the help of the doctrine of proportions and a few propositions, especially the 14th of the 11th book of *Euclid's Elements*, easily enough calculate the weight of a cylinder of mercury of another diameter, and consequently the force of the pressure of an atmospherical pillar of the same diameter. For since according to the forenamed 14th proposition of the 11th, cylinders of equal heights are to one another as their bases; and since by the second proposition of the same 11th element, circles (such as are the bases of cylinders) are to one another as the squares of their diameters; and since, lastly, we suppose that mercury being a homogeneous body, at least as to sense, the mercurial cylinders will have the same proportion to each other in weight that they have in bulk; since, I say, these things are so, if, for instance, we desire to know what will be the weight of a cylinder of 30 inches high whose diameter is two inches, the rule will be this:

As the square of the diameter of the standard cylinder (as I call that whose weight is already known) is to the square of the diameter of the cylinder proposed, so will the bulk of the former cylinder be to that of the latter, and the weight of that to the weight of this.

ACCORDING to which rule, the square of one inch (which is the diameter of the standard cylinder) being but one (whereby your lordship may perceive how much the measure I pitched on facilitates computations) and the square of two (which is the diameter of the proposed cylinder) being four, the bulk or solid contents of this latter cylinder, and consequently its weight, will be four times as great as those of the standard cylinder; and so, since the lesser has been already supposed to weigh 11.8 lb. avoirdupoise, the mercurial cylinder of two inches in diameter will weigh 47.2 lb. of the same weight.

## EXPERIMENT XXXI.

*About the attractive virtue of the loadstone in an exhausted receiver.*

SOME learned modern philosophers that have attempted to explicate the cause and manner of magnetical attraction or coition, give such an account of it, as supposes that the air between the two magnetical bodies, being driven away by their effluvia, from between them, presses them on the parts opposite to those where the contact is to be made; and upon some such score (for I must not now stay to deliver their theories circumstantially) the air is supposed to contribute very much to the attraction and sustentation of the iron by the loadstone; wherefore, partly to examine this opinion, and partly for some other purposes (not necessary now to be mentioned) we thought fit to make the following experiment.

WE took a small, but vigorous loadstone, capped and fitted with a loose plate of steel, so shaped that when it was sustained by the loadstone, we could hang at a little crook, that came out of the midst of it, and pointed downward, a scale wherein to put what weights we should think fit; into this scale we put sometimes more and sometimes less weight; and then by shaking of the loadstone as much as we guessed it would be shaken by the motion of the engine, we found the greatest weight that we presumed it would



would be able to support, in spite of the agitation it would be exposed to, which proved to be, besides the iron-plate and the scale, six ounces Troy weight, to which if we added half an ounce more, the whole weight appeared too easy to be shaken off: this done, we hung the loadstone, with all the weight it sustained, at a button of glass, which we had procured to be fastened on to the top of the inside of a receiver, when it was first blown; and though in about 12 exsuctions we usually emptied such receivers as much as was requisite for most experiments, yet this time, to exhaust it the more accurately, we continued pumping till we had exceeded twice that number of exsuctions; at the end of which time, shaking the engine somewhat rudely, without thereby shaking off the weight that hung at the loadstone, the iron seemed to be very near as firmly sustained by it, as before the air began to be pumped out: I said very near, rather than altogether, because that the withdrawing of the air, though it be not supposed to weaken at all the power of the loadstone precisely considered, yet it must lessen its power to sustain the steel; because this in so thin a medium must weigh heavier than in the air, by the weight of as much air as is equal in bulk to the appended body.

SOME other magnetical trials (and also some electrical ones) I remember I attempted to make by the help of our engine; but not having the notes I took of them now at hand, I shall suspend the mentioning them, till I can give your lordship a more punctual account of them.

## EXPERIMENT XXXII.

*Shewing that when the pressure of the external air is taken off, it is very easy to draw up the sucker of a syringe, though the hole, at which the air or water succeed, be stopped.*

HAVING taken notice that some learned opposers of the modern doctrine about the weight of the atmosphere think themselves more than ordinarily befriended by the difficulty we find in drawing up the embolus or sucker of a syringe, when the hole at which the air or water should succeed is stopped, and by the violence with which, as soon as it is let go, it is, as they imagine, drawn back; and supposing the reason of this confidence of theirs to be that men have not yet been able in these phænomena (as in some others) to prove the interest of the atmosphere's gravity by direct or confessedly analagous experiments; I presumed it will not be unwelcome to your lordship, if I here fortify the speculations that have been or may be proposed to explicate these things according to the hypothesis of the weight of the air, by what we tried to that purpose, among others, when we were making use of a syringe in our engine.

### THE FIRST TRIAL.

WE took a syringe of brass (that metal being closer and stronger than pewter, of which such instruments are usually made) being in length (in the barrel) about six inches, and in diameter about one inch  $\frac{3}{8}$ ; and having, by putting a thin bladder about the sucker, and by pouring a little oil into the cavity of the cylinder (or barrel) brought the instrument to be stanch enough, and yet the sucker to move to and fro without much difficulty, we thrust this to the bottom (or basis) of the barrel to exclude the air; and having unscrewed and laid aside the slender pipe of the syringe (which in this and some other trials was like to prove not only needless but inconvenient) we carefully stopped the  
orifice.

orifice to which the pipe in these instruments is wont to be screwed, and then drawing up the sucker we let it go, to judge by the violence with which it would be driven back again, whether the syringe were light enough for our purpose; and finding it to be so, we fastened to the barrel a ponderous piece of iron to keep it down, and then fastening to the handle of the rammer (or axle-tree of the sucker) one end of a string, whose other end was tied to the often-mentioned turning key, we conveyed this syringe, and the weight belonging unto it, into a receiver; and having pumped out the air, we then See Plate VI. Fig 1. began to turn the key, thereby to shorten the string that tied the handle of the syringe to it; and, as we foretold, that the pressure of the air lately included in the receiver being withdrawn, we should no more find the wonted resistance in drawing up the sucker from the bottom of the cylinder, so we found upon trial that we could very easily pull it up without finding any sensible resistance.

HOWEVER, having thought fit to repeat the experiment (which we did with the like success) lest it might be objected that this want of resistance might proceed, as partly from our employing the turning key to raise the sucker, so principally from some unperceived leak at which the air may be supposed to have got into the cavity of the cylinder, I thought fit not only to examine by trial, after the receiver was removed from off the pump, whether the syringe were not stanch (upon which I found that I could not, without some straining, draw up the sucker even a little way, and that it would be violently beaten back again) but also in one of these experiments to make this variation, that when the receiver being exhausted, we had drawn up the sucker almost to the top of the barrel, by such a string as was purposely chosen somewhat weak, we kept the parts of the syringe in that posture, till we had opened a passage to the outward air, upon whose ingress the sucker was (as we intended it should be) so forcibly depressed, that it broke the string by which it was tied to the turning key, and was violently driven back to the lower part of the barrel, and that notwithstanding these two disadvantageous circumstances; one, that the string was not so weak but that one whom I employed to try it before it was fastened to the syringe, made it sustain a lump of iron that weighed between four and five pounds; and the other, that yet this string was broken long before all the air that flowed in to fill the receiver had got in; so that the pressure of all the admitted air would doubtless have broken a much stronger string, if we had employed such a one to resist the depression of the sucker, which will yet be more evident by a phenomenon of our syringe, that I shall presently have occasion to relate.

#### THE SECOND TRIAL.

##### *Containing a variation of the foregoing.*

We took the syringe employed in the foregoing experiments, and having found by trial that it was, though not perfectly, tight, nor altogether so much so as before, yet enough so for our present purpose (since, when the orifice of the vent in the basis was stopped, if the sucker were more forcibly drawn up a little way, and then let go, it would hastily return, or rather violently be impelled back towards the bottom of the barrel) we made it serve us as well as we could for the following experiment. Of this syringe we did very carefully with a cork and our cement close the vent; and then having tied to the barrel of the syringe a weight that happened to be at hand (and to amount to two pounds and as many ounces) we suspended the rammer of the syringe by a string in a large receiver; and then causing the pump to be applied, we made 11 or 12  
exsuctions



exfuctions of the air, without any appearance of change in the fyringe. But becaufe I had judged the above-mentioned weight fufficient, and fupposed, that the little air ftill remaining in the receiver had yet too ftrong a preffure to be furmounted by it, I caufed the pumping to be continued, and within two or three exfuctions more I perceived the cylinder to begin to be drawn down, though but very flowly, by the weight hanging at it (affifted by its own gravity) and likewife tried (after having purpofely ftopped a-while the working of the pump) that juft upon a frefh fuck the defcent would be manifefly accelerated. And when we had fuffered the barrel and weight to flide down as far as we thought fit, we let in the external air, which, as was to be expected, raifed them both again much fafter than they had fubfided.

N. B. THERE would not have needed any thing near fo great a weight to deprefs the barrel of the fyringe, but that it is difficult in fuch an inftrument to make the fucker fill it accurately enough, without making it fomewhat uneasy to be moved to and fro; upon which account it was neceffary that a weight fhould be added, not only to furmount the preffure of the air remaining in the receiver (which was not, nor needed to be diligently exhausted in this experiment) but to overcome that refiftance which we juft now noted the inequalities of the infide of the cylinder, and thofe of the fucker to give to the motion of the one in or over the other. And yet for all this it is not eafy, though it be not impoffible, to make one of thefe fyringes very tight, efpecially when the nofe is well ftopped and the fucker drawn up, there being often fome little air that ftrains in between the fucker and the barrel, and fome that will be harboured between the fucker, though thruft home, and the bottom of the barrel, befides what may lurk between the fame fucker and the cork that ftops the orifice of the vent: nor were we confident that our fyringe did not at length let fome aërial particles infinuate themfelves into the cavity, which the depression of the barrel had made betwixt the bafes of that barrel and the fucker; and in fuch cafes we ought not to wonder, if upon the return of the air the barrel and weight be not impelled up altogether to the fame height they refted at when they were firft fufpended in the receiver.

2. IT agreed very well with our doctrine, that as the cylinder and weight began not to fall till a great quantity of air had been pumped out of the receiver, fo they did not begin to move upwards prefently upon the freedom that was allowed the air to return into the receiver; for till it had continued a pretty while flowing in, there was not enough of it entered to reftore, by its preffure, the cylinder and the annexed weight to their former fituation.

3. WHAT has been delivered about our experiment may be confirmed by this variation which we made of it; that having fubftituted a far heavier weight inftead of that lately mentioned, the depression of the barrel of the fyringe fucceeded two or three times one after another much fooner than formerly, viz. about the fixth, or at moft the feventh exfuction.

### EXPERIMENT XXXIII.

*About the opening of a fyringe whose p'pe was ftopped in the exhausted receiver, and by the help of it making the preffure of the air lift up a confiderable weight.*

THOUGH the trial I am about to relate had not all the fuccefs I defired, yet perhaps it will not be impertinent to make mention of it, becaufe there is not any fort of experiments that is wont fo much to perfuade the generality of fpectators of the great force of

the pressure of the air, as those wherein they plainly see heavy and solid bodies made to ascend (upon the operation of the air on them) without seeing any other thing lift them up.

See Plate  
VI. Fig. 2.

WE took the often-mentioned syringe, and having closed up the hole at the bottom with good cement, we tied to the barrel a hollow piece of iron that served us for a scale, into which we put divers weights one after another, trying from time to time, whether, when the sucker was forcibly drawn up and held steadily in its highest station, the weight tied to the barrel (which was held down whilst the sucker was drawn up, and afterwards let go) would be considerably raised. And when we perceived that the addition of half a pound or a pound more would make the weight too great to be so raised, we forbore to put in that increase of weight; and having tied the handle of the rammer to the turning-key, we conveyed the syringe, together with its clog, into a receiver, out of which a convenient quantity of air being pumped, we were thereby enabled easily to draw up the sucker without the cylinder; after which, having let in the air, the bystanders concluded that the weight was raised a little, which yet I would not have allowed, if we had not been able, by inclining the engine and the receiver, to make the syringe and weight a little to swing. But to make the effect more evident, I caused a two pound weight to be taken out, and then the receiver being somewhat exhausted, and the air re-admitted, the clog, when all the air was come in, was swiftly raised, and, as it were, snatched up from the middle to the upper part of the suspended rammer.

It is no easy matter to measure, with any certainty and exactness by a syringe, the weight of an atmospherical pillar equal to it in diameter, especially if there be any imperfection in the syringe, either because the sucker does not go close enough, in which case it can scarce be stanch, or because by its pressure against the inside of the barrel, which often happens if it be too close, it hinders the sucker and barrel from sliding without resistance by one another, and consequently there is an undue resistance made to the endeavour of the atmosphere to raise the barrel and weight; and therefore, though our syringe being, upon the account of some ill accident, less in order than it was in some of the foregoing experiments, I must not conclude that a cylinder of the atmosphere of the same wideness with it is equipollent to no greater a weight than that which was taken up in our trial, yet we may safely conclude, that so slender a pillar of the atmosphere is able to raise by a syringe at least such a weight, as in our experiment it actually lifted up, which amounted to above sixteen pounds (avoirdupoise weight) for it exceeded fifteen pounds and three quarters, besides the weight of the syringe's barrel itself.

#### EXPERIMENT XXXIV.

*Shewing that the cause of the ascension of liquors in syringes is to be derived from the pressure of the air.*

I SHALL not here trouble your lordship with what I have elsewhere proposed about the explicating of suction; but as by the lately recited experiments (I mean the 31st, 32d, and 33d) it has appeared, that it is to the pressure of the external air that we should ascribe the difficulty of drawing up the sucker of a syringe when the pipe or the vent is stopped; so I shall now endeavour to shew that the ascension of liquors which follow the sucker when it is drawn up, the pipe being open, depends also upon the pressure of the air incumbent on that liquor.



IF I had been furnished with very tall receivers and such other glasses as I could have wished, I had tried the following experiments with water, as well as quicksilver; but for want of those accommodations I was reduced to make my experiment with the latter only of those liquors, which yet will, I hope, sufficiently make out what was intended.

### THE FIRST TRIAL.

WE took a small receiver shaped almost like a pear, cut off horizontally at both ends (being the same capped glass that is elsewhere mentioned in the accounts of other experiments); we also took the syringe formerly described, and, having fastened on to it with good cement, instead of its own brass-pipe, a small glass-pipe of about half a foot in length, we put this syringe in at the narrower end of the receiver; to whose orifice was afterwards carefully cemented on the brass-cap with the turning-key, whereto was tied by a string the handle of the rammer; then having conveniently placed upon the engine a very short thick glass shaped like a sugar-loaf (which was made use of for want of a better) with a sufficient quantity of quicksilver in it, we so placed the receiver over it, that the lower end of the pipe of the syringe reached almost to the bottom of this glass, and consequently was immersed a pretty way beneath the surface of the quicksilver: we had also poured a little water in the upper part of the syringe, that no air might get in between the sucker and the cylinder, notwithstanding that, by some accident or other, the syringe was become somewhat less tight than before: and last of all, we cemented the receiver to the engine after the usual manner.

THAT which now remained being to try the experiment itself, in order to which all this had been done, the air was pumped out of the receiver (and consequently out of the little glass that held the mercury) and then the sucker being warily drawn up, we could not see the quicksilver ascend to follow it, though a little water, which, it seems, the outward air had thrust in between the sucker and the cylinder, was either raised or flopped in the glass-pipe of the syringe (whereof yet much the greatest part remained unfilled); of which the reason, according to our hypothesis, was manifest, namely, that the air being pumped out of the receiver, the little that remained had not strength enough to press up so ponderous a liquor as the quicksilver into the pipe (though even that little unexhausted air might have spring enough left to raise a little water); and since it appeared by this, that without the pressure of the air the quicksilver would not be elevated, we thought it seasonable to shew, that by the pressure of the air it would: whereupon, the air being let slowly into the receiver, the mercury was quickly impelled up at least to the top of the glass-pipe (though, by reason of some unperceived leak it was not long sustained there).

AND for further satisfaction, when the experiment was to be tried over again, we ordered it to be so made that might plainly be observed, that though, when the receiver not being yet exhausted, the sucker was drawn up but one inch, the mercury would be raised to the upper part of the glass-pipe of the syringe, yet after the exhausting of the receiver, though the sucker was drawn up twice as high, there appeared no ascension of the mercury in the pipe, whose lower part only was darkened by the little glass which contained that fluid metal.

BEFORE I dismiss this experiment, I must, to make good a promise I made your lordship, acquaint you with a phenomenon which does not a little confirm our doctrine, according to which it was easy both to foresee, and to explain it; the phenomenon was, that if, when the air was diligently pumped out of the receiver, the sucker were endeavoured

voured to be pulled up, it could not be so without much difficulty and resistance, such as was formerly found when the vent of the syringe was stopped, of which in our hypothesis the reason may be clearly this; that there being no common air in the receiver to assist by its pressure (whether immediate or mediate) the raising of the sucker, this could not be raised but by a force great enough to surmount the weight of the external air or atmospheric pillar that leaned upon it, so that as the other phenomena of our experiments manifest, that the raising of liquors by a syringe, which is commonly ascribed to attraction, depends upon the pressure of the air; so by this phenomenon it appears that the difficulty of opening a syringe whose pipe is stopped need not be attributed to such a *fuga vacui* as vulgar philosophers refer it to; since, in our case, the same difficulty was found, though the pipe were open and the liquor it was immersed in might have had free access to the place deserted by the sucker.

### THE SECOND TRIAL.

#### *Being a prosecution of the former attempt.*

To vary as well as confirm the foregoing experiment, we caused the syringe to be tied fast to a competently ponderous body that might keep the cylinder unmoved, when the sucker should be drawn up; we also cemented on to the vent or screw at the bottom of the syringe, a pipe of glass of about two inches in length (which should have been longer, but that then there would not have been room in the receiver for pulling up of the sucker) and having placed the heavy body whereto the syringe was tied upon a pedestal of a convenient height, that the glass-pipe might be all seen beneath it, and a very low phial almost filled with quicksilver might be so placed underneath the pipe, that the stagnant mercury reached a good way above the immersed orifice of the said pipe. These things being thus provided, and the handle of the syringe's rammer being tied with a string to the turning-key that belonged to the brass-cover of the receiver, this vessel was cemented on to the engine, and by it exhausted after the usual manner.

WHEN this was done, we looked upon the syringe's glass-pipe above mentioned, and being able to see through it (whereby we were certain that it was not yet full of quicksilver) we did, by the string, draw up the sucker to a good height, but could not perceive the pipe to be filled with any succeeding mercury; wherefore, warily letting in some air, we quickly saw the mercury impelled to the very top of the pipe; and we concluded, from the quantity of quicksilver that was raised, that a pretty deal was also driven into the cavity of the cylinder.

N. B. I HAD once before seen the mercury ascend into the pipe upon the letting in of the air into the emptied receiver; but it seeming somewhat difficult to me to determine whether the sucker had been raised, because there was no mark to guide my estimate by, I thought it might be suspected, that in case the sucker had not been raised, the ascension of the quicksilver might have proceeded from hence, that the air contained in the glass-pipe breaking out through the stagnant mercury upon the exhausting of the receiver, the quicksilver might, upon the return of the air into the receiver, be pressed up into the place deserted by the air that broke out of the pipe; wherefore we caused a string to be tied about the rammer, as near as we could to the top of the cylinder, by which means, when the receiver was the next time exhausted, we perceived, that by drawing up the sucker we had raised it about two inches, if not more, and yet



we could not discern any mercury to follow it (the glass-pipe still continuing transparent) until we had let some air return into the receiver.

THIS experiment, joined with those we have formerly related to have been tried with our syringe, may teach us, that if a syringe were made use of above the atmosphere, neither the stopping of the pipe would hinder the easy drawing up of the sucker, nor the drawing up of the sucker, though the pipe were not stopped, would raise by suction the liquor which the pipe was immersed in.

#### POSTSCRIPT.

SINCE the last-recited experiment was made and written, finding some of our instruments to be in better order than they were when that trial was made, we thought fit to endeavour, by that which follows, to repair an omission or two that formerly we could not well avoid.

HAVING then caused such a glass-pipe, as has been lately mentioned, to be well cemented on to the syringe (whose sucker did now move more easily, and yet fill the barrel more exactly than before) I ordered (being to be absent for a while myself) that the pipe should be filled with spirit of wine tinged with cochineal, that the liquor and its motions might be the better discerned, and that the pipe being filled, that air might be excluded which would else be harboured in the pipe, which caution was omitted in the foregoing experiment: and this the person to whom I committed it, affirmed to have been carefully done, though when he inverted the pipe thus filled into the rest of the red liquor, that was put into a phial, he could not possibly do it so well but that a bubble of air got into the pipe, and took up some (though but a little) room there: by that time I was called upon to see the event of the trial, and could come to look upon it, the receiver was almost quite exhausted; wherefore, after I had made the pumping be continued a little longer, and perceived that the tinged spirit was fallen down out of the pipe, and that which lay in the phial seemed almost to boil at the top, by reason of the emersion of numerous bubbles, I caused the sucker to be, by the help of the turning-key, drawn up by our estimate about two inches and a half, notwithstanding which we could not perceive the spirit of wine to rise in the pipe, though the pumping were before left off: for which reason I ordered the air to be let in very leisurely, upon which we could plainly see that the red spirit was quickly driven up to the top of the pipe; and that it was so likewise into the cavity of the barrel, appeared, when the receiver was removed, by the small quantity of liquor that remained in the phial, and the plenty of it which came out of the syringe.

N.B. THAT if I had not wanted dexterous artificers to work according to a contrivance I had designed, I had attempted to imitate, by the help of the bare spring of the air, such experiments as in the lately recited trials were made to succeed, by the help of the pressure exercised by the air upon the account of its weight.

## EXPERIMENT XXXV.

*Shewing, that upon the pressure of the air depends the sticking of cupping-glasses to the fleshy parts they are applied to.*

It is sufficiently known, that if the air within a cupping-glass be rarefied by the flame of tow, flax, or the like, burned for a little while in it, and the glass be presently claped upon some fleshy part of a man's body, there will quickly ensue a painful and visible swelling of the part covered by the cupping-glass.

It is also known that this experiment is wont to be urged by the schools, as a clear proof of that abhorrence of a vacuum they ascribe to nature; for, say they, the reason of this phenomenon is plainly, that the internal air of the cupping-glass, præternaturally rarefied by heat when the instrument is applied, that heat after a while ceasing, the succeeding cold must again necessarily condense the air; and so this contracted air being no longer able to fill the whole space it replenished before, there would ensue a vacuum, if the flesh covered by the cupping-glass, or adjoining to it, did not swell into the cavity of it, to fill the place deserted by the air.

THOSE moderns that assert the weight of the atmosphere, do thence ingeniously endeavour to deduce the phenomenon; and indeed, if to their hypothesis about the air's weight the consideration of its spring be added, it will be easy enough to explicate the phenomenon, by saying, that when the cupping-glass is first set on, though much of the air it formerly contained were a little before expelled by the heat, yet the same heat, increasing the pressure of the remaining air, is the cause that the absence of the air driven out of the glass does not immediately occasion so sensible a pain; but when that adventitious agitation of the included air ceases, that air having now (because of the paucity of its corpuscles) but a weak spring, can no longer press upon the part covered by the cupping-glass near so strongly, as the outward air does by its weight press upon all the neighbouring parts of the flesh; by which means, according to what we have more than once explicated already, some of the yielding flesh or other body covered by the skin must be forcibly thrust into the cavity of the cupping-glass, where there is less pressure than at the outside of it: and the fibres and membranous parts being thus violently stretched, there must needs follow a sensible pain as well as tumour; which tumour yet does not fill up the cupping-glass, not only because of the resistance of the skin to be so far distended, but also, if the included air have not been much rarefied, because of the spring of the imprisoned air, which grows so much the stronger by how much the swelling flesh reduces the air into less room, as I have sometimes tried, by applying a cupping-glass to quicksilver, or even to water, which will rise in it but to a certain height.

BUT though by this, or some such explication, the argument urged by the schools in favour of the *fuga vacui* may be sufficiently enervated; yet it suited better with the design of this treatise to propose some new experiment to illustrate our hypothesis; and though it seemed to be far more difficult to do it in reference to cupping-glasses than to other subjects, yet I pitched upon two different ways of experimenting, whose success not disappointing me, I shall now give your lordship an account of them.

WE took a glass of about one inch and a half in diameter, but a good deal longer than an ordinarily shaped cupping-glass of that breadth would have been, that there might be the more room for the flame to burn in it and rarefy the air; we also provided a receiver shaped almost like a pear; this receiver was open at both ends; at the sharper whereof



whereof there was but a small orifice, but at the obtuse end there rose up a short neck, <sup>Plate VI.</sup> whose orifice was wide enough to admit with ease the newly mentioned cupping-glass, <sup>Fig. 3.</sup> without touching the sides of it, and we were not willing it should be much larger, lest it should not be so exactly covered by the palm of the hand that should be laid upon it, and lest also the hand should be broken or hurt by the too great weight of the atmosphere, when the included air should be withdrawn from under it.

THESE things being thus prepared, and the smaller orifice of the receiver being fastened with cement to the engine, I caused the cupping-glass to be fastened, with the mouth upwards, to the palm of the hand of a youth whom your lordship may remember to have seen with me, whose hand seemed framed by nature for this experiment, being broad, strong, and very plump; and having pulled the glass to try whether it stuck well on, I caused him to put it into the receiver and lay his hand so upon the orifice lately mentioned, that it might serve for a cover to it, and hinder any air from getting in between them.

THAT which we pretended was, that the receiver being but small (that it might be quickly exhausted, and so not put the youth to a long pain) upon an exsuction or two made with the pump of the air about the cupping-glass, the remaining air should have its pressure so far weakened, as not to be able to support the cupping-glass; especially since if the air without the cupping-glass, but yet in the receiver, should be more rarefied by the removal of that which had been pumped out, than the air included in the cupping-glass was by the precedent heat, this last-mentioned air having a stronger spring (or tendency to expand itself) than the external air of the receiver, the glass must needs fall down, or rather be thrust off, though, in case there had been no air at all left in the cavity of the cupping-glass, the air in the receiver would by its pressure sustain a far greater weight.

THE event of our trial agreed very well with our conjecture. For upon the first suck the cupping-glass fell off, the weight of the atmosphere pressing so hard upon the young man's hand, that, though he be more than ordinary strong, he complained he could very hardly take it off the glass, it was almost thrust into, and, a while after, that his hand was very sore: but this last inconvenience became not so quickly very sensible but that we had time to repeat our experiment by fastening the cupping-glass more strongly than before; so that he complained that it drew in his hand very forcibly; and though that part be not wont to be fleshy, yet the tumour occasioned by the cupping-glass was manifest enough to the eye; but as before, so now, at the very first turning of the stop-cock, to let out the air of the receiver, the cupping-glass fell off.

## EXPERIMENT XXXVI.

*About the making, without heat, a cupping-glass to lift up a great weight.*

THE other experiment I lately told your lordship we had made to illustrate our doctrine about the cause of the sticking of applied cupping-glasses, was tried after the following manner.

WE took the brass-hoop or ring, mentioned in the fifth and sixth experiments, and covered it with a bladder, which was wetted to make it the more limber, and was so tied on to it (which was easy to do) that the bottom of the bladder covered the upper orifice of the hoop, and was stretched, though not strongly, upon it, almost like the membrane that makes the head of a drum; and the neck of the bladder was tied with a string

Plate VI. firing near the middle of the lower orifice of the hoop, and in this lower part of the bladder  
 Fig. 4. we made two or three small holes for the air to pass in and out at; then having placed at the bottom of the often mentioned capped receiver a thick piece of wood that had a hole in it, to receive the neck of the bladder, we so placed the covered hoop upon this piece of wood, that the upper part of the bladder lay parallel to the horizon. This done, we suspended, at the turning-key belonging to the cap of our receiver, a blind head, as chemists call it, of glass, which, for want of a true cupping-glass, we were fain to substitute, and which indeed was not very unlike one either for shape or size; and to the upper part of this glass we fastened a large ring of metal, the better to depress it and make it lean strongly on the bladder.

THESE things being thus made ready, and the receiver cemented on to the engine, we did by the help of the turning-key let down the cupping-glass (for so we shall hereafter call it) till it came almost to touch the level superficies of the bladder; and when the receiver was as far exhausted as we thought fit, but not near as far as it might have been, we let down the cupping-glass a little lower, so that it leaned upon the bladder, and touched it with all the parts of its orifice; so that the cupping-glass with the subjacent bladder was become an internal receiver, if I may so call it, whose air was considerably expanded, and consequently weakened as to its spring. All this being done, we warily let the air into the receiver, and thereby the air that did surround the cupping-glass, which we just now called the internal receiver, having now a stronger pressure than the air in the cupping-glass could resist, the bladder, on which the cupping-glass rested, was, as we looked for, thrust up a pretty way into the cavity of the glass, in which it made a conspicuous tumour, and was made to stick so close to the orifice of it, that one would have thought that the bladder had been violently drawn in, as the skin is wont to be in the ordinary applications of cupping-glasses.

AND because we took notice, that though this glass were not capacious, for it scarce held a pint of water, yet the orifice of it was not very narrow, being in diameter an inch and  $\frac{4}{5}$ , we thought fit in repeating the experiment to add something that seemed odd enough, and was fit to manifest, that cupping-glasses may, without heat, by the bare pressure of the external air, be more strongly fastened, than, for aught we know, they are by the help of flame: having then reiterated the former experiment with this only variation, that we exhausted the receiver further than before, we took out the cupping-glass and the bladder, which together with the included brass-hoop was hanging at it; and then having tied the glass to the hook of a good statera, and tied a large scale to the neck of the bladder, we put in by degrees weights into the scale, till we had loaded it enough to force off the bladder from the glass; which happened not till the whole weight that tended to draw down the bladder amounted to 35 pounds, if not better, of sixteen ounces in the pound: nor did we doubt but that the pressure of the atmosphere would in our experiment have kept up a much greater weight, if we had, before we let in the outward air, diligently exhausted the receiver, which we had purposely forbore to do, for fear the too disproportionate pressure of the external air should break the bladder; which puts me in mind of adding, upon the by, that as more weight was put into the scale, the bladder (stretched more and more by the weight on one side, and the air on the other) appeared to swell higher in the cavity of the glass.

Plate VI.  
 Fig. 5.



## EXPERIMENT XXXVII.

*Shewing that bellows, whose nose is very well stopped, will open of themselves when the pressure of the external air is taken off.*

It is wont by the Peripateticks and others to be made a great argument for the *fuga vacui* which they attribute to nature, that if the nose of a pair of bellows be well stopped, one cannot open them by raising the upper board from the lower. But of this another reason may be easily assigned, without determining whether there be a vacuum or no, namely, the weight and pressure of the air; for when the nose of a pair of bellows that are tight enough is well stopped, no air being able to insinuate itself upon the disjoining of the boards into the cavity made by that disjunction, this cannot be effected, but by such a force as is almost able (I say almost, because ordinary bellows cannot be so well shut, but that there will remain some air in them whose spring will facilitate the opening of them) to raise an atmospherical pillar, whose basis shall be the upper board, which is commonly so large that a less force may serve to break common bellows than to raise so great a weight; but if they were made strong enough, and there were applied a sufficient force to lift so great a weight as the newly mentioned pillar of the atmosphere, the sides might be disjoined, how close and stanch soever the instrument were made.

Thus far one may argue upon the bare principle of the weight of the air, but taking in the spring of it too, I thought one might proceed so much further, that I ventured to foretel divers ingenious men, that if the pressure of the ambient air were taken off, not only it would be easy to open the bellows in spite of their being carefully stopped at the nose, but that they would fly open, as it were, of their own accord, without the application of any external force at all; and it was partly to justify this prediction, as well as to make a trial I thought more considerable, that we made the following experiment.

We caused, then, to be made a pair of bellows, differing from ordinary ones in these particulars. First, that the boards were circular (and so without handles) and of about six inches in diameter: 2. That there was no clack or valve: 3. That the nose was but an inch long, or less, being to be lengthened, if occasion required, with a pipe: 4. That the leather, which was not spared, that the instrument might be the more capacious, was not horny or very stiff, but limber. The reason of the first and third diversity was, that the bellows might be capable to be conveyed into our receiver (for which purpose also, if there had appeared need, the nose might have been made in the uppermost of the two boards); the reason of the second variation was, that the instrument might be the more stanch; and of the fourth, that the bases of the bellows might, as in organ-bellows, be clapped closer together, and harbour less air in the wrinkles and cavity: so that when the bellows were opened to their full extent, by drawing up the upper basis at a button purposely made in the midst of it, the bellows looked like a cylinder of sixteen or eighteen inches high; upon which resemblance I take the liberty to call both the boards, as geometricians do both the circular parts of a cylinder, bases.

BUT though these were made by an artificer, otherwise dexterous, yet it not being his trade to make bellows, nor any other man's in the town I then was in, he could not make them so tight, but that in spite of our oiling the leather and choaking the seams with good cement, there was some little and unperceived hole or cranny, whereby

some air had passage when the nose was accurately stopped; but this was not so considerable, but that if we drew up the upper basis from the lower, the external air would on all sides press the leather inwards, and so make the shape of the instrument very far from being so cylindrical as it would be if the nose were left open.

WHEREFORE, concluding, that notwithstanding this imperfection the bellows would serve, though not for both the experiments I designed, yet for one of them, we carefully stopped the nose, after we had approached the bases to one another, and conveying them into a large receiver, it quickly appeared, when the pump was set on work, that at every exhalation of the incumbent air, the air harboured in the folds of the leather, and the rest of the little cavity that could not but be left between the bases, made the upper of those bases manifestly rise, though its weight (because of the thickness and solidity of the wood) would soon after depress it again, either by driving out some of the air at some place where the instrument was not sufficiently tight, or by making it, as it were, strained through the leather itself; and if the pump were agitated somewhat faster than ordinary, the expansion of the internal air would be greater than could be rendered quite ineffectual by so small a leak, and the upper part of the bellows would be soon raised to a considerable height, as would appear more evidently, if we hastily let in the external air, upon whose ingress the bases would be clapped together, and the upper of them a good way depressed: so that the imperfection of the bellows made the experiment rather more than less concluding; for since there was no external force applied to open them, if notwithstanding that some of the included air could get out of them, yet the spring of the internal air was strong enough to open the bellows, when the ambient air was withdrawn, much more would the effect have been produced, if the bellows had been perfectly staunch.

#### EXPERIMENT XXXVIII.

*About an attempt to examine the motions and sensibility of the Cartesian Materia subtilis, or the Æther, with a pair of bellows made of a bladder, in the exhausted receiver.*

I WILL not now discuss the controversy betwixt some of the modern atomists and the Cartesians; the former of whom think, that betwixt the earth and the stars, and betwixt these themselves, there are vast tracts of space that are empty, save where the beams of light do pass through them; and the latter of whom tell us, that the intervals betwixt the stars and planets, among which the earth may perhaps be reckoned, are perfectly filled, but by a matter far subtler than our air, which some call celestial, and others æther. I shall not, I say, engage in this controversy; but thus much seems evident, that if there be such a celestial matter, it must make up far the greatest part of the universe known to us. For the interstellar part of the world, if I may so stile it, bears so very great a proportion to the globes, and their atmospheres too, if other stars have any, as well as the earth, that it is almost incomparably greater in respect of them, than all our atmosphere is in respect of the clouds, not to make the comparison between the sea and the fishes that swim in it.

WHEREFORE I thought it might very well deserve a heedful inquiry, whether we can by sensible experiments (for I hear what has been attempted by speculative arguments) discover any thing about the existence, or the qualifications of this so vast æther; and I hoped our curiosity might be somewhat assisted by our engine, if I could manage in it such a pair of bellows as I designed: for I proposed to myself to fasten a convenient weight



weight to the upper basis, and clog the lower with another great enough to keep it horizontal and immoveable; that when by the help of the turning-key frequently above mentioned, the upper basis should be raised to its full height, the cavity of the bellows might be brought to its full dimensions: this done, I intended to exhaust the receiver, and consequently the thus opened bellows, with more than ordinary diligence, that so both the receiver and they might be carefully freed from air: after which I purposed to let go the upper base of the bellows, that, being hastily depressed by the incumbent weight, it might speedily enough fall down to the lower basis, and by so much, and so quickly lessening the cavity, might expel thence the matter (if any where) before contained in it, and that (if it could by this way be done) at the hole of a slender pipe fastened either near the bottom of the bellows, or in the upper basis; against, or over the orifice, of which pipe there was to be placed at a convenient distance, either a feather, or (if that should prove too light) the sail of a little windmill made of cards, or some other light body, and fit to be put into motion by the impulse of any matter that should be forced out of the pipe.

By this means it seemed not improbable that some such discovery might be made, as would not be altogether useless in our inquiry. For if, notwithstanding the absence of the air, it should appear by the effects, that a stream of other matter capable to set visible bodies a moving, should issue out at the pipe of the compressed bellows, it would also appear that there may be a much subtler body than common air, and as yet unobserved by the vacuists, or (their adversaries) the schools, that may even copiously be found in places deserted by the air; and that it is not safe to conclude from the absence of the air in our receivers, and in the upper part of those tubes where the Torricellian experiment is made, that there is no other body left but an absolute vacuity, or (as the atomists call it) a *vacuum coarctatum*. But if, on the other side, there should appear no motion at all to be produced, so much as in the feather, it seemed that the vacuists might plausibly argue, that either the cavity of the bellows was absolutely empty, or else that it would be very difficult to prove by any sensible experiment that it was full; and if, by any other way of probation, it be demonstrable that it was replenished with æther, we, that have not yet declared for any party, may by our experiment be taught to have no confident expectations of easily making it sensible by mechanical experiments; and may also be informed, that it is really so subtle and yielding a matter that does not either easily impel such light bodies as even feathers, or sensibly resist, as does the air itself, the motions of other bodies through it, and is able, without resistance, to make its passage through the pores of wood and leather, and also of closer bodies, which we find not that the air doth in its natural or wonted state penetrate.

To illustrate this last clause, I shall add, that to make the trial more accurate, I waved the use of other bellows (especially not having such as I desired) and caused a pair of small bellows to be made with a bladder, as a body, which some of our former experiments have evinced to be of so close a texture, that air will rather break it than pass through it; and that the bladder might no where lose its entireness by seams, we glued on the two bases, the one to the bottom and the other to the opposite part of it, so that the neck came out at a hole purposely made for it in the upper basis; and into the neck it was easy to insert what pipe we thought fit, binding the neck very close to it on the outside. We had likewise thoughts to have another pair of tight bellows made with a very light clack in the lower basis, that by hastily drawing up the other basis, when the receiver and bellows were very carefully exhausted, we might see by the rest, as the lifting up of the clack, whether the subtle matter that was expelled by the upper basis in its



ascent would, according to the modern doctrine of the circle made by moving bodies, be impelled up or not.

We also thought of placing the little pipe of the bladder-bellows (if I may so call them) beneath the surface of water exquisitely freed from air, that we might see, whether upon the depression of the bellows by the incumbent weight, when the receiver was carefully exhausted, there would be any thing expelled at the pipe that would produce bubbles in the liquor wherein its orifice was immersed.

To bring now our conjectures to some trial, we put into a capped receiver the bladder accommodated as before is mentioned; and though we could have wished it had been somewhat larger, because it contained but between half a pint and a pint, yet in regard it was fine and limber, and otherwise fit for our turn, we resolved to try how it would do; and to depress the upper basis of these little bellows the more easily and uniformly, we covered the round piece of pasteboard that made the upper basis with a pewter-plate (with a hole in it for the neck of the bladder) which nevertheless, upon trial, proved not ponderous enough, whereby we were obliged to assist it by laying on it a weight of lead. And to secure the above-mentioned feather (which had a slender and flexible stem, and was left broad at one end, and fastened by cement at the other, so as to stand with its broad end at a convenient distance just over the orifice of the pipe) from being blown aside to either hand, we made it to move in a perpendicular slit in a piece of pasteboard that was fastened to one part of the upper basis, as that which the feather was glued to was to another part. These things being thus provided, the pump was set a-work; and as the ambient air was from time to time withdrawn, so the air in the bladder expanded itself so strongly, as to lift up the metalline weight, and yet in part to fall out at the little glass-pipe of our bellows, as appeared by its blowing up the feather and keeping it suspended till the spring of the air in the bladder was too far weakened to continue to do as it had done. In the mean time we did now and then, by the help of a string fastened to the turning-key and the upper basis of the bellows, let down that basis a little, to observe how upon its sinking the blast against the feather would decrease as the receiver was further and further exhausted: and when we judged it to be sufficiently freed from air, we then let down the weight, but could not perceive that by shutting of the bellows, the feather was at all blown up, as it had been wont to be, though the upper basis were more than usually depressed: and yet it seems somewhat odd, that when, for curiosity, in order to a further trial, the weight was drawn up again, as the upper basis was raised from the lower, the sides of the bladder were sensibly (though not very much) pressed, or drawn inwards. The bellows being thus opened, we let down the upper basis again, but could not perceive that any blast was produced; for though the feather that lay just over and near the orifice of the little glass pipe had some motion, yet this seemed plainly to be but a shaking and almost vibrating motion (to the right and left hand) which it was put into by the upper basis, which the string kept from a smooth and uniform descent, but not to proceed from any blast issuing out of the cavity of the bladder: and for further satisfaction we caused some air to be let into the receiver, because there was a possibility, that unawares to us the slender pipe might by some accident be choked; but though upon the return of the air into the receiver, the bases of the bellows were prest closer together, yet it seemed, that, according to our expectation, some little air got through the pipe into the cavity of the bladder: for when we began to withdraw again the air we had let into the receiver, the bladder began to swell again, and upon our letting down the weight, to blow up and keep up the feather, as had been done before the receiver had been so well exhausted.

What

See Plate  
VI. Fig 7



What conjecture the opening and shutting of our little bellows, more than once or twice, without producing any blast sensible by the raising of the feather, gave some of the by-standers, may be easily guessed by the preamble of this experiment; but whilst I was endeavouring to prosecute it for my own farther information, a mischance that befel the instrument kept me from giving myself the desired satisfaction.

## EXPERIMENT XXXIX.

*About a further attempt to prosecute the inquiry proposed in the foregoing Experiment.*

CONSIDERING with myself, that by the help of some contrivances not difficult, a syringe might be made to serve, as far as our present occasion required, instead of a pair of bellows; I thought it would not be improper to try a differing, and, in some regards, a better way to prosecute an attempt which seemed to me to deserve our curiosity.

I CAUSED then to be made for the formerly mentioned syringe, instead of its straight See Plate-  
pipe, a crooked one, whose shorter leg was parallel to the longer; and this pipe was VII. Fig.  
for greater closeness, after it was screwed on carefully, fastened with cement to the barrel; and because the brass-pipe could scarce be made small enough, we caused a short and very slender pipe of glass to be put into the orifice of the shorter leg, and diligently fastened to it with close cement: then we caused the sucker (by the help of oil, water, and moving it up and down) to be made to go as smoothly as might be, without lessening the stanchness of the syringe. After this there was fastened to the handle of the rammer a weight, made in the form of a ring or hoop, which, by reason of its figure, might be suspended from the newly mentioned handle of the rammer, and hang loose on the outside of the cylinder, and which, both by its figure and its weight, might evenly and swiftly enough depress the sucker, when that being drawn up the weight should be let go. This syringe, thus furnished, was fastened to a broad and heavy pedestal, to keep it in its vertical posture, and to hinder it from tottering, notwithstanding the weight that clogged it. And besides all these things, there was taken a feather which was about two inches long, and of which there was left at the end a piece about the breadth of a man's thumb-nail (the rest on either side of the slender stalk, if I may so call it, being stript off) to cover the hole of the slender glass-pipe of the syringe; for which purpose the other extreme of it was so fastened with cement to the lower part of the syringe (or to its pedestal) that the broad end of the feather was placed (as the other feather was in the foregoing experiment) just over the little orifice of the glass, at such a convenient distance, that when the sucker was a little (though but very little) drawn up and let go again, the weight would depress it fast enough to blow up the broad part of the feather, as high as was permitted by the resistance of the stalk (and that was a good way) the spring of which would presently restore the whole feather to its former position.

ALL these things being done, and the handle of the rammer being tied to the turning-key of a capped receiver, the syringe and its pedestal were inclosed in a capacious receiver (for none but such an one could contain them, and give scope for the rammer's motions) and the pump being set on work, we did, after some quantity of air was drawn out, raise the sucker a little by the help of the turning-key, and then turning the same key the contrary way, we suffered the weight to depress the sucker, that we might see at what rate the feather would be blown up; and finding that it was impelled forcibly enough, we caused the pumping to be so continued that a pretty many pauses were made,



made, during each of which we raised and depressed the sucker as before, and had the opportunity to observe, that as the receiver was more and more exhausted of the air, so the feather was less and less briskly driven up, till at length, when the receiver was well emptied, the usual elevations and depressions of the sucker would not blow it up at all that I could perceive, though they were far more frequently repeated than ever before; nor was I content to look heedfully myself, but I made one, whom I had often employed about pneumatical experiments, to watch attentively, whilst I drew up and let down the sucker; but he affirmed that he could not discern the least beginning of ascension in the feather. And indeed to both of us it seemed that the little and inconsiderable motion that was sometimes (not always) to be discerned in the feather, proceeded not from any thing that issued out of the pipe, but from some little shake, which it was difficult not to give the syringe and pedestal, by the raising and depressing of the sucker.

AND that which made our phenomenon the more considerable was, that the weight that carried down the sucker being still the same, and the motions of the turning-key being easy to be made equal at several times, there seemed no reason to suspect that contingencies did much (if at all) favour the success; but there happened a thing which did manifestly enough disavour it. For I remember, that before the syringe was put into the receiver, when we were trying how the weight would depress it, and it was thought, that though the weight were conveniently shaped, yet it was a little of the least, I would not alter it, but foretold, that when the air in the cavity of the syringe (that now resisted the quickness of its descent, because so much air could not easily and nimbly get out at so small a pipe) should be exhausted with the other air of the receiver, the elevated sucker would fall down more easily, which he that was employed to manage the syringe whilst I watched the feather, affirmed himself afterwards to observe very evidently: so that when the receiver was exhausted, if there had been in the cavity of the syringe a matter as fit as air to make a wind of, the blast ought to have been greater, because the celerity that the sucker was depressed with was so.

AFTER we had long enough tried in vain to raise the feather, I ordered some air to be let into the receiver; and though when the admitted air was but very little, the motions of the sucker had scarce, if at all, any sensible operation upon the feather, yet when the quantity of air began to be somewhat considerable, the feather began to be a little moved upwards, and so by letting in air not all at once, but more and more from time to time, and by moving the sucker up and down in the intervals of those times of admission, we had the opportunity to observe, that as the receiver had more air in it, the feather would be more briskly blown up.

BUT not content with a single trial of an experiment of this consequence, we caused the receiver to be again exhausted, and prosecuted the trial with the like success as before, only this one circumstance that we added, for confirmation, may be fit to be here taken notice of. Having, after the receiver was exhausted, drawn up and let fall the sucker divers times ineffectually, though hitherto we had not usually raised it any higher at a time, than we could by one turn of the hand, both because we could not so conveniently raise it higher by the hand alone, and because we thought it unnecessary, since that height sufficed to make the air briskly toss up the feather; yet *ex abundanti* we now took an instrument that was pretty long, and fit so to take hold on the turning-key, that we could easily raise the sucker between two and three inches, by our estimate, at a time, and nimbly depress it again; and for all this, which would much have increased the blast, if there had been a matter fit for it in the cavity of the syringe, we could not sensibly blow up the feather till we had let a little air into the receiver.



To be able to make an estimate of the quantity of air pumped out, or let in, when the feather was strongly or faintly, or not at all raised by the fall of the sucker, we took off the receiver, and conveyed a gage into it, but though for a while we made some use of our gage, yet a mischance befalling it before the operation was quite ended, I shall forbear to add any thing concerning that trial, and proceed to say something of another attempt, wherein, though I foresaw and met with such difficulties, as kept me from doing altogether what I desired, yet the success being almost as good as could be expected, I shall venture to acquaint your lordship with the trial, which was this.

INSTEAD of the hitherto employed pipe of brass, there was well fastened, with ce-See Plate ment, to the syringe, a pipe of glass, whose figure differed from that of the other in this VII. Fig. particular, that the shorter or remoter leg of our new pipe, after it had for a while been carried parallel to the other leg, was bent off so, that above an inch and a half of it tended downwards, that the orifice of it might be immersed into water contained in a small open jar: the design of which contrivance was, that when the receiver should be well exhausted, we might, according to what I told your lordship was at first designed, try whether by the raising and depressing of the sucker any such matter would be driven out at the nose of the pipe, as would produce bubbles in the incumbent water, which air (though highly rarefied, perhaps to some hundreds of times beyond its wonted dimensions) is capable of doing: and I choose to employ rather water than quicksilver, because, though by using the latter, I might hope to be less troubled with bubbles, yet the ponderousness and opacity of it seemed to outweigh that convenience.

I NEED not tell your lordship, that in other respects this experiment was made like the former; so that I shall mention only its peculiarities, which were, that as the air was pumped out of the receiver, that in the glass-pipe made its way through the water in bubbles; and a little air having once by a small leak got in, and forced some of the water out of the jar into the pipe, when the receiver was again well emptied, both that water, and even the little quantity of stagnant water that was contained in the immersed part of the pipe, produced so many bubbles of several sizes, as quite disturbed our observations. Wherefore we let alone the receiver, exhausted as it was, for six or seven hours, to give the water time to be freed from air; and then causing what air might have stolen in to be again pumped out, till we had perceived by the gage that the receiver was well exhausted, we caused the sucker of the syringe to be raised and depressed divers times; and though even then a bubble would now and then make our observations troublesome and less certain, yet it seemed to us, that when we were not thus confounded, we sometimes observed that the elevation and fall of the sucker, though reiterated, did not drive out at the pipe any thing that made any discernible bubbles in the incumbent water; for though there would appear now and then some small bubbles on the surface of the water, yet I could not perceive that the matter that made them issued out at the pipe; and some of them manifestly proceeded from aerial particles, till then lurking in the water, as I concluded from the place and time of their rising. But this non eruption of bubbles at the nose of the pipe was not that which gave me the most satisfaction; for at length both I and another had the opportunity to observe the water in the immersed part of the pipe, which was very slender, to be about an inch higher than the rest of the stagnant water, and to continue at that height or place in the pipe, though the sucker were divers times together raised and depressed, by guess, between two and three inches at a time; which seemed to argue either that there was a vacuum in the cavity of the syringe, or else, that if it were full of æther, that body was so subtle that the impulse it received from the falling sucker would not make it displace a very little thread (perhaps not exceeding a grain in weight) of water that was in the slender pipe, though it appeared.



appeared by the bubbles, that sometimes disclosed themselves in the water, after the receiver had been exhausted, that far more water would be displaced and carried up by a small bubble, consisting of such rarefied air, that according to my estimate, the aerial particles of it did not, before the pump was begun to be set on work, take up in the water a five-hundredth part of the quantity of a pin's head.

BUT whilst we were considering what to do further in our trial, a little air that strained in at some small undiscoverable leak, drove the water into the emptied part of the pipe, and put an end for that time to our trial, which had been too toilsome to invite us then to reiterate it.

I HAD indeed thoughts of prosecuting the inquiry by dropping from the top of the exhausted receiver light bodies conveniently shaped, to be turned round or otherwise put out of their simplest motion of descent, if they met with any resistance in their fall; and by making such bodies move horizontally and otherwise in the receiver, as would probably discover whether they were assisted by the medium. And other contrivances and ways I had in my thoughts, whereby to prosecute our enquiry; but wanting time for other experiments, I could not spare so much as was necessary to exhaust large receivers so diligently as such nice trials would exact; and therefore I resolved to desist till I had more leisure than I then had, or have since been master of.

IN the interim, thus much we seem to have already discovered by our past trials, that if when our vessels are very diligently freed from air, they are full of æther, that æther is such a body as will not be made sensibly to move a light feather by such an impulse as would make the air manifestly move it, not only whilst it is no thinner than common air, but when it is very highly rarefied (which, if I mistake not, it was in our experiment so much, as to be brought to take up above an hundred times more room than before).

AND one thing more we gained by the trial made with water, namely, a clear confirmation of what I have delivered in the 34th experiment, about the cause of the suction that is made by syringes; for your lordship may remember, that at the close of the experiment we have all this while been reciting, I observed, that when the external air was so very well withdrawn, the pulling up of the sucker would not make the stagnant water that the pipe of the syringe was immersed in, to ascend one inch, or so much as the tenth part of it.

## EXPERIMENT XL.

*About the falling, in the exhausted receiver, of a light body, fitted to have its motion visibly varied by a small resistance of the air.*

PARTLY to try, whether in the space deserted by the air, drawn out of our receivers, there would be any thing more fit to resist the motion of other light bodies through it, than in the former experiment we found it to impel them into motion; and partly for another purpose to be mentioned by and by, we made the following trials.

WE took a receiver, which, though less tall than we would have had, was the longest we could procure; and that we might be able, not so properly to let down as to let fall a body in it, we so fastened a small pair of tobacco-tongs to the inside of the receiver's brass-cover, that by moving the turning-key we might, by a string tied to one part of them, open the tongs, which else their own spring would keep shut. This being done, the next thing was to provide a body which would not fall down like a stone, or another dead



dead weight through the air, but would, in the manner of its descent, shew, that its motion was somewhat resisted by the air; wherefore, that we might have a body that would be turned about horizontally, as it were, in its fall, we thought fit to join cross-wise four broad and light feathers (each about an inch long) at their quills with a little cement, into which we also stuck perpendicularly a small label of paper, about an 8th of an inch in breadth, and somewhat more in height, by which the tongs might take hold of our light instrument without touching the cement, which else might stick to them.

By the help of this small piece of paper the little instrument, of which it made a See Plate part, was so taken hold of by the tongs, that it hung as horizontal as such a thing could VII. Fig. well be placed; and then the receiver being cemented on to the engine, the pump was<sup>4</sup> diligently plied, till it appeared by a gage which had been conveyed in, that the receiver had been carefully exhausted; lastly, our eyes being attentively fixed upon the connected feathers, the tongs were by the help of the turning-key opened, and the little instrument let fall, which, though in the air it had made some turns in its descent from the same height which it now fell from, yet now it descended like a dead weight, without being perceived by any of us to make so much as one turn, or a part of it: notwithstanding which I did, for greater security, cause the receiver to be taken off and put on again, after the feathers were taken hold of by the tongs; whence being let fall in the receiver unexhausted, they made some turns in their descent, as they also did being a second time let fall after the same manner.

But when after this, the feathers being placed as before, we repeated the experiment by carefully pumping out the air, neither I nor any of the by-standers could perceive any thing of turning in the descent of the feathers; and yet for further security we let them fall twice more in the unexhausted receiver, and found them to turn in falling as before; whereas when we did a third time let them fall in the well exhausted receiver, they fell after the same manner as they had done formerly, when the air, that would by its resistance have turned them round, was removed out of their way.

N. B. 1. THOUGH, as I intimated above, the glass wherein this experiment was made, were nothing near so tall as I would have had it, yet it was taller than any of our ordinary receivers, it being in height about 22 inches.

2. ONE that had more leisure and conveniency might have made a more commodious instrument than that we made use of; for being accidentally visited by that sagacious mathematician Dr. Wren, and speaking to him of this matter, he was pleased with great dexterity as well as readiness to make me a little instrument of paper, on which, when it was let fall, the resistance of the air had so manifest an operation, that I should have made use of it in our experiment, had it not been casually lost when the ingenious maker was gone out of these parts.

3. THOUGH I have but briefly related our having so ordered the matter that we could conveniently let fall a body in the receiver when very well exhausted; yet, to contrive and put in practice what was necessary to perform this, was not so very easy, and it would be difficult to describe it circumstantially without very many words; for which reason I forbear an account that would prove too tedious to us both.

4. WHAT has been hitherto related was done in prosecution of but one of the two designs I aimed at in the foregoing contrivance, by which I intended, if I could have procured a receiver tall enough, to try whether bodies (some very light, and some heavier) being let fall, when the air was very diligently pumped out, would not descend somewhat faster than if the receiver were full of air: but though I had provided a pendulum that vibrated quarters of seconds, yet the glass being no higher than it was, the

descent even of our feathers took up so little time, that even this pendulum was of no use; only it seemed to all of us that were present at making the above recited trials, that when the feathers were let fall at such times as the air that would have turned them round in their descent was removed, they came to the bottom sensibly sooner than at other times. But when we shall have opportunity to repeat the experiment in taller glasses, and to make some variation of it, I hope to be able to give your lordship a fuller satisfaction about this particular. And in the mean while I shall forbear to examine whether the air might somewhat retard the descent of the feathers upon some other account, or merely upon that of its being a medium not quite devoid of gravity.

#### AN NOT A T I O N S.

1. BUT here I must be so sincere as to inform your lordship, that this fortieth experiment seemed not to prove so much as did the foregoing made with the syringe; for being suspicious, that, to make the feathered body above mentioned turn in its fall, there would need a resistance not altogether inconsiderable, I caused the experiment to be repeated, when the receiver was, by our estimate, little or nothing more than half exhausted, and yet the remaining air was too far rarefied to make the falling body manifestly turn.

2. AND yet perchance it would have happened otherwise, if the receiver had been tall enough; which, though I had not then leisure and conveniency to make it, yet it will not be amiss to let your lordship know by what means we did, that it might be somewhat fit to make the recited experiment and some others, bring it to the height it had, which did considerably exceed that of the tallest glass we could then procure.

To lengthen our receiver, therefore, we thought fit to try, whether we could not close enough fasten to the bottom of it with very good cement a cylindrical pipe of latten whose upper orifice should have near the same breadth with the bottom of the glass: and though this contrivance seemed liable to a couple of not mean difficulties; the one, that the latten being every where bended, and in some places necessary to be soldered, it would be very hard, as indeed we found it, to avoid some small cracks and leaks; and the other, that if the metalline pipe were wide enough, so great and heavy a pillar of the atmosphere would come to bear against it as to press it inwards, if not also to break it; yet we hoped we should be able to obviate both of these inconveniencies. Against the first of which our remedy was, to coat over very carefully the whole pipe with the same close cement wherewith we fastened it to the glass receiver: and against the second, we provided a little frame, consisting of divers small iron bars fastened together; which frame (though it were not too wide to go into the cylinder of latten, yet it) was wide enough to be so near it on the inside, that (though the weight of the atmosphere should, as we feared, press the latten so as to make it yield inward, yet) it could make it bend no further than the iron-frame would permit; which was not far enough to spoil either the receiver or the experiment: and this not unpleasant phenomenon would somewhat surprize unaccustomed spectators, that when after the receiver had been very well exhausted, the external air was permitted to return, there would be heard during some time, from the metalline part of the receiver, divers sounds brisk enough, which would make an odd cracking noise proceeding from the latten-plate, which having been forcibly, though but slowly, bent inwards by the predominant pressure of the atmosphere, was now assisted by the pressure of the returning air to regain its former figure: and as I thought not fit to omit this circumstance, because it confirms the practicableness of the remedy proposed against the second inconvenience; so I thought fit to mention this way of enlarging



larging and heightening receivers, because what we have related seems to give grounds of hoping that this contrivance may be made good use of in divers other trials, and particularly in attempts to make receivers capacious enough to contain larger animals, and perhaps even a boy or a man. In order to some of which purposes we endeavoured to get an improvement made of our metalline cylinder by additional contrivances; but could not, where we then were, get artificers that would perform what was directed.

## EXPERIMENT XLI.

*About the propagation of sounds in the exhausted receiver.*

To make some further observation than is mentioned in the \* published experiments, about the production and conveying of sounds in a glass whence the air is drawn out, we employed a contrivance, of which, because we make use of it in divers other experiments, it will be requisite to give your lordship here some short description.

WE caused to be made at the turner's a cylinder of box, or the like close and firm wood, and of a length suitable to that of the receiver it was to be employed in. See Plate VIII. Fig. Out of the lower basis of this cylinder (which might be about an inch and a half in diameter) there came a smaller cylinder or axle-tree, not a quarter so thick as the other, and less than an inch long; this was turned very true, that it might move to and fro; or, as the tradesmen call it, ride very smoothly in a little ferrule or ring of brass, that was by the same turner made for it in the midst of the fixed trencher (as we call a piece of solid wood, shaped like a mill-stone) being four or five inches, more or less (according to the wideness of the receiver) in breadth, and between one and two in thickness; and in a large and round groove or gutter, purposely made in the lower part of this trencher, I caused as much lead as would fill it up to be placed and fastened, that it might keep the trencher from being easily moved out of its place or posture, and in the upper part of this trencher it was intended that holes should be made at such places as should be thought fit, to place bodies at several distances as occasion should require. The upper basis of the cylinder had also coming out of the midst of it another axle-tree, but wider than the former, that, into a cavity made in it, it might receive the lower end of the turning-key divers times already mentioned, to which it was to be fastened by a slender peg of brass thrust through two correspondent holes, the one made in the key, and the other in the newly-mentioned socket (if I may so call it) of the axle-tree. Besides all which, there were divers horizontal perforations bored here and there in the pillar itself, to which this axis belonged, which pillar we shall, to avoid ambiguity, call the vertical cylinder. The general use of this contrivance (whose other parts need not to be mentioned before the experiments where they are employed) is, that the end of the turning-key being put into the socket, and the lower axis of the vertical cylinder into the trencher, by the motion of the key a body fastened at one of the holes to the cylinder may be approached to, or removed from, or made to rub or strike against another body fastened in a convenient posture to the upper part of the trencher.

To come now to our trial about sounds, we caused a hand-bell (whose handle and clapper were taken away) to be fastened to a strong wire, that, one end of the wire being made fast in the trencher, the other end, which was purposely bent downwards, took hold of the bell. In another hole made in the circumference of the same trencher was

\* Page 105, 106.

wedged in (with a wooden peg) a steel-spring, to whose upper part was tied a gad of iron or steel; less than an inch long, but of a pretty thickness. The length of this spring was such, as to make the upper part of the hammer (if I may so call the piece of iron) of the same height with the bell, and the distance of the spring from the bell was such, that when it was forced back the other way, it might at its return make the hammer strike briskly upon the outside of the bell.

THE trencher being thus furnished and placed in a capped receiver (as you know, for brevity sake, we use to call one that is fitted with one or other of the brass covers, often mentioned already) the air was diligently pumped out; and then, by the help of the turning-key, the vertical cylinder was made to go round, by which means as often as either of a couple of stiff wires or small pegs that were fastened at right angles into holes, made not far from the bottom of the cylinder, passed (under the bell, and) by the lately mentioned spring, they forcibly did in their passage bend it from the bell, by which means, as soon as the wire was gone by, and the spring ceased to be pressed, it would fly back with violence enough to make the hammer give a smart stroke upon the bell: and by this means we could both continue the experiment at discretion, and make the percussions more equally strong, than it would otherwise have been easy to do.

THE event of our trial was, that, when the receiver was well emptied, it sometimes seemed doubtful, especially to some of the by-standers, whether any sound were produced or no; but to me, for the most part, it seemed, that after much attention I heard a sound, that I could but just hear; and yet, which is odd, methought it had somewhat of the nature of shrillness in it, but seemed (which is not strange) to come from a good way off. Whether the often turning of the cylindrical key kept the receiver from being so stanch as else it would have been, upon which score some little air might insinuate itself, I shall not positively determine; but to discover what interest the presence or the absence of the air might have in the loudness or lowness of the sound, I caused the air to be let into the receiver, not all at once, but at several times, with competent intervals between them; by which expedient it was easy to observe, that the vertical cylinder being still made to go round, when a little air was let in, the stroke of the hammer upon the bell (that before could now and then not be heard, and for the most part be but very scarcely heard) began to be easily heard; and when a little more air was let in, the sound grew more and more audible, and so increased, until the receiver was again replenished with air; though even then (that we omit not that phenomenon) the sound was observed to be much less loud than when the receiver was not interposed between the bell and the ear.

AND whereas in the already published physico-mechanical experiments, I acquainted your lordship with what I observed about the sound of an ordinary watch in the exhausted receiver, I shall now add, that that experiment was repeated not long since, with the addition of suspending in the receiver a watch with a good alarm, which was purposely so set, that it might, before it should begin to ring, give us time to cement on the receiver very carefully, exhaust it very diligently, and settle ourselves in a silent and attentive posture. And to make this experiment in some respect more accurate than the others we made of sounds, we secured ourselves against any leaking at the top, by employing a receiver that was made all of one piece of glass (and consequently had no cover cemented on to it) being furnished only within (when it was first blown) with a glass-knob or button, to which a string might be tied. And because it might be suspected, that if the watch were suspended by its own silver chain, the tremulous motion of its sounding bell might be propagated by that metalline chain to the upper part of  
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the glass, to obviate this as well as we could, we hung the watch, not by its chain, but a very slender thread, whose upper end was fastened to the newly mentioned glass-button.

THESE things being done, and the air being carefully pumped out, we silently expected the time, when the alarm should begin to ring, which it was easy to know by the help of our other watches; but not hearing any noise so soon as we expected, it would perhaps have been doubted whether the watch continued going, if for prevention we had not ordered the matter so, that we could discern it did not stand still: wherefore I desired an ingenious gentleman to hold his ear just over the button at which the watch was suspended, and to hold it also very near to the receiver; upon which he told us, that he could perceive, and but just perceive something of sound that seemed to come from far; though neither we that listened very attentively near other parts of the receiver, nor he, if his ears were no more advantaged in point of position than ours, were satisfied that we heard the watch at all. Wherefore ordering some air to be let in, we did, by the help of attention, begin to hear the alarm, whose sound was odd enough, and, by returning the stop-cock to keep any more air from getting in, we kept the sound thus low for a pretty while, after which a little more air, that was permitted to enter, made it become more audible; and when the air was yet more freely admitted, the by-standers could plainly hear the noise of the yet continuing alarm at a considerable distance from the receiver.

FROM what has hitherto been related, we may learn what is to be thought of what is delivered by the learned *Mersennus*, in that book of his *Harmonicks*, where he makes this to be the first proposition. *Sonus à campanis, vel altis corporibus non solum producitur in illo vacuo (quicquid tandem illud sit) quod sit in tubis hydrargyro plenis, posteaque depletis, sed etiam idem acumen, quod in aere libero vel clauso penitus observatur & auditur.* For the proof of which assertion, not long after, he speaks thus: *porro variis tubis, quorum extremis lagene vitreae adglutinantur, observari campanas in illo vacuo appensas propriisque malleis percussas idem penitus acumen retinere, quod in aere libero habent: atque soni magnitudinem ei sono, qui sit in aere quem tubus clausus includit, nihil cedere.* But though our experiments sufficiently manifest, that the presence or absence of the common air is of no small importance as to the conveying of sounds, and that the interposition of glass may sensibly weaken them; yet so diligent and faithful a writer as *Mersennus* deserves to be favourably treated; and therefore I shall represent on his behalf, that what he says may well enough have been true, as far as could be gathered from the trials he made. For, first, it is no easy matter, especially for those that have not peculiar and very close cements, to keep the air quite out for any considerable time in vessels consisting of divers pieces, such as he appears to have made use of; and next, the bigness of the bell in reference to the capacity of the exhausted glass, and the thickness of the glass, and the manner whereby the bell was fastened to the inside of the glass, and the hammer or clapper was made to strike, may much vary the effect of the trial, for reasons easy to be gathered out of the past discourse, and therefore not needful to be here insisted on. And upon this account we chose to make our experiment with sounds that should not be strong or loud, and to produce them after such a manner, as that as little shaking as could be might be given by the sounding body to the glass it was included in. The proposal made by the same *Mersennus*, to have those that have industry enough try whether a bag-pipe will be made to afford the same sound as in the open air, in such vessels as he used for his bells, though he seems to think it would succeed, is that which your lordship will not, I presume, solicit me to make trial of, if you remember what is related in the almost immediately foregoing experiments, shewing, that

we



we could make nothing come out of the cavity of a pair of bellows that had force enough to blow away a feather, when that cavity was freed from air; as the bagpipe would be by the same operation, that empties the glass that contains it, or else the sound would not be made in such a vacuum as the scope of the experiment requires.

If I had had conveniency, I would have made some trials by conveying a small stringed instrument (perhaps some such as they commonly call a kit) exactly tuned, into a large receiver, and then upon briskly striking the string of a bigger instrument (tuned, as they speak, to an unison to (or with) that of the smaller instrument) I should have taken notice, whether the sound would have been so uniformly propagated notwithstanding the interposition of the glass receiver, as sensibly to shake the included string; in order to the discerning of which, a bended piece of straw or feather, or some such light body, was to be hoisted upon the string to be shaken. I also intended, in case the string were made to move, to make the like trial after the receiver was diligently exhausted. And lastly, I designed to try whether two unison strings of the same instruments, or of a couple to be placed in the same receiver, would, when the air (which is the usual medium of sounds) was well pumped out, yet maintain such a sympathy, as it is called, that upon the motion of the one, the other would also be made to stir; which trials may be varied by employing for the external instrument another instead of a stringed one.

AND because contraries, as is vulgarly noted, serve to illustrate each other, I thought to subjoin, to the trials above related about the propagation of sounds in a thinner medium than the air, some observations about the conveyance of them through that thicker medium, water; but having unluckily mislaid my notes upon that subject, I cannot at present acquaint your lordship with what I intended, but must defer the doing it till I shall have recovered them.

## EXPERIMENT XLII.

### *About the breaking of a glass-drop in an exhausted receiver.*

You know that among the causes that have been proposed of the strange flying of a glass-drop into a multitude of pieces, when the slender stem of it comes to be broken off; one of the least improbable was taken from the pressure of the air; as if that within the porous (and as it were honey-combed) inside of the glass, being highly rarefied when the drop of melted glass fell into the water at its first formation, it was forced to continue in that preternatural state of expansion by the hardness and closeness of the external case of glass that inclosed the pith-like part (if I may so call it) so that upon the breaking off a part of this solid case at the stem, the external air gaining access, and finding in the spongy part very little resistance from the highly rarefied and consequently weakened air included there, rushes in with such violence as to shiver the glass-drop into a multitude of pieces.

I SHALL not now trouble your lordship with the mention of what may be alledged to question this hypothesis, especially if it be compared with that accurate account of the phenomena of such glass-drops, which was sometimes since presented to the society by that great ornament of it Sir *Robert Moray*; but I shall only say in this place, that when I considered, that if the diffillation of the glass would succeed when the air was pumped out of it, it would be hard to ascribe that effect to the eruption of the external air. I thought fit to try what would happen if a glass-drop were broken in our exhausted receiver,



receiver; and accordingly did, though not without some difficulty, so order the matter that the blunter part of the glass-drop was fastened to a stable body (conveyed into the receiver) and the crooked stem was tied to one end of a string, whose other end was fastened to the turning-key; by which means, when the air had been diligently pumped out, the stem was (by shortening the string) broken off, and the glass-drop was shattered into a thousand pieces.

This experiment was long after repeated with the like success; and having at that time no gage to try how far the air had been drawn out, we let the external air impel up the water out of the pump into the receiver, and thereby found that that vessel had not been negligently exhausted.

### EXPERIMENT XLIII.

*About the production of light in the exhausted receiver.*

I PRESUME, I need not put your lordship in mind, that divers attempts were made to try whether either a flame, or kindled coals, would be made to continue for some time burning in our receiver. But those trials making it evident that it would be either impossible or very difficult to produce any durable light, without the presence of the air, by the burning of bodies, I thought it not amiss, considering the nobleness of light, to make trial, whether it might be otherwise produced in our exhausted receiver; since whether or no the attempts should prove successful, the event would probably be instructive. For as it is the property of light, when it is produced, to be discoverable by itself; so in such a trial as we intended, it would teach something concerning light, to find that the absence of the air would or would not hinder it from being produced. In prosecution of this design, knowing that hard sugar, being nimbly scraped with a knife, will afford a sparkling light, so that now and then one would think that sparks of fire fly from it, we caused a good lump of hard loaf-sugar to be conveniently and firmly placed in the cavity of our capped receiver, and to the vertical cylinder forementioned we caused to be fastened some pieces of a steel-spring, which, being not very thick, might in their passage along the sugar grate, or rub forcibly against it, and then the receiver being diligently exhausted in the night-time, and in a dark room, the vertical cylinder (whose lower axis was inserted into the often-mentioned trencher) was made for a pretty while to move round by the help of the turning-key, managed by a hand steady and strong enough. By which means the irons that came out of the vertical cylinder, making in their passage vigorous impressions upon the sugar that stood somewhat in their way, there were manifestly produced a good number of little flashes, and sometimes too, though not frequently, there seemed to be struck off little sparks of fire.

The contrivance here mentioned may be conceived by considering the figure belonging to the 41 experiment.

### EXPERIMENT XLIV.

*About the production of a kind of halo and colours in the exhausted receiver.*

WE took a large inverted cucurbite for a receiver, which being so well wiped both within and without as to be very clear, allowed me to observe, and to make others do so too, that when the pump began to be set a work, if I caused a pretty large candle

to be held on the other side of the glass, upon the turning of the stop-cock to let the air out of the receiver into the cylinder, the glass would seem to be full of fumes, and there would appear about the flame of the candle, seen through them, a kind of halo, that at first commonly was between blue and green, and after some fucks would be of a reddish or orange colour, and both very vivid. The production of this meteor, if I may so call it, was, according to my conjecture, made on some such score as this: that the cement being somewhat soft and new, as is convenient for this experiment, abounds with turpentine; and having a little, as well to fasten on the receiver, as for the other purpose, applied to it a hot iron, whereby the cement was both softened and heated, it seemed rational to expect, that upon the withdrawing of the air in the receiver, the aerial particles in the cement, freed from their former pressure, would extricate themselves, and with the looser steams of the turpentine, and perhaps of the bees-wax, would with a kind of explosion expand themselves in the receiver, and by their interposition between the light and the eye exhibit those delightful colours we had seen. To confirm which, I afterwards found, that by watchfully observing it, I could plainly enough perceive the colouring steams, just upon the turning of the stop-cock, to fly up from the cement towards the top of the glass; and if we continued pumping, the receiver would grow clearer, and the colours more dilute, till we had occasion to put on the receiver, and heat the cement afresh; of which the reason might be, partly that the aerial and volatile particles of the upper part of the cement did in that tract of time spend themselves more and more; and partly, because the agitation they received from the heat communicated by the iron did continually decay: not to mention, that when the receiver is more exhausted, the want of air makes it more difficult for steams to be supported, and, as it were, swim up and down in it.

For farther confirmation, I caused some cement to be put into a small crucible warm enough to melt it; and conveying this into a clear receiver of a convenient shape and size, I caused the pump to be set a work; whereupon it appeared manifestly enough, that upon the opening of the stop-cock to let out the air, the steams would copiously be thrown about from the crucible into the capacity of the receiver, and would, after having a little played there, fall down again. But in these apparitions the vividness, and sometimes the kind of the exhibited colours, seemed much to depend upon divers circumstances, such as the degrees of heat, the bigness and shape of the receiver, the quantity of air that yet remained unpumped out, and the nature of the cement itself; which last particular I the rather mention, because, though I were hindered from doing it, I had thoughts to try a suspicion I had, that by varying the materials exposed to this kind of operation, some pretty variety might be made in the phenomena of the experiment.

WHETHER or no the apparition or whiteness, or light, that we sometimes happened to take notice of divers years ago, and have mentioned in the already published part of our Physico-mechanical Experiments, may be partly (though not entirely) referred to some of the cements I then employed, differing from those I now use most, and to the unheeded temper of those cements, as to warmth and degrees of softness, is a doubt that further observation may possibly enable us to determine.



## EXPERIMENT XLV.

*About the production of heat by attrition in the exhausted receiver.*

THE opinion that ascribes the incalcescence of solid bodies, struck or rubbed hard against one another, to the attrition or vehement agitation of the intercepted air, is famous and received enough to seem worthy of a particular examination; but I confess to your lordship, that it was not any thing relating to this opinion that chiefly induced me to make the experiment I am now about to give an account of; for I thought it might be useful to more purposes than one to be able to produce, by attrition, a somewhat durable heat, even in our exhausted receiver; and therefore, though it were to foresee that it would prove no easy task, yet we thought fit to attempt it (sight of the difficulties met with at our first trials. In what way and with what success we afterwards made this attempt, I now proceed to relate.

Cross the stable trencher, formerly mentioned, there was fastened a pretty strong spring of steel or iron, shaped almost like the lath of a cross-bow; and to the midst of this spring was strongly fastened, on the outside, a round piece of brass hollowed almost like a concave burning-glass, or one of those tools wherein they use to grind eye-glasses for telescopes: to this piece of brass, which was not considerably thick, nor above two inches diameter, was fitted a convex piece of the same metal, almost like a gage for a tool to grind glasses in, which had belonging to it a square handle, whereinto as into a socket was inserted a square piece of wood, proceeding from the basis of a square wooden pillar, which we made use of on this occasion instead of our vertical cylinder. By the help of another piece of wood coming from the other basis of the same pillar, the turning-key was joined to this pillar, which was made of such a length, that when the turning-key was forcibly kept down as low as the brass cover it was a part of would permit, the convex piece of metal lately described did depress the concave piece a pretty way, notwithstanding a vigorous resistance of the subjacent spring.

BESIDES these things, a little fine powder of emery was put between the convex and concave pieces of brass, to make them more congruous, and facilitate the motion that was to be made; and there was fastened to the upper part of the turning-key a good wimble, without which we presumed the turning of the key would not produce a sufficient motion; in order to the making of which, it was, after the first trial, judged requisite to have a strong man that was used to exercise his hands and arms in mechanical labours, upon which account we sent for a certain locksmith that was a lusty and dexterous fellow.

ALL things that were thought necessary being thus in readiness, and a mercurial gage being conveyed into the receiver, we caused the air to be diligently pumped out; and then the smith was ordered to turn the wimble, and to continue to lean a little on it, that he might be sure to keep the turning-key from being at all lifted up by the former mentioned spring.

WHILST this man with much nimbleness and strength was moving the wimble, I watched the gage, to observe whether the agitation of the stop-cock, and consequently the engine, did not prejudice the experiment; and for greater caution I caused the pump to be almost all the while kept at work, though that seemed not so necessary.

WHEN the turner of the wimble was almost out of breath, we let in for haste the air at the cover of the receiver by lifting up the turning-key, and nimbly removing the

receiver, we felt the pieces of brass betwixt whom the attrition had been made, and, as we expected, found both of them very sensibly warm.

BUT being willing to confirm the experiment by a second trial, which we hoped might, after the experience taught us by the first, be somewhat better performed, we caused the smith, after he had well refreshed himself with rest and drink, to lay hold of the wimble again, when the gage made it appear that the receiver was well exhausted, so that by further pumping the quicksilver seemed not to be further depressed: and in this second trial the nimble smith played his part so well, the pump in the mean while not being neglected, that when we did as before hastily let in the air and take out the bodies that had been rubbed against one another, they were both of them, especially the uppermost, so hot that I could not endure to hold my hand on either of them, and they did for a considerable time retain a not inconsiderable degree of warmth.

THE same day I caused to be made at the turner's two bodies of wood, for size and shape like those of brass we had just before employed; the upper of these was of hard oak, the other of beech, such a difference between woods, to be heated by mutual attrition, being thought to be an advantageous circumstance: but though the wimble was swiftly turned as before, and that by the same person, nevertheless the wood seemed not to me (for all the by-standers were not of my opinion) to have manifestly acquired any warmth; and yet that there had been a considerable attrition, appeared by the great polish which part of the wood had evidently acquired, which made me suspect that though the wood seemed dry enough, yet it might not really be so, notwithstanding the contrary was affirmed to me. But not being willing to sit down with a single trial, I caused the experiment to be repeated with more obstinacy than before; the effect of which was, that the wood, especially the upper piece of it, was brought to a warmth unquestionably sensible.

## EXPERIMENT XLVI.

### *About the slacking of quick-lime in the exhausted receiver.*

THE several scopes I aimed at in making the following trial are not necessary to be here particularly taken notice of; but one of them may be guessed at by the subsequence of this experiment to that immediately foregoing, and the phenomena of it may be mentioned in this epistle, upon the account of their being exhibited by our engine.

WE took in an evaporating glass a convenient quantity of water, and having conveyed it into a receiver and well drawn out the air, we let down into it by the turning-key a lump of strong lime about the bigness of a pippin, and observed not, that at the first immersion, nor for some while after, there appeared any considerable number of bubbles; but within about  $\frac{1}{4}$  of an hour, as I guessed it, the lime began (the pump, having been and being still plied from time to time) to slack with much violence, and with bubbles wonderfully great, that appeared at each new exsuction, so that the inside of the receiver, though pretty large, was at length lined with lime-water, and a great part of the mixture did from time to time overflow the vessel that had purposely been but little filled; nor did any thing but our weariness put a period to the bubbling of the mixture, whose heat was sensible, even on the outside of the receiver, and which continued considerably hot in the evaporating glass for  $\frac{1}{4}$  of an hour, as I conjectured, after the receiver was removed.

NOTE,



NOTE, That the lime employed about this experiment was of a very good and strong kind, made of hard stones, and not such lime, made of chalk, as is commonly used at London, which probably would not have been strong enough to have afforded us the same phenomenon.

# EXPERIMENT XLVII.

*About an attempt made to measure the force of the spring of included air, and examine a conjecture about the difference of its strength in unequally broad-mouthed vessels.*

THOUGH several of the foregoing trials have sufficiently manifested that the spring of the air in its natural or wonted state hath a force very considerable, and indeed much greater than men seem to have hitherto believed; yet I could not hope by any of these experiments to determine by any known weight how great that force is, so as to conclude that it is equivalent to such a weight, as so many pounds, ounces, &c. and to no more. Wherefore, among the uses I had designed to make of our syringe formerly often mentioned, it was one to try if by the help of that instrument we could determine somewhat near (for no more was to be expected) how much weight a cylinder of uncompressed air included in it, and consequently of the same diameter with the cavity of the barrel, would be able to sustain, or also to lift up.

In order to this trial, 1. we provided a stable pedestal or frame, wherein the syringe might be kept firm and erected; next we also provided a weight of lead shaped like our brass-hoop, or ring formerly described, that by the advantage of its figure it might be made to hang down by strings from the top of the handle of the rammer, and so press evenly enough on all sides, without making the upper part of the instrument top-heavy. 2. We took care to leave between the bottom of the syringe (which was firmly closed with strong cement) and that part of it where the sucker was, a convenient quantity of air to expand itself and lift up the weight, when the air external to that included air should be pumped out of the receiver. And lastly, the handle of the rammer (from which the annular weight lately spoken of depended) was so fastened to the turning-key of the cover of the receiver, that the weight might not compress the air included in the syringe, but leave it in its natural state or wonted laxity, till the air were withdrawn from the receiver.

Exper. V.

BUT notwithstanding all this, when we actually tried the experiment, that happened which I feared. For though by this method the included air would well enough lift up a weight of seven or eight pounds, yet, when the rammer came to be clogged with so considerable a weight as my scope in making the experiment required, the instrument proved not so staunch, but that it was easier for some particles of air to force themselves a passage, and get away between the sucker and the inside of the barrel, than to heave up so great a weight. And yet I have thought fit to relate the experiment thus particularly, because if an exact syringe can be procured, which I fear will be very difficult, but do not think impossible, this seems to be one of the likeliest and least exceptionable ways I know of measuring the force of the air's spring.

BUT despairing to get such a syringe as I desired in the place where I then was, I betought myself of another way, by which I hoped to be able, though not to arrive at an exact knowledge of the full force of the air's spring, yet at least to approach nearer it than I have been able to do by the help of the syringe. For this purpose, considering with myself that if a convenient quantity of air were included in a fine small bladder,

the sides of it would hinder the air from getting away, and the limberness of them would permit the air to accommodate itself and the bladder to the figure of a cylindrical vessel into which it might be put:

WHEREFORE with much ado I procured to be made by a person exercised in turning, a couple of hollow cylinders, whose sides were of a sufficient thickness, that they might resist the pressure of the air to be imprisoned in them, and of such differing breadths, that the first had but one inch in diameter, and the second two; their depths being also unequal, that the one might receive a much larger bladder than the other.

WITH the lesser of these, which was very carefully turned, I made a diligent trial; whose circumstances I cannot now acquaint your lordship with, the paper wherein they were amply recorded having been, with other notes belonging to this continuation, unluckily lost; but the most considerable things in the event were, that it was very difficult to procure a bladder small and fine enough for that little cylinder; and that one which at length we procured, would not continue stanch for many trials, but would after a while part with a little air in the well exhausted receiver, when it was clogged with the utmost weight it could sustain; but whilst it continued stanch we made one fair trial with it, from whence we concluded that a cylinder of air of but an inch in diameter, and less than two inches in length, was able to raise visibly, though but a little, a weight of above ten pounds (I speak of avoirdupoise weights, where a pound contains sixteen ounces). The manner of making this experiment, and the cautions used in judging of it, your lordship may learn by the recital of the subsequent trial; my notes about which were not so unfortunate as those that concerned the former.

See Plate  
VIII. Fig  
2 and 4.

INTO a hollow cylinder of wood of four inches in depth, and two in diameter, furnished with a broad and solid bottom or pedestal to make it stand the firmer, was put a lamb's or sheep's bladder very strongly tied at the neck, on which was put a wooden plug, marked with ink where the edge of the cylinder was contiguous to it: this plug being loaden with weights, amounting to 35 pounds, (the uppermost of which weights was fastened to the turning-key to keep it upright, and to help to raise it at first) the receiver was exhausted till the mark appeared very manifestly above the brim of the cylinder; and then, though the string were by turning the key quite slackened, yet the mark on the plug continued very visible: and when so much air was let into the receiver, as made the weight depress the plug quite beneath the mark, upon the repumping out of the air, the weight was without the help of any turning-key lifted up, and by degrees all the mark on the plug was raised about  $\frac{1}{3}$  above the edge of the cylinder.

WHEREFORE we substituted for a seven pound weight, one that was estimated at 14. (for then we had not a balance strong enough to weigh it with) and using the same bladder we repeated the experiment, only having a care to support a little the uppermost weight by the turning key, till the bladder had attained its expansion; and then the weight being gently let go, depressed not the plug so low, but that we could yet see the mark on it (which yet was all we could do) though that part of the plug where the mark was, were manifestly more depressed than the other.

FOR the clearing up of some particulars relating to this trial, we will subjoin the following notes.

1. THE plug is to be so fitted to the cavity of the cylinder, as easily to slip up and down it, without grating against the sides of it, lest it needlessly increase the resistance of the weight to be raised; and this plug ought to be of a convenient length, as about an inch and a half at least, that it may be the fitter to help to reduce the bladder by compression into a somewhat cylindrical shape, and yet that it may not be thrust in too deep by the incumbent weight; and that the weight might rest more firmly upon it, there was

a broad



a broad and strong ledge made at the top of it, by which it might lean on every side upon the brim of the hollow cylinder.

2. BEFORE the instrument was conveyed into the receiver, the bladder (which ought to be of a just size, and not full blown, and of a fine and limber contexture) was put into the cylinder, and by divers gradual (but not immoderate) compressions was reduced to conform itself as much as might be to the cylindrical shape of the containing vessel, and then the weight being put on, and taken off again, there was a mark (in the form of an horizontally placed arch) made with ink, where the edge of the brim of the hollow cylinder did almost touch the plug. This we thought necessary to do to avoid a mistake; for we must not judge that all the weight that might be raised by our bladder, may pass for the weight sought after by our experiment; since the air in the bladder is by reason of the incumbent weight more compressed than it was before, and consequently its being able to heave up a great weight will not infer that our common air is able in its natural state (as they call it) to exert so great a strength; that weight being only to be looked on as raised or sustained by the uncompressed air, that is raised or sustained when the plug is lifted up to the mark, since till then the spring of the air does but bring it back from its new state of adventitious compression to its natural or wonted laxity.

3. WHEN, after the operation was ended, we took the bladder out of the vessel, it had obtained a form cylindrical enough; and though it could be but two inches in diameter, yet it was so little, as to be but half an inch more long than broad.

THE reason why I chose to have the two cylinders made of the unequal diameters above-mentioned, was to examine, as far as by this way I could, a conjecture I had that the force of the spring of differing cylinders of air to lift up solid weights would, at the very first raising of the weights, be in duplicate proportion to the diameters of their cylinders (those diameters being proportionable to the areas of the plain superficies against which the air does immediately press) without very much considering the inequality that may be between the quantity of the several parcels of air whose pressures are compared. But it is to be remembered, that I said at the very first raising of the weights, because presently after that the quantity of the parcels of air may be very considerable; for as I have shewn in another treatise, two very unequal quantities of air being made by their expansion to possess two equal spaces, the lesser quantity of air must be much more rarefied in proportion than the greater; and consequently, to bring this home to our present argument, though both be lifted up a quarter or half of an inch, the spring of a very little air must be much more weakened than that of a very considerable quantity, and so it cannot continue to lift up its weight as the above-mentioned proportion would (if it were not for this advertisement) seem to require.

4. TAKING then our conjecture in the sense now declared, the success of our trials is agreeable to it, inviting us to conclude that the air in the bladder, which was but two inches in diameter, was able by its pressure to countervail the weight of 42 pounds, which is about four times the weight that we lately observed the spring of a cylinder of air of one inch in diameter to be able to lift up. For though, according to what we have formerly said of a duplicate proportion, 42 pounds seems to be somewhat more than ought to have been lifted up in the cylinder of two inches bore, when that of one inch lifted up not above 10 pounds; yet this disagrees not with the hypothesis, if we consider that the substance of the bladder straightens the cavity of the smaller cylinder in a greater proportion than that of the bigger.

5. THOUGH we have thus (as far as the instruments we were able to procure would assist us) measured the pressure of included air, yet I must not forbear to advertise your lordship, that considering what I formerly observed to you about the weight of an atmosphere

mospherical pillar of an inch in diameter, I cannot but think that if a cylinder or other convenient instrument, exactly tight, can be procured, the spring of an aerial cylinder will appear to be greater than we found it by the foregoing trials; in which I consider that, not to mention the resistance of the bladder itself, the membranous substance that lined the cylinders (though it were very thin and fine) could not but somewhat straighten their cavities, and consequently somewhat (though not much) lessen the diameters of the included aerial cylinders.

6. To all these notes I must add this advertisement, that it may be therefore the more difficult in such trials as ours to ascertain the force of the air's spring, because that air itself when it is concluded, being shut up with the pressure of the atmosphere upon it, it is probable that since that pressure, as we have shewn, is not at all times the same, the spring of the included air will accordingly be varied. And if my memory fail me not, when the lately recited experiments were made, our barometer declared the atmosphere to be somewhat light.

FROM what has been hitherto delivered, this may result, that it is likely that the spring of an aerial cylinder an inch broad may be able to sustain, if not raise a pretty deal more than ten pounds weight; and that the past trials, without determining that the air can raise no more in them than it did, do at least prove that it can raise up as much weight as we have related, since we actually found it to do so.

#### EXPERIMENT XLVIII.

*About an easy way of making a small quantity of included air raise in the exhausted receiver 50 or 60 pounds or a greater weight.*

I WOULD very willingly have further prosecuted the foregoing trials, to see how far the lately proposed conjecture or hypothesis would hold; but was hindered by the want of receivers tall and capacious enough to contain the weights that such an attempt required; but remembering that there were not any experiments made in our engine, that appeared more strange to the generality of spectators, and served more to give them a high opinion of the air's spring, than those wherein they saw solid bodies actually lifted up by it; and remembering that I had lying by me a brass vessel which had been bespoken for another experiment, for which the workmen had not made it fit, I thought it not amiss to employ it about making a trial very easy, and yet fit to be shewn to strangers, to convince them that the spring of the air is a much more considerable thing than they imagined.

See Plate  
VIII. Fig.  
4.

WE took then a brass vessel made like a cylinder, and having one of its orifices exactly covered over with a flat plate very firmly fastened to it, the other orifice being wide open. The depth of this vessel was four inches, and the diameter should have been precisely, but wanted about a quarter of an inch of, four inches. To this hollow cylinder we fitted a wooden plug, like one of those described in the foregoing experiment, save that it was not quite so long, and that it was furnished with a rim or lip, which was purposely made of a considerable breadth, that it might afford a stable basis to the weight that should lean upon it; and then taking a middle-sized and limber bladder, strongly tied at the neck, but not near full blown, we pressed it by the help of the plug into the cylinder to make it the better accommodate itself to the figure of it; then taking notice by an inky mark how much the plug was extant above the orifice of the vessel, we laid the weights upon the plug, whole rim or lip hindered it

See Plate  
VIII. Fig.  
3.

from



from being depressed too deep into the cavity of the vessel; and having conveyed them into the receiver, we found as we expected, that if we had loaded the plug but with a single weight, as to avoid trouble and the danger of breaking the glass we usually thought fit to do, though that were a common half-hundred weight, which you know amounts to 56 pounds, it would very quickly be manifestly heaved up by the spring of the included air. For confirmation of more than which, I shall subjoin the ensuing trial, as I find it recorded among my loose notes.

THE weight that was lifted up by the bladder in the cylinder four inches broad, was 75 pounds; this weight was lifted up till the wooden plug disclosed the mark that was to shew the height at which the air kept the said plug before it was compressed; disclosed it, I say, visibly at the fifth exsuction, and at the seventh that mark was  $\frac{1}{8}$ , or rather  $\frac{3}{8}$  above the edge of the cylinder. In the gage where the mercury in the open air was wont to stand about  $\frac{1}{8}$  above the uppermost glass-mark, it was depressed till it was  $\frac{1}{8}$  below the second mark. When the air was let in, it was a pretty while before the weight did manifestly begin to subside; the bladder being taken out, and the place it had possessed in the cylinder being supplied with a sleeve, or some such thing, and the weight laid again upon the plug, we found that at twenty-four exsuctions the mercury was depressed to the lowest mark of the gage; and it was the thirty-fourth or thirty-fifth exsuctions before the receiver appeared to be so exhausted, as to put an end to the sinking of the mercury, which was then above  $\frac{1}{8}$  beneath the lowest mark.

YOUR lordship will easily believe that most of the spectators of such trials thought it somewhat strange to see a small quantity of air, which was not only uncompressed in the bladder, but did not near fill it, and left it very soft and yielding to the least touch, lift up so easily by its rare spring such great weights as endeavoured to oppress it. But this not being any thing near a sufficient trial, how far the conjecture or hypothesis formerly proposed will hold, I thought fit to make the utmost trials the tallest receivers I could procure would admit; and having caused leaden weights to be purposely cast flat like cheeses, and as broad as we could conveniently put into the receiver, that by the advantage of this shape we might be able to pile up the more of them, without much danger that any of them should be shaken down; we laid divers of them one upon another, and then the upper part of the receiver growing too narrow to admit more of them, we added a less broad weight or two; and then exhausting the receiver till we perceived by the gage that the air was manifestly withdrawn, we found, as near as we could measure by the help of a mark and a pair of compasses, that the plug was so far raised, as that it was concluded that the elevation would have been much greater, if the included air being put upon so great a conatus, had not found it easier to produce some leak at the neck of the bladder, than to lift up so great a weight, which by our reckoning came to about 100 pounds of 16 ounces to the pound. But this last experiment, for want of some requisite accommodations, we were hindered from repeating and promoting; though the above-mentioned hypothesis made me presume that a far greater weight might this way have been raised, if the bladder had been stanch, and the receiver high enough.

I NEED not tell your lordship that if a larger bladder be employed and included in a brass vessel of a sufficiently wide orifice, a far greater weight may be lifted up by the spring of the internal air; but yet it will not be amiss to give your lordship on this occasion this advertisement, which may be fit to be taken notice of on divers others; that care must be had not to make receivers that ought to be well emptied, too large, and especially too wide at the orifice; for otherwise they will be exposed to so great a pressure of the atmosphere, that they need be of an extraordinary strength to resist it; and even

receivers.

receivers that seemed thick enough proportionably to their bulk, and which held out very well till the close of the operation, yet when they came to be very diligently exhausted, they did, by reason of the wideness of their orifices, begin to crack at the bottom.

## EXPERIMENT XLIX.

Viz: the  
XXXVI.

IN one of my published experiments I long since told your lordship that when I endeavoured, by the help of a sealed bubble, weighed in an exhausted receiver, to compare the gravity of air and water, I was hindered by the casual breaking of the glass from completing the experiment; wherefore I afterwards thought fit to repeat the trial; and though when I had done so twice or thrice, having given away the large receiver I had made use of about them, and not being able ever since to procure a good one that was capacious enough for the tender scales I thought so nice an experiment required, I did not prosecute that attempt so far as I intended; yet this very difficulty I met with to procure the requisites of making the trial, invites me to subjoin the two following notes, which I find among my loose papers.

April the  
19, 1662.

WE weighed a bubble in the receiver, which we found to weigh above half a grain heavier, when much of the air was exhausted, than when it was full. Afterwards we took out this sealed bubble, and weighing it, found it to weigh 68 grains and a half; then breaking off the small tip of it under water, we found that the heat by which it was sealed up, had rarefied its included air, so that it admitted 125 grains of water, for the admitted water and glass weighed  $193\frac{1}{2}$  grains. Then filling it full with water, we found it to contain in all 739 grains of water, for it weighed  $807\frac{1}{2}$  grains; whence it is evident that the difference between the weight of water and air was less than 1228 to 1.

WE weighed in the receiver a bubble, the glass of which weighed 60 grains; the air that filled it weighed *in vacuo*  $\frac{37}{32}$  of a grain; the water that filled it weighed  $720\frac{1}{4}$  grains; so that by this experiment the proportion of the weight of air to water is as one to  $853\frac{17}{32}$ .

May 26,  
1662.

THE trials mentioned in these notes, though they were too few for me to acquiesce in, yet being made in a new way, and which has some advantages above those that have been hitherto employed to weigh the air, may yet serve to keep us from the contrary extremes that have not been avoided by such eminent mathematicians as *Galileo* and *Ricciolus*; the former of which makes water to be about 400 times as heavy as the air; and the latter, whose conjecture is much remoter from the truth, 10,000 times heavier.

BUT it is so desirable a thing, and may prove of such importance, to know the proportion in weight betwixt air and water, that I shall not scruple to acquaint your lordship with an attempt or two, that I made to discover it by another way; for though at first sight this experiment may seem to be the same with one published a pretty while ago in the learned *Schottus* his *Mechanica hydrolico-pneumatica*; yet your lordship will easily perceive this difference between them; that whereas the industrious author of that experiment contents himself to shew, by the diminution of the weight of a glass, when the air has been drawn out of it, that the air, before it was drawn out, was not devoid of gravity; the following trial does not only perform the same thing, and by a super-added circumstance confirm the truth to be thereby proved, but it endeavours also to shew the proportion in gravity betwixt the air and water. The trials themselves were registered among my *Adversaria* as follow:

A small



A SMALL receiver being exhausted of air by the engine, and counterpoised whilst it continued so, the stop-cock was turned, and the air re-admitted, which made it weigh 36 grains more than it did before; and to prevent jealousies, we caused it to be applied the second time to the engine, by which the air being emptied once more, the glass was put into the other scale of the former balance, and so counterpoised; and then the external air being re-admitted (which rushed in as formerly with a whistling noise) there was found 36 grains or better requisite to restore the balance to an æquilibrium.

We took a small glass receiver fitted with a stop-cock, and having exhausted it of the air, and counterpoised it, and let in the outward air, we found the weight of the vessel to be increased by that admission 36 grains. This done, we took the receiver, after having well counterpoised it, out of the scale, and having applied it the second time to the engine, we once more withdrew the air, and then turning the stop-cock to keep out the external air, we took care that none of the cement employed to join it to the engine, should stick to it, as we had diligently freed it from adherent cement before we last applied it to the engine. Then weighing it again, we found it to weigh either 35 or 36 grains (but rather the former) heavier than it did when it was last counterpoised in the same balance. This being also done, we immersed the stop-cock into a basin of fair water, and let in the liquor, that we might find how much water would succeed in place of the air we had drawn out. When no more water was impelled in, we turned the stop-flock once more to keep it from falling out, and then weighing it in the same scales (after we had wiped the stop-cock, that no water might stick to it on the outside) we found the water (without computing the vessel) to weigh 47 ounces, 3 drachms, and 6 grains, which divided by 35 grains (which I took to be the weight of the air that was equal in bulk to this water that succeeded it) the quotient was (wanting a very little) 650 grains, for the proportion of the weight between air and water of the same bigness, at the time when the experiment was made; which circumstance I therefore take notice of, because the atmosphere appeared by the baroscope (wherein the mercury stood then at 29 inches and three quarters) to be very heavy; which made me the less wonder to find this proportion not so great, as at other times I had observed it to be between water and air in point of weight; though I suspected, that because this odd experiment cannot be nimbly dispatched, some little air may have got in at the stop-cock, besides the air that disclosed itself in numerous bubbles in the water that was admitted, where, though it lay in such small particles as not to be discerned before, yet these particles, by this opportunity to expand themselves, extricated themselves from the water, and by getting together might somewhat resist the ingress of more; which is a difficulty whereto the measuring the proportion between water and air in a heated æolipile is liable. But the stealing in of any air before the water was let in, is mentioned but as a suspicion.

YOUR lordship may perhaps think it somewhat strange that I should present you trials whose events do not so well agree together as perchance you expected; but this very disagreement was one of the motives that induced me to acquaint you with them; for all those comprized in these experiments being made faithfully, and not without at the least an ordinary diligence, as they seem to make it probable, that one may without any great error estimate the proportion of our English air to water to be as one to some number between 600 and 1100; so it is not to be expected that the proportion, whatever it be that should be pitched upon, should be accurate and stable. For though learned men seem to have hitherto taken it for granted that it may suffice once for all diligently to investigate the proportion betwixt those two bodies, yet not only that I am apt to believe that a determinate quantity of air (as a pint or quart) may be unequally



heavy in distant countries, and even in differing places of the same country; but what I have taken notice of in the 17th of the printed experiments, and afterwards frequently observed of the great inequalities of the weight of the atmosphere, inclines me to think that in the self same place two experiments may be made with the same instruments and equal diligence, and yet the weights of the air may be found differing enough; which may keep your lordship from much wondering, that in the 36th printed experiment, made when I had the variations of the atmosphere's gravity in my eye, I found the air to be less ponderous in reference to water, than in these latter trials. But of this I hope I shall, if God permit, make further trial with the same vessels, at times when I shall perceive by the baroscope that the gravity of the atmosphere is very great and very small; and I wish the curious would make the like trials in other regions. I do not forget that not only the school-philosophers, but most of the moderns, deny that air hath any weight in air, no more than water in water; but having \* elsewhere declared and explained my sense about this received opinion, I shall not here spend any of the little time I have remaining to justify my dissent; for which your lordship may find sufficient grounds in the newly related experiments, especially if you please to consider, that though the opinion I disallow have been chiefly and generally grounded upon some arguments supposed to evince that water has no weight in water, I have † elsewhere shewn those proofs not to be cogent, and taught a practical way of weighing water in water with a pair of ordinary scales ‡.

#### EXPERIMENT L.

*About the disjoining of two marbles (not otherwise to be pulled asunder without a great weight) by withdrawing the pressure of the air from them.*

IN our formerly published experiments about the air ||, I did, if I misremember not, acquaint your lordship with an attempt I had made to make a couple of coherent marbles fall asunder by withdrawing the air from them. But though I then esteemed that their cohesion depended upon the pressure of the air, yet not being at that time furnished with all the accommodations requisite to make an experiment not easy to be performed succeed, I thought fit, when I had afterwards opportunity, to prosecute what I then began, and add some circumstances that I could not then make trial of, and yet whose success will not, I presume, be unwelcome, since it supplies us with no less than matters of fact; whence we may argue this, that this experiment of coherent marbles (which not only the Aristotelian plenists have much triumphed in, but which some recent favourers of our hypothesis have declared themselves to be troubled with) is not only reconcileable to our doctrine, but capable of being made a confirmation of it; notwithstanding what has lately been published (upon the supposition of a case, which at first blush may seem somewhat of a kin to our experiment) by a very learned § writer, to whose objection

\* In the Hydrostatical Paradoxes.

† In an Appendix to those Paradoxes.

‡ This method was omitted in the English edition of the newly mentioned Appendix, but not in the Latin Version.

|| Experiment XXXI. See also the cause of this phenomenon discoursed of in the author's History of Fluidity and Firmness.

§ Dr. H. M. in the second chap. of the second book of the new edition in folio, of his *Antidote against Atheism*.



against our hypothesis, though as well confidently as very civilly proposed, an answer may in due place, if your lordship desire it, be returned.

We took two flat round marbles, each of them of two inches and about three quarters in diameter, and having put a little oil between them to keep out the air, we hung at a hook fastened to the lowermost a pound weight, to surmount the cohesion which the tenacity of the oil and the imperfect exhaustion of the receiver might give them; then having suspended them in the cavity of a receiver, at a stick that lay horizontally a-crois it; when the engine was filled and ready to work, we shook it so strongly, that those that were wont to manage it, concluded it would not be near so much shaken by the operation. Then beginning to pump out the air, we observed the marble to continue joined, until it was so far drawn out, that we began to be diffident whether they would separate; but at the 16th suck, upon the turning of the stop-cock (which gave the air a passage out of the receiver into the pump) the shaking of the engine being almost, if not quite over, the marble spontaneously fell asunder, wanting that pressure of the air that formerly had kept them together: which event was the more considerable, not only because they hung parallel to the horizon, but adhered so firmly together when they were put in, that having tried to pull them asunder, and thereby observed how close they stuck together, I foretold it would cost a good deal of pains so far to withdraw the air, as to make them separate; which conjecture your lordship will the less wonder at, if I add that a weight of 80 and odd pounds, fastened to the lowermost marble, may be drawn up together with the uppermost, by virtue of the firmness of their cohesion.

N. B. THIS is not the only time that this experiment succeeded with us; for sometimes, when they were not so closely pressed together before they were put in, the disjunction was made at the 8th suck, or sooner, and we seemed to ourselves to observe, that when we hung but half a pound weight to the lower marble, it required a greater exhaustion of the receiver to separate them, than when we hung the whole pound.

AFTER having proceeded thus far with the instruments we then had, meeting with an artificer that was not altogether unskilful, we directed him to make (what we wanted before in that place) such a brass-plate to serve for a cover or cap to the upper orifice of receivers open at the top, as we have divers times had occasion to mention already in giving accounts of some of the foregoing trials; by the help of which contrivances we prosecuted the newly related experiment much farther than we could do before, as may appear by the following account.

We fastened to the lowermost of the two marbles a weight of a very few ounces (for I remember not the precise number) and having cemented the capped receiver with the marbles in it, as before to the pump, we did by a string, whereof one end was tied to the bottom of this turning-key, and the other to the uppermost marble, and which (string) passed through the crank or hook belonging to the brass-cover; we did, I say, by the help of this string, and by turning round the key, draw up the superior marble, and by reason of their coherence the lowermost also, together with the weight that hung at it: by which means being sure that the two marbles stuck close together, we began to pump out the air that kept them coherent; and after a while, the air being pretty well withdrawn, the marbles fell asunder. But we having so ordered the matter that the lowermost could fall but a little way beneath the other, we were able by inclining and shaking the engine to place them one upon another again, and then letting in the air somewhat hastily, that by its spring it might press them hard together, we found the expedient to succeed so well, that we were not only able by turning the abovementioned cylindrical key, to make the uppermost marble take up the other, and the annexed

weight; but we were fain to make a much more laborious and diligent exhaustion of the air to procure the disjunction of the marbles this second time, than was necessary to do it at the first.

AND for further prevention of the objections or scruples that I foresaw some prepossessions might suggest, I thought fit to make this further trial; that when the marbles were thus asunder, and the receiver exhausted, we did, before we let in the air, make the marbles fall upon one another as before; but the little and highly expanded air that remained in the receiver having not a spring near strong enough to press them together, by turning the key we very easily raised the uppermost marble alone, without finding it to stick to the other as before; whereupon we once more joined the marbles together, and then letting in the external air, we found them afterwards to stick so close, that I could not without inconvenience strain any farther, than I fruitlessly did, to pull them fairly asunder; and therefore gave them to one that was stronger than I, to try whether he could do it, which he also in vain attempted to perform.

AND now, my lord, though I had thoughts of adding divers other experiments to those I have hitherto entertained you with, yet (upon a review) finding these to amount already to fifty, I think it not amiss to make a pause at so convenient a number; and the rather, because an odd quartianary distemper that I slighted so long, as to give it time to take root, is now grown so troublesome, that I fear it may have too much influence upon my style; which apprehension obliges me as well to avoid abusing or distressing your lordship's patience, as to allow myself some seasonable refreshment, to reserve the mention of the designed additions until they can with less trouble to us both be presented you by,

My dear lord,

Your lordship's most humble servant, and affectionate uncle,

Oxford, March 24.  
1667.

ROBERT BOYLE.



NOTES,



# NOTES, &c.

ABOUT THE

ATMOSPHERES of CONSISTENT BODIES here below.

SHEWING,

That even hard and solid Bodies (and some such as one would scarce suspect) are capable of emitting EFFLUVIA, and so of having ATMOSPHERES.

## ADVERTISEMENT.

**H**E that shall take the pains to peruse the following paper, will easily believe me, when I tell him that it was not designed to come abroad with the experiments, in whose company it now appears: but the stationer earnestly representing, that divers experiments being reserved by me for another occasion, the remaining ones alone would not give the book a thickness any thing proportionable to its breadth; I consented, at his solicitation, to annex to them the following observations, because of some affinity between the small atmospheres of lesser bodies, and the great atmosphere that surrounds the terrestrial globe; in which the other that do at least help to compose it, are lost and confounded, as brooks and rivers are in the ocean: and to save the reader the pains of making guesses to what kind of writing the ensuing discourse may belong, I shall here intimate, that it is dismembered from certain papers about occult qualities in general, which make part of the notes I long since designed, and also partly published, about the origin of qualities, of which notes those that concerned effluvia, being the most copious, I referred them to four general heads; whereof the first only is treated of in the following discourse, the others being withheld, as having not affinity enough with the atmosphere to accompany this, whereon they have no such absolute dependence, but that they may well enough spare it: and I make the less scruple to let it appear without them, because the inducements already mentioned are not a little strengthened by this superadded consideration, that the following notes may give light to several of the observations I have made, of some less heeded phenomena of the alterations of the air, in case they be allowed to enter into the Appendix to this Continuation.

*Of*

## Of the ATMOSPHERES of Consistent Bodies.

THE school-philosophers and the vulgar, in considering the more abstruse operations and phenomena of nature, are wont to run into extremes, which, though opposite to one another, do almost equally contribute to keep men ignorant of the true causes of those effects they admit: for the vulgar, being accustomed to converse with sensible objects, and to conceive grossly of things, cannot easily imagine any other agents in nature than those that they can see, if not also touch and handle; and as soon as they meet with an effect that they cannot ascribe to some palpable, or at least sensible efficient, they are, and stick not to confess themselves utterly at a loss: and though the vulgar of philosophers will not acknowledge themselves to be posed by the same phenomena with the vulgar of men, yet in effect they are so; but the school-philosophers, on the contrary, do not only refuse to acquiesce in sensible agents, but, to solve the more mysterious phenomena of nature, nay and most of the familiar ones too, they scruple not to run too far to the other side, and have their recourse to agents that are not only invisible but inconceivable, at least to men that cannot admit any save rational and consistent notions: they ascribe all abstruse effects to certain substantial forms, which, however, they call material, because of their dependence on matter, they give such descriptions to, as belong but to spiritual beings; as if all the abstruser effects of nature, if they be not performed by visible bodies, must be so by immaterial substances; whereas betwixt visible bodies and spiritual beings there is a middle sort of agents, invisible corpuscles; by which a great part of the difficult phenomena of nature are produced, and by which may intelligibly be explicated those phenomena, which it were absurd to refer to the former, and precarious to attribute to the latter. Now, for method's sake, I will refer the notes that occur to me about effluvioms to four heads; whereof the first is mentioned in the title of this paper, and each of the other three shall be successively treated of in as many distinct ones.

THAT fluid bodies, as liquors, and such as are manifestly either moist or soft, should easily send forth emanations, will, I presume, be granted without much difficulty; especially considering the sensible evaporation that is obvious to be observed in water, wine, urine, &c. and the loose contexture of parts that is supposed to be requisite to constitute soft bodies (as flowers, balsams, and the like); but that even hard and ponderous bodies, notwithstanding the solidity and strict cohesion of their component parts, should likewise emit steams, will to many appear improbable enough to need to be solemnly proved.

WHETHER you admit the atomical hypothesis or prefer the Cartesian, I think it may be probably deduced from either, that very many of the bodies we are treating of may be supposed exhaleable as to their very minute parts; for, according to the doctrine of *Lucippus*, *Democritus*, and *Epicurus*, each indivisible particle of matter hath essentially either a constant actual motion or an unlooseable endeavour after it; so that though it may be so complicated in some concretions with other minute parts, as to have its avolation hindered for a while; yet it can scarce otherwise be, but by this incessant endeavour of all the atoms to get loose, some of them should from time to time be able to extricate themselves and fly away: and though the Cartesians do not allow matter to have any innate motion, yet, according to them, both vegetables, animals, and minerals consist of little parts so contexted that their pores give passage to a celestial matter; so that this matter continually streaming through them, may well be presumed to shake  
the



the corpuscles that compose them; by which continued concussion now some particles, and then others, will be thrown and carried off into the air, or other contiguous body fitted to receive them: but though by these and perhaps other considerations, I might endeavour to shew *à priori*, as they speak, that it is probable consistent bodies themselves are exhaleable, yet I think it may be as satisfactory, and more useful, to prove it *à posteriori*, by particular experiments and other examples.

THAT then a dry and consistent form does not necessarily infer, in the bodies that are endowed with it, an indisposition to send forth steams, which are, as it were, little colonies of particles, is evident, not only in the leaves of damask-roses, whether fresh or dried, as also in wormwood, mint, rue, &c. but in amber-gris, musk, storax, cinnamon, nutmegs, and other odoriferous and spicy bodies: but more eminent examples to our present purpose may be afforded us by camphire and volatile salts, such as are chemically obtained from hartshorn, blood, &c. for these are so fugitive, that sometimes I have had a considerable lump of volatile salt (either of fermented urine, or of hartshorn) fly away by little and little out of a glass that had been carefully stopped with a cork, without leaving so much as a grain of salt behind it: and as for camphire, though by its being uneasy to be powdered, it seem to have something of toughness or tenacity in it, yet I remember, that having for trial's sake counterpoised it in nice scales, even a small lump of it would in a few hours suffer a visible loss of its weight by the avolation of strongly scented corpuscles, and this, though the experiment were made both in a north window and in winter.

BUT I expect you should require instances of the effluvioms of bodies of a close or solid texture; wherefore I proceed to take notice, that amber, hard-wax, and many other electrical bodies do, when they are rubbed, emit effluvioms: for though I will not now meddle with the several opinions about the cause and manner of electrical attraction, yet besides that almost all the modern naturalists that aim at explicating things intelligibly, ascribe the attraction we are speaking of to corporeal effluxes; and besides that I shall ere long have occasion to shew you, that there is no need to admit with Cartesius, that because some electrical bodies are very close and fixed, what they emit upon rubbing is not part of their own substance, but somewhat that was harboured in their pores: besides these things, I say, I have found that many electrical bodies may by the very nostrils be discovered, when they are well rubbed, to part with store of corpuscles, as I have particularly, but not without attention, been able to observe in amber, resin, brimstone, &c.

I KNOW not, whether it will be worth while to take notice of the great evaporation I have observed, even in winter, of fruits, as apples, and of bodies that seem to be better covered, as eggs; which, notwithstanding the closeness of their shells, did daily grow manifestly lighter and lighter; as I observed in them, and divers other bodies, that I kept long in scales, and noted their decrements of weight: but perhaps you will be pleased to hear, that having a mind to shew how considerable an evaporation is made from wood, I caused a thin cup, capable of holding about a pint or more, to be turned of a wood that was chosen by the turner as solid and dry enough, though it were not of the closest sort of woods, such as are *lignum vitæ* and box; and as I caused the shape of a cup to be given it, that it might have a greater superficies exposed to the air, and consequently might be the fitter to emit store of steams into it; so the success did not only answer my expectation, but exceed it: for though the trial were made some time in winter, there was so thick and plentiful an evaporation made from the cup, that I found it no easy matter to counterpoise it; for whilst grains were putting into the opposite scale, to bring the tender balance to an equilibrium, the copious avolation of invisible



ble steams from the wood (which had so much of superficies contiguous to the air) would make the scale that held it sensibly too light. And I remember, that for further satisfaction, being afterwards in a city where there were both good materials and workmen, I ordered to be made a bowl about the same bigness with the former, of well seasoned wood, which being suspended in the chamber I lay in (which circumstance I therefore mention, because the weather and a little physic I had taken obliged me to keep a fire there) it quickly began manifestly to lose of its weight; and though the whole cup wanted near two drachms of near two ounces, yet in twelve hours, *viz.* from ten of the clock in the morning to the same hour at night, it lost about 40 grains (for it was above 39): but of such experiments and the cautions belonging to them, I may elsewhere speak farther.

It were not difficult for me to multiply instances of the continual emanation of steams from vegetable and animal substances; but I am not willing to enlarge myself upon this subject, because I consider that there are other bodies which seem so much more indisposed to part with effluvia, that a few instances given in such may evince what I would prove, much more than a multitude produced in other bodies: and since I consider that those substances are the most unlikely to afford effluvia that are either very cold or very ponderous, and very solid and hard, or very fixed; if I can shew you that neither of these qualifications can keep a body from emitting steams, I hope I shall have made it probable, that there is no sort of bodies here below that may not be thought capable of affording the corporeal emanations we speak of.

AND first I remember, that I have not only taken eggs, and in a very sharp winter found them, notwithstanding the coldness of the air where I kept them, to grow sensibly lighter, in a faithful pair of scales, in not very many hours; but because ice is thought the coldest visible body we know, I thought fit to shew, that even this body will lose by evaporation; for having counterpoised a convenient quantity of ice in a good balance, and forthwith exposed it therein to the cold air of a frosty night, that the evaporations should be from ice not from water, I found the next morning, that though the scale, wherein the ice were put, was dry, which argued as well as the coldness of the weather, that the exposed concretion had not thawed; yet I found its weight to be considerably diminished, and this experiment I successfully made in more than one winter, and in more than one place; and it is now but a few days since, exposing not long before midnight less than two ounces of ice in a good balance to a sharply freezing air, I sent for it before I was up in the morning; and though by the dryness of the scales, the ice, that was in one of them, appeared not to have thawed, yet it had lost about ten grains of its former weight; so that here the evaporation was made in spite of a double cold, of the ice and of the air.

I SHOULD now proceed to the mention of ponderous and solid bodies; but before I do so it may be expedient to give you notice, that, to make the proof of what I have proposed more satisfactory and more applicable to our future purposes, I shall forbear to give you any examples of the exhalations of bodies, where so potent an agent as the fire is made to intervene.

BUT though I purposely forbear to insist on such examples, yet it may not be amiss to intimate, that in explicating some occult qualities, even such exhalations as are produced by the help of the fire, may be fit to be taken into consideration, as we may hereafter have occasion to shew; and therefore we may observe, in general, that the fire is able to put the parts of bodies into so vehement a motion, that except gold, glass, and a very few more, there are not any bodies so fixed and solid, that it is not thought capable to dissipate either totally or in part: it is known to those that deal in the



the fusion of metals, that not only lead and tin, but much harder bodies will emit copious and hurtful steams; and there are some kinds of that iron which our smiths call cold-share iron, about whose smell, whilst it was red hot, when I made inquiry, the ingeniousest smith I had then met with told me, that he had found it several times to be so strong and rank that he could scarce indure to work with his hammer those parcels of metal whence it proceeded: and even without being brought to fusion, not only brass and copper will, being well heated, become strongly scented, but iron will be so too, as is evident by the displeasing smell of many iron stoves: and on this occasion I might not impertinently add here a trial we made to observe, whether the steams of iron may not be made, though not immediately visible, yet perceptible to the eye itself, though the metal had not a red, much less a white heat; but having elsewhere related it at large, in a discourse you may command a sight of, I shall rather refer you to it than lose the time it would take up to transcribe it.

THESE things premised, I proceed now to the mention of ponderous bodies; and concerning them, to represent, that if you will admit, what almost all the corpuscularians assert, and divers of the peripateticks do not now think fit to deny, that the magnetical operations are performed by particles issuing forth of the body of the loadstone or other magnetical agent; I shall not need to go far for an instance to our present purpose, since I have hydrostatically found that some loadstones (for I have found those minerals very differing in gravity) are so ponderous as to exceed double the weight of flints or other stones of the same bulk.

BUT not to insist on loadstones, stone-cutters will inform you, as they did me, that black marble, and some other solid and heavy stones, will, upon the attrition they are exposed to when the workmen are polishing them, especially without water, emit, and that without the help of external heat, a very sensible smell; which I found to be much more strong and offensive, when, to make it so, I had the curiosity to cause a piece of solid black marble to have divers fragments struck off from it with a chisel and a hammer; for the strokes succeeding one another fast enough to make a great concussion of the parts of the black marble (for in white, which is not so solid, the trial will not succeed well) there quickly followed, as I expected, a rank unpleasant smell; and you will grant me, I know, that odours are not diffused without corporeal emanations. I remember also, that having procured some of those acuminate and almost conical stones that pass among the vulgar for thunder-stones, by rubbing them a little one against the other, I could easily, according to my expectation, excite a strong sulphureous stink. I have also tried upon a certain mineral mass that was ponderous almost as a metal, but to me it seemed rather an unusual kind of marcasite, that I could in a trice, without external heat, make it emit more strongly scented exhalations than I could contentedly endure; to which I shall add this example more, that having once made a chemical mixture of a metalline body and coagulated mercury, which you will believe could not but be ponderous, though this mixture had already endured as violent a fire as was necessary to bring it to fusion, in order to cast it into rings; yet it was so disposed to part with corporeal effluxes, that a very ingenious person that practised physick, and was there when I made it, earnestly begged a little of it of me for some patients troubled with distempers in the eyes and other parts remote enough from the hand; which he affirmed himself to have very happily cured, by making the patient wear a ring of this odd mixture, or wearing a little of it as an appenium near the disaffected part. If you make a *vitrum Saturni* with a good quantity of minium in reference to the sand or crystal, which it helps to bring to fusion, you shall have a glass exceeding ponderous, and yet not devoid of electricity: and I remember, that having sometimes caused brass itself to be turned



like wood, that I might try whether so great though invisible a concussion of all the parts would not throw off some steams that might be smelled, I was not reduced to forego my expectation; but yet because it was not fully answered, and because also there is great difference of brasse upon the score of the *lapis calaminaris*, whereof, together with copper, it is made, I enquired of the workman who used to turn great quantities of brasse, whether he did not often after find it more strong; and he informed me that he did, the smell being sometimes so strong as to be offensive to strangers that came to his shop and were not used to it.

I PROCEED now to the effluvioms of solid and hard bodies; of which, if most of our corpuscularian philosophers and divers others be not much mistaken, I may be allowed to give instances in all electrical bodies, which, as I have already noted, must according to their doctrine be acknowledged to operate by substantial emanations: now among electrical bodies I have observed divers that are of so close a texture that aqua fortis itself, nor spirit of salt, will work upon them, and to be so hard, that some of them will strike fire like flints: of the former sort I have found divers gems which I named in my notes about electricity; and even the cornelian itself, which I found to attract hairs, though it be thought to be of a much slihter texture than precious stones, did yet resist aqua fortis, as I tried in a large ring brought out of the East-Indies, which I purposely broke, and reduced some part of it to powder, that I might make these and some other trials with it: rock crystal also, though it have a very manifest attractive virtue, as they call it, I have yet found it so hard as to strike fire rather better than worse than ordinary flints: and to shew that no hardness of a body is inconsistent with its being electrical, I shall add, that though diamonds be confest to be the hardest bodies that are yet known in the world, yet frequent experience has assured me, that even these, whether raw or polished, are very manifestly and sometimes vigorously enough electrical.

AND to let you see that I need not to have recourse to this kind of bodies, to prove that very solid ones are capable of effluvia, I will, to what I have formerly noted about the odour of black marble, subjoin two or three examples of the like nature.

THE first shall be taken from a sort of concretions very well known in divers parts of Italy by the name of cugoli, because of the great use that is made of it by the glassmen; these concretions, you will easily believe, are very hard, as other minerals of that sort are wont to be; and yet being invited by my conjectures about the atmospheres of bodies to try them by rubbing them one against the other, I found, as I expected, that they afforded not only a perceptible but a very strong smell, which was far from that of a perfume.

AND this brings into my mind, that having met with some stones cut out of human bladders, whose texture was so close that I could not with corrosive menstrums make any sensible solution of one, whereon I made my trial, though, to facilitate the liquors operation, part of it were reduced to fine powder; yet by a little rubbing of one of these so closely contexted stones, it would presently afford a rank smell, very like the stink of stale urine.

I REMEMBER I have caused iron to be turned with a lath, to examine, whether by the internal commotion that would by that operation be produced in the corpuscles of the metal, even that solid as well as ponderous body would not become capable of being smelled; and though by reason of the nature of that parcel of iron whereon we made our trial, or some accidental disposition which was at that time (being winter) in my organs of smelling, the odour seemed to me but very faint; yet upon the inquiry I made of the artificers, whether in turning greater pieces of iron they did not find the smell stronger;



stronger; they told me that they often found it very strong, and sometimes more so than they desired.

AND this brings into my mind what I have carefully observed in grinding of iron; for there are many grindstones so qualified, that in case iron instruments be held upon the stone, whilst it is nimbly turned under it, though the water that is wont to be used on such occasions stifles, if I may so speak, the smell, and keeps it from being commonly taken notice of; yet if you purposely cause, as I remember I have done, the use of water to be forborn, your success will not be like mine, if you do not find that store of foetid exhalations will be produced: and though it be not always so easy to discern by the smell from which of the two bodies they issue, or whether they proceed from both, yet it seems probable enough that some of the steams come from the iron, and it is more than probable that if they proceed not from that metal, they must from a body that is so hard as to be able to make impressions, in a trice, upon iron and steel themselves.

THE last example I shall name under this head, is furnished me by *marcasites*, some of which would, after a short concussion without external heat, be made to exhale for a pretty while together a strong sulphureous odour, and yet were so hard, that when struck with a steel-hammer (which would not easily break them) they afforded us such a number of sparks as appeared strange enough; and it is known that it is from their disposition to strike fire (which yet I dare not attribute to all sorts of *marcasites*) that this kind of mineral is, by a name frequently to be met with in writers, called *pyrites*: and in this example we may take notice, that a body capable of being the source of corporeal emanations may be at once both very solid and very ponderous.

It remains now that I manifest, that even the fixedness of bodies is not incompatible with their disposition to emit effluvioms.

I MIGHT alledge on this occasion that the *regulus* of antimony and also its glass, though they must have endured fusion to attain their respective forms; yet they will without heat communicate to liquors antimonial expirations, with which those liquors being impregnated, become emetick and purgative; I might also add, that divers electrical bodies are very fixed in the fire, and particularly that crystal, as we have more than once tried, will endure several ignitions and extinctions in water, without being truly calcined, being indeed but cracked into a great multitude of little parts; but because the above named antimonial bodies will after a while fly away in a strong fire, and because the effluvioms of crystal are not so sensible as those which can immediately affect our eyes or nostrils, I will here subjoin one instance, such as I hope will make it needless for me to add any more, it being of a body which must have sustained any exceeding vehement fire, and is looked upon by most of the chemists as more undestroyable than gold itself; and that is glass, which is able, as you know, to endure so great a brunt of the fire, that you did not perhaps imagine I should of all bodies name it on this occasion, but my conjectures about the atmospheres of bodies leading me to think that glass itself might afford me a confirmation of them, I quickly found, that by rubbing a very little while two solid pieces of it (not, as I remember, of the finer sort) one against the other, they would not only yield a sensible odour, but sometimes so strong an one as to be offensive: by which you will easily perceive why I told you above that I did not acquiesce in the Cartesian argument against electrical bodies performing their operations by emanations of their own substance, drawn from hence, that glass does attract light bodies, as indeed it does, though but weakly; and yet is too fixed to emit effluvioms, the contrary of which supposition the lately mentioned experiment, and by us often repeated, does sufficiently evince.



FROM what other solid bodies, and that will endure the fire, I have, or have not been able to obtain such odorous steams, it is not necessary to declare in this place, but may perhaps be done in another.

You may, I presume, have taken notice, that, according to what I intimated a while ago, I have forbore in the precedent examples to mention those effluvia of solid bodies that need the action of the fire to be obtained: but since the sun is the grand agent of nature in the planetary world, and since during the summer, and especially at noon, and in southern climates, his heat makes many bodies have little atmospheres that we cannot so well discern that they have constantly; I see not why I may not be allowed to ascribe atmospheres to such bodies as I have observed to have them when the sun shines upon them; and also to think, that the like may be attributed, at least sometimes, to such other bodies as will do the things usually performed by effluvia when yet they are excited but by an external heat, which exceeds not that of the hot sun.

Of these two sorts of bodies I shall for brevity's sake name but two or three examples, and then hasten to a conclusion.

THE first of these I must make bold to borrow from my observations about electricity, among which this is one, that to shew that the particular and usual manner of exciting such bodies, namely by rubbing them, is not always necessary, I took a large piece of good amber, and having in a summer morning, whilst the air was yet fresh, tried, that it would not without being excited attract a light body I had exposed to it, I removed it into the sun's beams, till they had made it moderately hot, and then I found according to my expectation, that it had acquired an attractive virtue, and that not only in one particular place, as is usually observed, when it is excited by rubbing, but in divers and distant places at once; at any of which it would draw to it the light body placed within a convenient distance from it; so that even in this climate of ours a solid body may quickly acquire an atmosphere by the presence of the sun, and that long before the warmest part of the day.

THE next instance you will perchance think somewhat strange, it being that, when for want of an opportunity to make the like trial in the warm sun, I took a little but thick vessel made of glass, and held it near the fire till it had got a convenient degree of heat (which was not very great, though it exceeded that which I had given the amber) I found, as I had imagined, that the heat of fire had made even this body attractive, as that of the sun had made the other.

WHAT degree of heat I have observed to be either necessary or the most convenient to excite electrical bodies according to their different natures (for the same degree will not indifferently serve for them all) this is not the properest place to declare, that it will be more to our present purpose to make some short reflection on what has been hitherto delivered.

It seems then probably deducible from the foregoing experiments and observations, that a very great number, if not the greatest part, even of consistent bodies, whether animal, vegetable, or mineral, may emit effluvia, and that even those that are solid may, at least sometimes, have their little atmospheres, though the neighbouring solids will often keep the evaporations from being every way ambient in reference to the bodies they issue from.

FOR as the instances hitherto alledged (which are not all that I could have named) do plainly shew that divers bodies, and some that have not been thought very likely, are such as we speak of; so several things induce me to believe that there may be many more of the like nature.



For first, very few, if any, have (that I know of) had the curiosity to make use of nice scales, which such trials require, to examine the expirations of inanimate bodies, which if they shall hereafter do, I make little doubt but they will light on many things that will confirm what we have been proposing, by their finding that some bodies which are not yet known to yield exhalations, do afford them, and that many others do part with far more copious ones than is imagined: for one would not easily have thought that so extremely cold a body as a solid piece of ice should make a plentiful evaporation of itself in the cold air of a freezing night; or that a piece of wood that had long lain in the house and was light enough to be conveniently hung for a long time at a balance that would lose its æquilibrium with, as I remember, half a quarter of a grain, should in less than a minute of an hour send forth steams enough to make the scales manifestly turn, and that in winter.

BUT supposing (which is my second consideration) that trials were made with good instruments for weighing; though it will follow, that in case the exposed body grow lighter, something exhales from it, yet it will not follow, that if no diminution of weight be discovered by the instrument nothing that is corporeal recedes from it. I will not urge that it is affirmed, not only by the generality of our chemists, but by learned modern physicians, that when either glass of antimony or crocus metallorum impregnate wine with vomitive and purgative particles, they do it without any decrement of their weight, because the scales in apothecaries shops, and the little accurateness wont to be employed in weighing things, by those that are not versed in statical affairs, made me, though not deny the tradition, which may perchance be true, yet unwilling to build upon observations, which to be relied on are to be very nicely made; and therefore I shall rather take notice, that though the loadstone be concluded to have constantly about it a great multitude of magnetical effluvia, which may be called its atmosphere, yet it has not been observed to lose any thing of its weight by the recess of so many corpuscles; but because, if the Cartesian hypothesis about magnetisms be admitted, the arguments drawn from this instance will not be so strong as it seems, and as it otherwise would be, I shall add a more unexceptionable example; for I know you will grant me that odours are not diffused to a distance without corporeal emanations from the odorous body; and yet, though good ambergris be, even without being excited by external heat, constantly surrounded by a large atmosphere, you will in one of the following discourses find cause to admire how inconsiderable the waste of it is.

If it be said, that in tract of time a decrement of weight may appear in bodies, that in a few hours or days discovers not any; the objection, if granted, overthrows not our doctrine, it being sufficient to establish what we have been saying, if we have evinced that the effluvia of some bodies may be subtle enough not to make the body by their avolation appear lighter in statical trials that are not extraordinarily (and as it were obstinately) protracted: and this very objection puts me in mind to add, that, for aught we know, the decrement of bodies in statical experiments long continued may be somewhat greater than even nice scales discover to us; for how are we sure that the weights themselves, which are commonly made of brass (a metal very unfixed) may not in tract of time suffer a little diminution of their weight, as well as the bodies counterpoised by them? and no man has, I think, yet tried whether glass, and even gold, may not in tract of time lose of their weight, which, in case they should do, it would not be easily discovered unless we had bodies that were perfectly fixed, by comparison to which we might be better assisted, than by comparing them with brass weights, or the like, which being themselves less fixed will lose more than gold and glass.

Mr



My third and last consideration is, that there may be divers other ways, besides those furnished us by staticks, of discovering the effluvia of solid bodies, and consequently of shewing that it is not safe to conclude, that because their operation is not constant or manifest, such bodies do never emit any effluvia at all, and so are incapable to work by their intervention on any other body, though never so well disposed to receive their action; and this I the rather desire that you would take notice of, because my chief (though not only) design in those notes is (you know) to illustrate the doctrine of occult qualities; and it may conduce to explicate several of them to know that some particular bodies emit effluvia, though perhaps they do it not constantly and uniformly; and though, perchance too, they do not appear to emit any at all, if they be examined after the same manner with other exhaleable bodies, but only may be made to emit them by some peculiar way of handling them, or appear to have emitted them by some determinate operation on some other single body, or at most small number of bodies.

PERCHANCE you did not think, until you read what I lately told you about glass, that from a body that had endured so violent a fire, there could, by so slight a way as rubbing a little while one piece against another, be obtained such steams as may not only affect but offend the nostrils: nor should we easily believe, if experience did not assure us of it, that a diamond, that is justly reputed the hardest known body in the world, should by a little rubbing be made to part with electrical effluvia; nay (that I may give some kind of confirmation to that part of the last paragraph that seems most to need it) I shall add, that I once had a diamond not much bigger than a large pea, which had never been polished or cut, whose electrical virtue was sometimes so easily excited, that if I did but pass my fingers over it to wipe, the virtue would disclose itself; and if, as soon as I had taken it out of my pocket, I applied a hair to it, though I touched not the stone with my fingers, that I might be sure not to rub it, that hair would be attracted at some distance, and many times one after another, especially by one of the sides of the stone (whose surface was made up of several almost triangular planes); and though this excitation of the diamond seemed to proceed only from the warmth that it had acquired in my pocket, yet I did not find that that warmth, though it seemed not to be altered, had always the same effect on it, though the wiping it with my finger failed not (that I remember) to excite it: something like this uncertainty I always observed in another diamond of mine, that was much nobler than the first and very well polished, and in a small ruby that I have yet by me, which would sometimes be considerably electrical without being rubbed, when I but wore the ring it belonged to on my little finger; and sometimes again it seemed to have lost that virtue of operating without being excited by friction, and that sometimes within a few minutes, without my knowing whence so quick a change should proceed. But I must insist no longer on such particulars of which I elsewhere say something; and therefore I proceed to take notice that we should scarce have dreamed, that when a partridge or hunted deer has casually set a foot upon the ground, that part where the footstep hath been (though invisibly) impressed, should continue for many hours a source of corporeal effluxes, if there were not setting-dogs, and spaniels, and blood-hounds, whose noses can take notice at that distance of time of such emanations, though not only other sorts of animals, but other sorts of dogs are unable to do so.

I saw a stone in the hands of an academick, an acquaintance of mine, which I should by the eye have judged it to be an agate, not a blood-stone; and consequently I should not have thought that it could have communicated medicinal effluvia appropriated to excessive bleedings, if the wearer of it had not been subject to that disease, and had not often cured  
both



both himself and others by wearing this stone about his neck; which if he left off, as sometimes he did for trial's sake, his exceedingly sanguine complexion (to which I have rarely seen a match) would in a few days cast him into relapses: what I have elsewhere told you about the true virtues of some stones (for I fear that most of those that are wont to be ascribed to them are false) may give some confirmation to what I have been delivering, which I cannot now stay to do, being to draw to a conclusion as soon as I have put you in mind, that it would not probably have ever been expected that so ponderous and solid a body as the loadstone should be environed by an atmosphere, if iron had been a scarce mineral, and had not chanced to have been placed near it.

AND with this instance I shall put an end to these notes, because it allows me to make this reflection, that since solid bodies may have constant atmospheres about them, and yet not discover that they have so, but by their operation upon one particular body, or those few which participate of that; and since there are already (as we have seen) very differing ways whereby bodies may appear to be exhaleable, it is not unlikely that there may be more and more bodies (even of those that are solid and hard) found to emit effluvia, as more and more ways of discovering that they do so, shall either by chance or industry be brought to light.



A N  
I N V E N T I O N  
F O R

Estimating the Weight of Water in Water, with  
ordinary Balances and Weights.

First Printed in the PHILOSOPHICAL TRANSACTIONS, No. L.

p. 1001.

“ THE author of this invention is the noble *Robert Boyle*; who was pleased to  
“ comply with our desires of communicating it in English to the curious in *Eng-*  
“ *land*, as by inserting the same in the Latin translation of his *Hydrostatical Paradoxes*  
“ he hath gratified the ingenious abroad: and it will doubtless be the more welcome,  
“ for as much as no body we know of hath so much as attempted to determine, how  
“ much water may weigh in water; and possibly if such a problem had been proposed  
“ it would have been judged impracticable.

“ THE method or expedient he made use of to perform it as near as he could, may  
“ be easily learned by the ensuing account of a trial or two he made for that purpose,  
“ which among his notes he caused to be registered in the following words.”

A glass-bubble of about the bigness of a puller's egg was purposely blown at the flame of a lamp, with a somewhat long stem turned up at the end, that it might the more conveniently be broken off: this bubble being well heated to rarefy the air and thereby drive out a good part of it, was nimbly sealed at the end, and by the help of the figure of the stem was by a convenient weight of lead depressed under water, the lead and glass being tied by a string to one scale of a good balance, in whose other there was put so much weight as sufficed to counterpoise the bubble as it hung freely in the midst of the water, then with a long iron forceps I carefully broke off the sealed end of the bubble under water, so as no bubble of air appeared to emerge or escape through the water, but the liquor by the weight of the atmosphere sprung into the un replenished part of the glass-bubble, and filled the whole cavity about half full; and presently, as I foretold, the bubble subsided and made the scale it was fastened to preponderate so much



much that there needed four drachms and 38 grains to reduce the balance to an æquilibrium; then taking out the bubble with the water in it, we did, by the help of the flame of a candle, warily applied, drive out the water (which otherwise is not easily excluded at a very narrow stem) into a glass counterpoised before; and we found it, as we expected, to weigh about four drachms and thirty grains, besides some little that remained in the egg, and some small matter that may have been rarefied into vapours, which, added to the piece of glass that was broken off under water and lost there, might very well amount to seven or eight grains; by which it appears not only that water hath some weight in water, but that it weighs very near \* or altogether as much in water as the self same portion of liquor would weigh in the air.

THE same day we repeated the experiment with another sealed bubble larger than the former, being as big as a great hen-egg, and having broken this under water, it grew heavier by seven drachms and thirty-four grains; and having taken out the bubble and driven out the water into a counterpoised glass, we found the transvafated liquor to amount to the same weight, abating six or seven grains, which it might well have lost upon such accounts as have been newly mentioned.

\* This expression was added, to leave liberty for a further inquiry, whether the experiment which hereby appears convincing as to the main thing intended to be proved, may not admit the having something further debated and annexed about some circumstantial thing or other.



# T R A C T S

ABOUT

The Cofmical Qualities of Things.

Cofmical Suspitions.

The Temperature of the Subterranean Regions.

The Temperature of the Submarine Regions.

The Bottom of the Sea.

To which is prefixed,

An Introduction to the History of PARTICULAR QUALITIES.

AN ADVERTISEMENT of the PUBLISHER to the READER.

**T**Hough the noble author of the following trafts hath also written divers other short discourses upon feveral occasions, yet, had he not been diverted from his purpose, he had continued to let them lie by him, intending, in case he should suffer them to come abroad, to dispose of them agreeably to a design that it is not necessary the reader should be now acquainted with.

In the mean while, several virtuosi, to whom some of these trafts had been shewn, and with whom the matters handled in some others had been discoursed, did, out of a concern (as they gave out) for the commonwealth of learning, pressingly represent to the author :

FIRST, That divers of these loose trafts, having little or no dependency upon one another, might without inconvenience be published apart, in what number and order the author should please.

SECONDLY, That since his main design in these, as well as his other physical writings, was to provide materials for the history of nature, it would be thought enough that they

be



be substantial and fit for the work, in what order or association soever they should happen to be brought into the philosophical repository.

THIRDLY, That the communicating these traacts to the curious would be the best way to secure them from being lost or embezzled, as some others of his papers have been, not only formerly but very lately.

FOURTHLY, That the kind reception the curious had given to what he had hitherto presented them, might well invite, if it did not oblige him, not to envy them the early use of those experiments and hints, which will, probably before the time wherein his design would suffer them to come abroad, prove serviceable to philosophy, by setting divers inquisitive heads on work, exciting the curiosity of some, and exercising the industry of others.

LASTLY, That, as of the pieces, he had hitherto published (except where his own backwardness had expressly interposed) the first edition had not long been the only, so probably within a moderate space of time, another edition of those traacts he should first put out, would both allow him to increase their number and change their order as he should judge most expedient, and (in case he should in the mean while return to his library) recruit his discourses with those passages that he designed to borrow for them thence.

BUT though these considerations, joined to the earnestness of the persons that made them, and the just respect he had for them, rendered it uneasy for him to resist their persuasions, yet they never obtained an actual compliance, until they were assisted by such an unhappy juncture of sickness and business, as, leaving him small hopes of accomplishing his first intentions in any reasonable time, made him consent to send away to the press some of those traacts that he found the least unready for it, in the order wherein they chanced to come to his hands; which being thus represented, the considering and ingenious reader will soon find what cause there is, and how much it concerns the advancement of valuable philosophy, that, since this excellent author hath (to the publisher's knowledge, as also was insinuated above) many other rare traacts of a philosophical nature in store, he be solicited from time to time that he would be pleased, according to the measure of health he shall enjoy, to impart with all possible speed those discourses which tend to the enlargement and progress of useful knowledge, maugre all envy and malice.

H. O.

T H E  
H I S T O R Y  
O F  
P A R T I C U L A R Q U A L I T I E S.

C H A P T E R I.

THE past discourse has, I hope, *Pyrophilus*, given you some tolerable account both of the nature and of the origin of qualities in general; wherefore it now follows that we proceed to qualities in particular, and consider how far the manner whereby they are produced, and those other phaenomena of them that we shall have occasion to take notice of, will accord with, and thereby confirm the doctrine I have hitherto proposed; and whether they will not, at least, much better comport with that, than with the opinions either of the peripateticks or the chymists.

I SHALL not spend time to enquire into all the several significations of the word quality, which is used in such various senses, as to make it ambiguous enough; since by the subsequent discourse it will sufficiently appear in which of the more usual of those significations we employ that term. But thus much I think it not amiss to intimate in this place, that there are some things that have been looked upon as qualities which ought rather to be looked upon as states of matter, or complexions of particular qualities, as animal, inanimal, &c. health and beauty, which last attribute seems to be made up of shape, symmetry, or comely proportion, and the pleasantness of the colours of the particular parts of the face. And there are some other attributes, namely, size, shape, motion, and rest, that are wont to be reckoned among qualities, which may more conveniently be esteemed the primary modes of the parts of matter; since from these simple attributes, or primordial affections, all the qualities are derived. But this consideration relating to words and names, I shall not insist upon it.

NOR do I think it worth while to enumerate and debate the several partitions that have been made of qualities (of which I have met with divers, and could perchance myself increase the number of them) for though one that were disposed to criticize upon them, would not perhaps acquiesce in any of them, but look upon them as being more arbitrary, than grounded upon an attentive consideration of the nature of the things themselves; yet because it seems not to me so easy to make an accurate distribution of qualities, till some things that concern them be better cleared up than yet they are, I shall content myself for the present, to propose to you one of the more received divisions of physical qualities (for you know I do not pretend to treat of any other) allowing myself the liberty of making, where there seems cause, the members of the distribution somewhat



more comprehensive. We will then, with many of the moderns, divide physical qualities into manifest and occult; and reserving the latter to be treated of apart, we will distribute the former into first, second, and third; to the two last of which we will reserve divers qualities not wont to be treated of by school writers of physical systems, which, for distinction sake, we may without much inconveniency stile some of them the chemical qualities of things, because as *Aristotle* and the school-men were not acquainted with them, so they have been principally introduced and taken notice of by means of chemical operations and experiments; such as are fumigation, amalgamation, cupellation, volatilization, precipitation, &c. by which operations, among other means, corporeal things come to appear volatile or fixed, soluble or insoluble in some menstruums, amalgamable or unamalgamable, capable or incapable to precipitate such bodies, or be precipitated by them, and, in a word, acquire or lose several powers to act on other bodies, or dispositions to be wrought on by them; which attributes do as well deserve the name of qualities, as divers other attributes to which it is allowed. And to these chemical qualities we may add some others, which, because of the use that physicians either only, or above other men make of them, may be called medical, whereby some bodies taken into that of a man are deopiplating, others inciding, resolving, discussing, suppurating, absterfise of noxious adherences, and thickening the blood and humours, being astringent, anodinous or appeasing pain, &c. for though some of the faculties of medicines, as those of heating, cooling, drying, attenuating, purging, &c. may be conveniently enough referred to the first, second, or third qualities wont to be mentioned by naturalists, and others are wont to be reckoned among occult ones; and though these medical qualities are wont to be treated of by physicians, yet it seems to me that divers of them ought not to be referred to the qualities to which they are wont to be so; and the handling of them may be looked upon as a desideratum in natural philosophy, and may well enough deserve a distinct place there; since the writers of that science are not wont to treat of them at all, and physicians handle them as physicians, whom it concerns but to know what bodies are endowed with them, and what good or ill effects they may have upon human bodies, not as naturalists, whose business it is to enquire into the production and causes of those as well as of other qualities.

## C H A P. II.

BEFORE we descend to the mention of any of these particular qualities, I think it very expedient to spend a little time in considering three grand scruples about our and the corpuscularian doctrine touching qualities, which three difficulties, though I remember to have found them expressly objected by the adversaries of the corpuscularian philosophy, nor (perhaps only for that reason) to have been purposely solved by the patrons of it, are yet such, that having been suggested to me by considering the nature of the thing, I cannot but fear that they also may occur to and trouble you; since they seem to me of that importance, that unless they be removed, they may very much prejudice the reception of a good part of what I am to deliver about particular qualities.

THE first of the above mentioned objections is grounded upon the received opinion of vulgar and Aristotelian philosophers, that diversity of qualities must needs flow from substantial forms, either because it is part of their nature to be the principals of properties, and peculiar operations in the bodies they inform; or else because divers of them are such, that no mixture of the elements is capable of producing them.

OF

Of the two suppositions whereon this difficulty is founded, we have already shewed the former to be unfit to be admitted, by what has been said in our examen of substantial and subordinate forms; and therefore it will only remain that we examine also this second supposition, which may therefore deserve the greater consideration, because it is much pressed and relied on by the learned *Sennertus* (and his followers) who improves the argument by this addition, that as no bare mixture of the elements, so no general *formamissionis* (such as divers of the moderns have introduced to help out the hypothesis) is sufficient to give an account of divers qualities, which he somewhere reckons up.

BUT, in the first place, whereas the proposers of this difficulty take it for granted that there are four elements, from whose various mixtures all other sublunary bodies spring, and are therefore only solicitous to prove that such and such qualities cannot flow from their mixture; I need not much concern myself for their whole discourse, since I admit not that hypothesis of the four elements that is supposed in it; and yet I may be allowed to observe from hence, that by the confession of those modern peripateticks that urge this argument, those ancient and other Aristotelians were mistaken, who ascribed to the mixture of the elements effects, for which these maintain them to be incompetent.

BUT since replies of this nature do rather concern the objectors than the objection, I proceed to consider the difficulty itself, not only as it may be proposed by peripateticks, but by chemists, who, though some of them do not with others of their sect allow of the four elements, do yet agree with the schools in this, that there is a determinate number of ingredients of compounded bodies, from whose mixture and proportion many qualities must be derived; and those that cannot, must be resolved to flow from a higher principle, whether it be a substantial form, or something for which chemists have several names, though, I doubt, no settled and intelligible notion.

To consider then the difficulty itself, I shall for the removal of it present to you four principal considerations.

BUT before I begin by any of these to answer the objection, I shall readily acknowledge, that in some respects, and in some cases, it may not be ill grounded; but I shall add, that in those cases I look upon it rather as a part of the corpuscularian doctrine than an objection against it; for when it happens that there is a strict connexion betwixt that modification of matter which is requisite to exhibit one phenomenon, and that from which another will necessarily follow; in such case we may not only grant, but teach, that he, who by a change of its texture gives a portion of matter the former modification, does likewise qualify it by the same change to exhibit the congruous phenomenon; though one would not perchance suspect them to have any such dependence upon one another. As for instance, strong spirit of distilled vinegar, by virtue of its being an acid spirit, hath the faculty to turn syrup of violets red; but if by making with this spirit as strong a solution as you can of coral, or some such body, you destroy the acidity of the spirit of vinegar; this liquor, as it has quite another taste, so it may, and indeed will, have another operation than formerly upon syrup of violets. For I remember that upon a trial I purposely devised to illustrate this matter, I found that the lately mentioned solution, and some others made with spirit of vinegar, would presently, like an alkalizate or urinous salt, turn syrup of violets from its native blue, not any longer into a red, but into a lovely green; and prosecuting the experiment a little farther, I found that spirit of salt itself deslegmed by a fit concrete, though the solution were horribly strong, had yet the same effect on syrup of violets. But because the cases where the above mentioned connexion of qualities and modifications occur, are comparatively but few, I shall here consider them no farther, but proceed to the four particulars I was lately proposing.

AND



AND in the first place, I say, that things may acquire by mixture very differing qualities from those of any of the ingredients.

Of this I shall have occasion to give a multitude of instances in the following notes upon particular qualities; and therefore it may now suffice to mention two or three, that are the more obvious in the laboratories of chemists; as, that sugar of lead is extremely sweet, though the minium, and the spirit of vinegar of which it is made, be the former of them insipid, and the latter sour. And though neither aqua regis, nor crude copper, have any thing in them of blue, yet the solution of this metal in that liquor is of a deep blue; and sometimes I have had the solution of crude mercury in good aqua fortis of a rich green, though it would not long continue so: and of such instances you will, as I was saying, hereafter meet with plenty. So that they are much mistaken who imagine either that no manifest qualities can be produced by mixture, except those that reside in the elements, or result immediately from the combinations of the four first qualities. For not to repeat what variations the mixtures of the most simple ingredients only may produce; it is manifest that nature and art must continually make mixtures of bodies, both of already compounded bodies, as when ashes and sand compose the common coarse glass, and when nature combines sulphur with unripe vitriol, and perhaps other substances in a marcasite; and also of bodies already decomposed, as native vitriol is made in the bowels of the earth of an aqueous liquor impregnated with an acid salt, and of a cupreous or martial mineral, strictly united both to a combustible sulphureous substance, and to another body of a more fixed terrestrial nature. And thus artificers may easily, as trial hath assured me, produce new and fine colours, by skilfully mixing in the flame two pieces of annels (which are already decomposed bodies) of colours more simple or primary than that which results from their colligation. And this way of so combining bodies, not simple or elementary, will be acknowledged capable of being made much more fertile in the production of various qualities and phenomena of nature, if you consider how much the variation of the proportion of the ingredients in a mixed body may alter the qualities and operations of it, and that proportion is capable of being varied almost *in infinitum*. Thus much may suffice for our first consideration; especially since divers things by which it may be much confirmed, will be met with in the two following chapters.

In the second place I observe, that it is but an ill-grounded hypothesis to suppose that new qualities cannot be introduced into a mixed body, or those that it had before be destroyed, unless by adding or taking away a sensible portion of some one or more of the Aristotelian elements, or chemical principles; for there may be many changes as to quality produced in a body without visibly adding or taking away any ingredient, barely by altering the texture, or the motion of the minute parts it consists of: for when (for instance) water hermetically sealed up in a glass is by the cold of the winter turned into ice, and thereby both loseth its former fluidity and transparency, and acquires firmness, brittleness, and oftentimes opacity, all which qualities it loseth again upon a thaw; in this case, I say, I demand what element or hypostatical principle can be proved to get into, or out of this sealed glass, and by its intrusion and recess produce these alterations in the included body. And so in that fixed metal, silver, what sensible accession or decrement can be proved to be made as to ingredients, when by barely hammering it (which doth but change the situation and texture of the parts) it acquires a brittleness, which by ignition, wherein it doth not sensibly lose any thing, it may presently be made to exchange for its former malleableness? And the same experiment gives us an instance also that the invisible agitation of the parts may alone suffice to give a body, at least for a while, new qualities; since a thick piece of silver nimbly hammered will quickly acquire



quire a considerable degree of heat, whereby it will be enabled to melt some bodies, to dry others, and to exhibit divers phenomena that it could not produce when cold. I might add, that spirit of nitre moderately strong, though when included in a well-stopped phial in the form of a liquor it will appear diaphanous, and without any redness, will yet fill the upper part of the phial with red fumes, if the warm sun-beams or any fit heat (though but externally applied, and though the glass continue close stopped) do put the nitrous spirits into a somewhat brisker motion than they had or needed whilst in the form of a liquor. I might also demand both what new element or principle is added to a needle, when the bare approach of a vigorous loadstone endows it with those admirable qualities of respecting the poles, and (in due circumstances) drawing to it other needles; and what ingredient the steel loses, when by a contrary motion of the loadstone, it is in a minute deprived of its magnetism. And to these I might subjoin divers like questions; but of instances and reflections proper to confirm this second consideration you may meet with so many, partly in another treatise, and partly in the ensuing chapters, that it will be needless to multiply them here. Wherefore in the third place I shall observe, that when we are considering how numerous and various phenomena may be exhibited by mixed bodies, we are not to look upon them precisely in themselves; that is, as they are portions of matter, of such a determinate nature or texture, but as they are parts of a world so constituted as ours is, and consequently as portions of matter, which are placed among many other bodies. For being hereby fitted to receive impressions from some of those bodies, and to make impressions upon others of them, they will upon this account be rendered capable of producing, either as principal or auxiliary causes, a much greater number and variety of phenomena than they could exhibit, if each of them were placed in *vacuo* (or if a vacuum be a thing impossible) in a medium, that could no way either contribute to, or hinder its operations.

THIS has been partly proved already in the discourse of the origin of forms, and will be farther manifested ere long; and therefore it may suffice, that of the particulars mentioned in those writings, those that are pertinent to this argument be mentally referred hither.

WHEREFORE having thus dispatched the third consideration, I now proceed to the fourth and last, which is, that the four peripatetick elements and the three chemical principles are so insufficient to give a good account of any thing near all the differing phenomena of nature, that we must seek for some more catholick principles; and that those of the corpuscularian philosophy have a great advantage of the other in being far more fertile and comprehensive than they. I must not here stay to make full representation of the deficiencies of the Aristotelian hypothesis, having in other tracts said much to that purpose already; but yet our present argument invites me to intimate these two things; the first, that such phenomena as the constant and determinate shape and figure of the mountains our telescopes discover (together with their shadows) in the moon, and the strange generation and perishing of the spots of the sun, to omit the differing colour of the planets, and divers other qualities of celestial bodies, cannot be ascribed to the four elements, or their mixtures, nor to those of the three chemical principles, which are allowed to be confined to the sublunary region. And the second, that there are very many phenomena in nature (divers of which I \* elsewhere take notice of) several whereof neither the peripatetick nor the chemical doctrine about the elements, or the ingredients of bodies, will enable a man to give so much as any probable account; such are the eclipses of the sun, the moon, and also the satellites of Jupiter, the proportion of the

\* Principally in the Sceptical Chymist.



acceleration of descent observable in heavy bodies, ebbing and flowing of the sea, a great number of magnetical, musical, statical, dioptrical, catoptrical, and other sorts of phenomena, which haste makes me here leave unmentioned.

AND having said thus much about the first part of our proposed consideration, and thereby shewn that the vulgar doctrine about the ingredients of bodies falls very short of being able to solve several kinds of nature's phenomena, we may add, in favour of the second part, that it will follow in general, that it is fit to look out for some more pregnant and universal principles; and that, in particular, those of the corpuscular hypothesis are, as to those two attributes, preferable by far to the vulgar ones, will I hope appear by our answers to the two objections that remain to be examined in the two following chapters, to which that I might the more hasten, I thought fit to insist the less upon the objection hitherto examined, especially because partly in this and the two next chapters, and partly elsewhere, I suppose there is contained a very sufficient reply to that objection. And I confess I should think it strange that the consideration of the various motions and textures of bodies should not serve to solve far more phenomena than the bare knowledge of the number (and even that of the proportions) of their quiescent ingredients; for as local motion is that which enables natural bodies to act upon one another, so the textures of bodies are the main things that both modify the motion of agents, and diversify their effects according to the various natures of the patients.

### C H A P. III.

I ENTER now upon the consideration of the second, and indeed the grand difficulty objected against the (corpuscularian) doctrine proposed by me about the origin of qualities, viz. that it is incredible that so great a variety of qualities as we actually find to be in bodies should spring from principles so few in number as two, and so simple as matter and local motion; whereof the latter is but one of the six kinds of motion reckoned up by *Aristotle* and his followers, who call it *lotion*, and the former, being all of one uniform nature, is according to us diversified only by the effects of local motion. Towards the solving this difficulty, I shall endeavour to shew, first, that the other catholic affections of matter are manifestly deducible from local motion; and next, that these principles being variously associated, are so fruitful, that a vast number of qualities and other phenomena of nature may result from them.

THE first of these will not take us up much time to make out; for supposing, what is evident, that the (1) local motion belonging to some parts of the universal matter does not at all tend the same way, but has various determinations in several parts of that matter, it will follow, that by local motion thus circumstanced, matter must be divided into distinct parts; each of which being finite, must necessarily be of some (2) bigness or size, have some determinate (3) shape or other.

AND since all the parts of the universal matter are not always in motion, some of them being arrested by their mutual implication, or having transferred (as far as our senses inform us) all that they had to other bodies, the consequence will be, that some of these portions of the common matter will be in a state of (4) rest (taking the word in the popular sense of it). And these are the most primary and simple affections of matter.

BUT because there are some others that flow naturally from these, and are, though not altogether universal, yet very general and pregnant; I shall subjoin those that are the most fertile principles of the qualities of bodies and other phenomena of nature.



MOREOVER then not only the greater fragments of matter, but those lesser ones, which we therefore call corpuscles or particles, have certain local respects to other bodies, and to those situations which we denominate from the horizon; so that each of these minute fragments may have a particular (5) posture or position (as erect, inclining, horizontal, &c.) and as they respect us men that behold them, there may belong to them a certain (6) order or consecution, upon whose account we say one is before or behind another; and many of these fragments being associated into one mass or body, have a certain manner of existing together, which we call (7) texture, or by a word more comprehensive, modification. And because there are a very few bodies whose constituent parts can, because of the irregularity or difference of their figures, and for other reasons, touch one another every where so exquisitely, as to leave no intervals between them, therefore almost all consistent bodies, and those fluid ones that are made up of grosser parts, will have (8) pores in them, and very many bodies having particles, which by their smallness, or their loose adherence to the bigger, or more stable parts of the bodies they belong unto, are more easily agitated and separated from the rest by heat and other agents: therefore there will be great store of bodies that will emit those subtle emanations that are commonly called (9) effluvijs. And as those conventions of the simple corpuscles that are so fitted to adhere to, or be complicated with one another, constitute those durable and uneasily dissoluble clusters of particles that may be called the primary concretions or elements of things, so these themselves may be mingled with one another, and so constitute compounded bodies; and even those resulting bodies may, by being mingled with other compounds, prove the ingredients of decompounded bodies, and so afford a way whereby nature varies matter, which we may call (10) mixture, or composition; not that the name is so proper as to the primary concretions of corpuscles; but because it belongs to a multitude of associations, and seems to differ from texture, with which it has so much affinity, as perhaps to be reducible to it, in this, that always in mixtures, but not still in textures, there is required a heterogeneity of the component parts. And every distinct portion of matter, whether it be a corpuscle or a primary concretion, or a body of the first, or of any other order of mixts, is to be considered, not as if it were placed in vacuo, nor as if it had relation only to the neighbouring bodies, but as being placed in the universe, constituted as it is, amongst an innumerable company of other bodies, whereof some are near it, and others very remote, and some are great and some small, some particular and some catholic agents, and all of them governed as well by (11) the universal fabrick of things, as by the laws of motion established by the author of nature in the world.

AND now, *Pyrophilus*, that we have enumerated 11 very general affections of matter, which with itself make up 12 principles of variation in bodies, let me on the behalf of the corpuscularians apply to the origin of qualities a comparison of the old atomists employed by *Lucretius*, and others, to illustrate the production of an infinite number of bodies, from such simple fragments of matter as they thought their atoms to be. For since of the 24 letters of the alphabet associated several ways, as to the number and placing of the letters, all the words of the several languages in the world may be made; so, say these naturalists, by variously connecting such and such numbers of atoms, of such shapes, sizes, and motions, into masses or concretions, an innumerable multitude of different bodies may be formed. Wherefore, if to those four affections of matter, which I lately called the most primary and simple, we add the seven other ways, whereby, on whose account, it may be altered, that are, though not altogether, yet almost as catholic, we shall have eleven principles so fruitful, that from their various associations may result a much vaster multitude of phenomena, and among them of qualities.



qualities, than one that does not consider the matter attentively would imagine. And to invite you to believe this, I shall desire you to take notice of these three things.

THE first is, that supposing these ten principles were but  $\alpha$  many letters of the alphabet, that could be only put together in differing numbers, and in various orders, the combinations and other affociations that might be made of them, may be far more numerous than you yourself will expect, if you are not acquainted with the way of calculating the number of differing affociations that may be made between ten things proposed. The best way I know of doing this is by algebra or symbolical arithmetick, by which it appears, that of so few things so many ( $\alpha$ ) affociations may be made, each of which will differ from every one of the rest, either in the number of the things associated, or in the order wherein they were placed.

BUT (which is the second thing to be taken notice of) each of these ten producers of phenomena admits of a scarce credible variety. For not to descend so low as insensible corpuscles (many thousands of which may be requisite to constitute a grain of mustard seed) what an innumerable company of different bignesses may we conceive between the bulk of a mite, a crowd of which is requisite to weigh one grain, and a mountain, or the body of the sun, which astronomers teach us to be above an hundred and threescore times bigger than the whole terrestrial globe.

AND so though ( $\beta$ ) figure be one of the most simple modes of matter, yet it is capable partly in regard of the surface, or surfaces of the figured corpuscles (which may consist of triangles, squares, pentagones, &c.) and partly in regard of the shape of the body itself, which may be either flat like a cheese, or lozenge; or spherical like a bullet; or elliptical, almost like an egg; or cubical like a dye; or cylindrical like a rolling-stone; or pointed like a pyramid, or sugar-loaf: figure, I say, though but a simple mode, is, upon these and other scores, capable of so great a multitude of differences, that it is concerning them, and their affections, that *Euclid*, *Apollonius*, *Archimedes*, *Theodorus*, *Clavius*, and later writers than he, have demonstrated so many propositions. And yet all the hitherto named figures are almost nothing to those irregular shapes, such as are to be met with among rubbish, and among hooked and branched particles, &c. that are to be met with among corpuscles and bodies; most of which have no particular appellations, their multitude and their variety having kept men from enumerating them, and much more from particular naming them.

To which let me add, that these varieties of figure and shape do also serve to modify the motion, and other affections of the corpuscle endowed with them, and of the compounded body, whereof it makes a part.

AND that the ( $\gamma$ ) shape and also size of bodies, whether small or great, may exceedingly diversify their nature and operation, I shall often have occasion to manifest, and therefore I shall now only give you a gross example of it, by inviting you to consider how many differing sorts of tools and instruments, almost each of them fit for many different operations and uses, smiths, and other not the noblest sort of tradesmen, have been able to form out of pieces of iron, only by making them of differing sizes, and giving them differing shapes. For when I have named bodkins, forks, blades, hooks, scissars, anvils, hammers, files, rasps, chissels, gravers, screws, vices, saws, borers, wires, drills, &c. when (I say) I have named all these, I have left a far greater number unmentioned.

So likewise ( $\delta$ ) motion, which seems so simple a principle, especially in simple bodies, may even in them be very much diversified; for it may be more or less swift, and that in an almost infinite diversity of degrees; it may be simple or compounded, uniform or difform, and the greater celerity may precede or follow. The body may move in



a straight line, or in a circular, or in some other curve line; as elliptical, hyperbolic, parabolic, &c. of which geometricians have described several, but of which there may be in all I know not how many more; or else the body's motion may be varied according to the situation or nature of the body it hits against, as that is capable of reflecting it, or refracting it, or both, and that after several manners: the body may also have an undulating motion, and that with smaller or greater waves; or may have a rotation about its own middle parts; or may have both a progressive motion and a rotation, and the one either equal to the other, or swifter than it, in almost infinite proportions. As to the determination of motion, the body may move directly upwards or downwards, decliningly or horizontally, east, west, north, or south, &c. according to the situation of the impellent body. And besides these and other modifications of the motion of a simple corpuscle or body, whose phenomena or effects will be also diversified, as I partly noted already, by its bulk and by its figure: besides all these, I say, there will happen a new and great variety of phenomena, when divers corpuscles, though primogenial, and much more if they be compounded, move at once, and so the motion is considered in several bodies; for there will arise new diversifications from the greater or lesser number of the moving corpuscles; from their following one another close, or more at distance; from the order wherein they follow each other; from the uniformity of their motion, or the confusedness of it; from the equality or inequality of their bulk, and the similitude or dissimilitude of their figures; from the narrowness or wideness, &c. of the channel or passage in which they move, and the thickness, thinness, pores, and the conditions of the medium through which they move; from the equal or unequal celerity of their motion, and force of their impulse: and the effects of all these are variable by the differing situation and structure of the sensories, or other bodies on which these corpuscles beat.

WHAT we have elsewhere said to shew that local motion is, next the author of nature, the principal agent in the production of her phenomena, may I hope satisfy you that these diversities in the motion of bodies may produce a strange variety in their nature and qualities: and as I lately did, so I shall now adumbrate my meaning to you, by desiring you to apply to our present purpose what you may familiarly observe in musick; for according as the strings or other instruments of producing sounds do tremble more or less swiftly, they put the air into a vibrating motion more or less brisk, and produce those diversities of sounds, which musicians have distinguished into notes, which they have also subdivided, and whereto they have given distinct names. And though the bodies from whence these sounds proceed, may be of very differing (*i*) natures; as metalline as wire, gut-strings, bells, human voices, wooden pipes, &c. yet, provided they put the air into the like waving motion, the sound and even the note will be the same; which shews how much that greater variety which may be taken notice of in sounds, is the effect of local motion. And if the sound come from an instrument, as a lute, where not only one string hath its proper sound, but many have among them several degrees of tension, and are touched, sometimes these, sometimes those, together; whereby more, or fewer, or none of their vibrations come to be coincident, they will so strike the air, as to produce, sometimes those pleasing sounds we call concords, and sometimes those harsh ones we call discords.

It would take up too much time to insist upon each of the ten remaining affections of matter that I lately enumerated and represented to you as exceeding fertile; and by what I elsewhere deliver about pores alone, and the many sorts of phenomena, in which they may have an interest, I could add no small confirmation to what has been hitherto discoursed; if the inserting of it here would not enormously encrease the bulk of



of this paper, which I rather decline doing, because what has been already said of those we have now, though we have but very briefly treated of, may, I hope, be sufficient to persuade you that such principles as these are capable of being made far more pregnant than one would expect so few principles should be. And this persuasion will be much facilitated if we consider how great a variety may be produced not only by the diversifications that each single principle (upon the score of the attributes that may belong to it) is capable of; but much more by the several (ζ) combinations, that may be made of them, especially considering withal that our external and internal senses are so constituted, that each, or almost each of those diversifications or modifications may produce a distinct impression on the organ, and a correspondent perception in the discerning faculty; many of which perceptions, especially if distinguished by proper names, belong to the list of particular qualities.

#### C H A P. IV.

THE third and last difficulty that now remains to be considered, may be thus proposed: that whereas, according to the corpuscularian hypothesis, not only one or two qualities, but all of them proceed from the bigness and shape, and contexture of the minute parts of matter, it is consonant to their principles, that if two bodies agree in one quality, and so in the structure on which that quality depends, they ought to agree in other qualities also; since those do likewise depend upon the structure wherein they do agree; and consequently it will be scarce possible to conceive that two such bodies should be endowed with so many differing qualities, as experience shews they may.

To illustrate this objection by an example, it is pretended that the whiteness of froth proceeds from the multitude and hemispherical figure of the bubbles it is made up of. And if this or any other mechanical fabrick or contexture be the cause of whiteness, how comes it to pass that some white bodies are inodorous and insipid, as the calx of hartshorn; others both strongly scented and strongly tasted, as the volatile salt of hartshorn or of blood; some dissoluble in water, as salt of tartar; others indissoluble in that liquor, as calcined hartshorn, &c. some fixed in the fire, as the bodies last named; others fugitive, as powdered sal armoniack; some incombustible, as salt of tartar; others very inflammable, as camphire. To which examples a greater variety of white bodies might be added, if it were necessary.

THIS I confess is a considerable difficulty may puzzle more than a novice in the corpuscularian philosophy; wherefore, to do somewhat in order to the clearing of it, I shall recommend to you the four following considerations:

1. And first I shall consider, that in the pores of visible and stable bodies there may be often lodged invisible and heterogeneous corpuscles, to which a particular quality that belongs to the body as such, is to be referred. Thus we see in a perfumed glove, that in the pores of the leather odiferous particles are harboured, which are of quite another nature than the leather itself, and wholly adventitious to it, and yet endue it with the fragrantcy, for which it is prized. A like example is afforded us in raspberry wine made with claret; for the pleasing smell is imparted to the wine, by the corpuscles of the berries dispersed *per minima* through the whole body of it.

2. THE second thing that I consider is this, that oftentimes corpuscles of very differing natures, if they be but fitted to convene, or to be put together after certain manners, which yet require no radical change to be made in their essential structures, but only a certain



certain juxtaposition or peculiar kind of composition; such bodies, I say, may notwithstanding their essential differences exhibit the same quality; for invisible changes made in the minute, and perhaps undiscernible parts of a stable body may suffice to produce such alterations in its texture, as may give it new qualities, and consequently differing from those of other bodies of the same kind or denomination; and therefore though there remains as much of the former structure, as is necessary to make it retain its denomination, yet it may admit of alterations sufficient to produce new qualities. Thus when a bar of iron has been violently hammered, though it continues iron still, and is not visibly altered in its texture; yet the insensible parts may have been put into so vehement an agitation, as may make the bar too hot to be held in one's hand. And so if you hammer a long and thin piece of silver, though the change of texture will not be visible, it will acquire a springiness that it had not before; and if you leave this hammered piece of silver a little while upon the glowing coals, and after let it cool, though your eye will perchance as little perceive that the fire has altered its texture, as it did before, that the hammer had; yet you will find the elasticity destroyed.

If on the surface of a body there arise or be protuberant a multitude of sharp and stiff parts, placed thick or close together, let the body be iron, silver, or wood, or of what matter you please, these extant and rigid parts will suffice to make all these bodies to exhibit the same quality of asperity or roughness.

AND if all the extant parts of a (physical) superficies be so depressed to a level with the rest, that there is a coequation, if I may so speak, made of all the superficial parts of a body; this is sufficient to deprive it of former roughness, and give it that contrary quality we call smoothness; and if this smoothness be considerable exquisite, and happen to the surface of an opacous body of a close and solid contexture, and fit to reflect the incident rays of light and other bodies unperturbed, this is enough to make it specular, whether the body be steel, or silver, or brass, or marble, or flint, or quick-silver, &c.

AND so, as I noted in the last chapter on another occasion, if a body be so framed and stretched, as being duly moved by another body to put the air into an undulating motion, brisk enough to be heard by us, we call that sonorous, whether it be a metalline bell, or gut-strings, or wires, &c. Nay if waving motions whereinto the air is put by such differing bodies be alike, these bodies will not only in general give a sound, but will yield that particular degree of sound that men call the same note.

FOR here it is to be considered, that besides that peculiar and essential modification which constitutes a body, and distinguishes it from all others that are not of the same species, there may be certain other attributes that we call extra-essential; which may be common to that body with many others, and upon which may depend those more external affections of the matter, which may suffice to give it this or that relation to other bodies, divers of which relations we stile qualities.

OR this I shall give you an evident example in the production of heat: for provided there be a sufficient and confused agitation made in the insensible parts of a body, whether it be iron or brass, or silver, or wood, or stone, that vehement agitation, without destroying the nature of the body that admits it, will fit it for such an operation upon our sense of feeling; and upon bodies easy to be melted (as butter, wax, &c.) as we call heat.

AND so in the instance named in the objection about whiteness; it is accidental to that quality, that the corpuscles it proceeds from should be little hemispheres; for though it happen to be so in water agitated into froth, yet in water frozen to ice, and beaten very small, the corpuscles may be of all manner of shapes; and yet the powder  
be



be white. And it being sufficient to the producing of whiteness that the incident light be reflected copiously every way, and untroubled by the reflecting body, it matters not whether that body be water, or white wine, or some other clear liquor turned into froth, or ice, or glass, or crystal, or clarified resin, &c. beaten into powder; since without dissolving the essential texture of these formerly diaphanous bodies, it suffices that there be a comminution into grains numerous and small enough by the multitude of their surfaces, and those of the air, or other fluid that gets between them, to hinder the passage of the beams of light, and reflect them every way, as well copiously, as unperturbed.

PERHAPS it may not be impertinent to add to this, that there may be other catholick affections of corpuscles, besides the shape or structure of them, by virtue whereof aggregates even of such as are (as to sense) homogeneous, may exhibit differing qualities; as for instance, they may have some when they are in a brisk motion, and others, when they are but in a languid one, or at rest; as salt-petre, when its parts are sufficiently agitated by the fire in a crucible, is not only fluid but transparent almost like water; whereas when it cools again, it becomes a hard and white body; and butter, that is opacous in its most usual state, may be diaphanous when it is melted. So I shall hereafter have occasion to shew you that a great quantity of beaten alabaster, which usually retains the form of a moveless heap of white powder, by being after a due manner exposed to heat, obtains, and that without being brought to fusion, many of the principal qualities of a fluid body. And if with good spirit of nitre, or aqua fortis, you fill a glass half full, it will (unless it be extraordinarily deflegmed) exhibit no redness nor approaching colour in the vessel; but if you warm it a little, or cast into it a bit of iron or of silver, that it may put the liquor into a commotion, then the nitrous spirits devastating the form of a liquor, and ascending in that of fumes, will make all the upper part of the glass look of a deep yellow, or a red.

3. THE third thing I would recommend to your consideration is, to reflect on what I proposed in the last foregoing section, where I told you that in reference to the production of qualities, a body is not to be considered barely in itself, but as it is placed in, and is a portion of the universe. But of this subject I have said so much in the newly mentioned discourse, and in that which you are there referred to, that I shall now only put you in mind that divers of the particulars to be met with in those discourses are applicable to our present purpose.

4. To all this let me add in the last place, that, as to that part of the grand objection that we are clearing, which urges the difficulty of explicating upon the corpuscular principles, how, for example, the same body whose structure makes it shaped so as to be fit to exhibit whiteness, should likewise have divers other qualities that seemed to have no affinity with whiteness: this scruple, I say, we may, by what we have already discoursed, be assisted to remove; especially if we subjoin another consideration to it; for if corpuscles, without losing that texture which is essential to them, may (as we have shewed they may) have their shape, or their surfaces, or their situation changed, and may also admit of alterations (especially as these corpuscles make up an aggregate or congeries) as to motion or rest; as to these or those degrees, or other circumstances of motion; as to laxity and density of parts, and divers other affections; why should we not think it possible that a single, though not indivisible, corpuscle, and much more an aggregate of corpuscles, may by some of these, or the like changes, which, as I was saying, destroy not the essential texture, be fitted to produce divers other qualities, besides these that necessarily flow from it? Especially considering (which is that I have now to add) that the qualities commonly called sensible, and many others too, being according



cording to our opinion but relative attributes, one of these now mentioned alterations, though but mechanical, may endow the body it happens to with new relations both to the organs of sense, and also to some other bodies, and consequently may endow it with additional qualities.

If from good Venice or other turpentine you gently evaporate or abstract about a third part of its whole weight, you may obtain a fine transparent and almost reddish colophony; if you beat this very small, it will lose its colour and transparency, and will afford you an opacous and very white powder: if you expose this to a moderate heat, it will quickly and without violence both regain its colour and transparency, and acquire fluidity; and if, whilst it is thus melted, you put the end of a quill or reed a little beneath the surface, and blow skilfully into it, you may obtain bubbles adorned with very various and vivid colours. If when it has lost its fluidity, but whilst it is yet pretty warm, you take it into your hands, you will find that it has in that state a viscosity, by virtue of which you may draw it out into threads, as you may paste; but as soon as it grows quite cold, it becomes exceeding brittle; and if whilst it is yet warm, you give it the shape of a triangular prism, and make it of a convenient bulk, it will exhibit variety of colours almost like a triangular glass. Whilst this colophony is cold, and its parts are not put into a due motion, straws and other light bodies may be held unmoved close to it; but if by rubbing it a little you put the parts into a convenient agitation, though perhaps without sensibly warming the colophony, it displays an electrical quality, and readily draws to it the hairs, straws, &c. that it would not move before. All or most of these things you may also perform, if I mistake not, with clarified resin, though I am not sure it will do so well.

To this I shall add one instance more, which may let you see how the same body, which the chymists themselves will tell you is simple and homogeneous, may, by virtue of its shape, and other mechanical affections (for it is a factitious body, and that is made by the destruction of a natural one) have such differing respects to different sensories, and to the pores, &c. of divers other bodies, as to display several very differing qualities. The example I speak of, is afforded me by the distillation of putresced urine; for though such urine have already lost its first texture before it come to be distilled, yet when it has undergone two or three distillations to deflegm it, the spirits of it swimming in a phlegmatick vehicle have a pungent saltness upon the tongue, and a very strong, and to most persons an offensive smell in the nostrils; and when they are freed from the water, they are wont to appear white to the eye; and to very tender parts, as to those that are excoriated, or to the conjunctiva, they feel exceeding sharp, and seem to burn almost like a caustick, not to say like fire; insomuch that I have seen them presently make blisters upon the tongue itself, that was not raw or sore before they touched it; the same saline particles invisibly flying up to the eyes prick them, and make them water; and invading the nose often cause that great commotion in the head and other parts of the body, that we call sneezing. The same corpuscles, if they are much smelt to by a woman in hysterical fits, do very often suddenly relieve her, and so may be reckoned among the specifick remedies of that odd and manifold disease, which is not the only one in which they are considerable medicines, as we have elsewhere declared. The same corpuscles taken into human bodies have the qualities, that in other medicines we call diaphoretick and diuretick; the same particles being put upon filings of brass produce a fine blue, whereas upon the blue or purple juices of many plants they presently produce a green; being put to work upon copper, whether crude or calcined, they do readily dissolve it, as corrosive menstruums are wont to do other metals; and yet the same corpuscles being blended in due a proportion with the acid salts of such menstruums,



struums, have the virtue to destroy their corrosivenes; and if they be put into solutions made with such menstruums, they have a power, excepting in very few cases, to precipitate the bodies therein dissolved. I might here add, *Pyrophilus*, how the same particles applied to several other bodies, to which they have differing relations, have such distinct operations on them, as may intitle these saline spirits to other qualities. But to enumerate them in this place were tedious, especially having already named so many qualities residing in this spirituous salt; which I therefore the rather pitched upon, because being a factitious body, and made out of a putrescied one, and so simple as to be a chemical salt (which, you know, spagyriste make one of the three principles of compounded bodies) I suppose you will make the less scruple to admit, that it works by virtue of its mechanical affections; of which, to persuade you the more, I shall add, that if you compound this urinous salt with the saline particles of common salt (which is also a factitious thing, and confessed by chemists to be a simple principle of the concrete that yields it) these two being mingled in a due proportion, and suffered leisurely to combine, will associate themselves into corpuscles, wherein the urinous salt loses most of the qualities I have been ascribing to it, and with the acid spirit composes, as I have often tried, a body little differing from sal armoniac; which great change can be ascribed to nothing so probably, as to that of the shape and motion (not here to add the size) of the urinous salt, which changes the one, and loses a great part of the other by combining with the acid spirits; and to confirm that both these do happen, I have several times slowly exhaled the superfluous, but not near the whole liquor from a mixture made in a due proportion of the spirit of urine and that of salt, and found, that answerable to my conjecture, there remained in the bottom a salt, not only far more sluggish than the fugitive one of urine, but whose visible shape was quite differing from that of the volatile crystals of urine, this compounded salt being generally figured, either like combs or like feathers.

If after all this we do either add or inculcate, that the extra-essential changes that may be made in the shape, contexture, and motion, &c. of bodies, that agree in their essential modifications, may not only qualify them to work themselves immediately after a differing manner upon differing sensories, and upon other bodies also, whose pores, &c. are differently constituted, but may dispose them to receive other impressions than before, or receive wonted ones after another manner from the more catholick agents of nature; if, I say, we recommend this also to your consideration, what has been delivered in the whole discourse will, I hope, let you see that the scruple proposed at the beginning of it is not so perplexing a one to our philosophy, as perhaps you then imagined it.

THE three difficulties considered partly in this, and partly in the two foregoing sections, I was the more inclined to take notice of in this place (for in divers other passages of my writings you will meet with things that are applicable to the past discourse, and should be referred thither) partly because the scruples themselves are of great moment, and for ought I know have been discussed by others; and partly because these difficulties relating in some sort to the corpuscularian hypothesis in the general, the clearing of them may both serve to confirm several of these things that have above been written about the origin of forms and qualities (to which it might therefore have been joined) and will be conducive to a clear understanding, and explicating divers of the particulars that I am about to deliver, and perhaps several other phenomena of nature.

OF THE  
SYSTEMATICAL or COSMICAL  
QUALITIES of THINGS.

C H A P. I.

**I** EXPECT, *Pyrophilus*, that being somewhat surprized at the title of this discourse, you will presently ask what I understand by Cosmical or Sytematical Qualities; that name being new enough to require that I should tell you both what is meant by it, and why I make choice of it.

To answer so reasonable a question, I shall inform you that I consider that the qualities of particular bodies (for I speak not here of magnitude, shape, and motion, which are the primitive modes and catholick affections of matter itself) do for the most part consist in relations, upon whose account one body is fitted to act upon others or disposed to be acted on by them and receive impressions from them; as quicksilver has a quality or power (for I here take qualities in the larger sense) to dissolve gold and silver, and a capacity or disposition to be dissolved by aqua fortis, and (though less readily) by aqua regis: and this being premised, I observe farther, that, though in estimating the qualities of natural bodies we are wont to consider but the power any particular one has of acting upon, or the capacity it has of suffering from such and such particular bodies, wherewith it is taken notice of to have manifest commerce in point of making or of receiving impressions, yet there may be some attributes which may belong to a particular body, and divers alterations to which it may be liable, not barely upon the score of these qualities that are presumed to be evidently inherent in it, nor of the respects it has to those other particular bodies to which it seems to be manifestly related, but upon the account of a system so constituted as our world is, whose fabrick is such that there may be divers unheeded agents, which, by unperceived means, may have great operations upon the body we consider, and work such changes in it, and enable it to work such changes on other bodies, as are rather to be ascribed to some unheeded agents, than to those other bodies with which the body proposed is taken notice of to have to do: so that although if divers bodies that I could name were placed together *in vacuo*, or removed together into some of those imaginary spaces which divers of the schoolmen fancy to be beyond the bounds of our universe, they would retain many of the qualities they are now endowed with, yet they would not have them all; but by being restored to their former places in this world, would regain a new set of faculties (or powers) and dispositions, which, because they depend upon some unheeded relations and impressions which these bodies owe to the determinate fabrick of the grand system or world they are parts of, I have, until I can find a more proper appellation, thought fit to name their cosmical or their sytematical qualities.

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I HAVE in the Origin of Forms touched upon this subject already, but otherwise than I am now about to do: for whereas that which I do principally (and yet but transiently) take notice of is, that one body being surrounded with other bodies, is manifestly wrought on by many of those among whom it is placed; that which I chiefly in this discourse consider, is the impressions that a body may receive, or the power it may acquire from those vulgarly unknown or at least unheeded agents by which it is thus affected, not only upon the account of its own peculiar texture or disposition, but by virtue of the general fabrick of the world.

C H A P. II.

Now though there be several of the grand mundane bodies, and divers laws and customs of nature, which may contribute (more or less) to the phaenomena of the qualities we are treating of; yet, because a distinct and particular inquiry into each of them would challenge a much longer discourse than this short essay is to be, and a much abler pen than his that writes it, I did not only think it fit to reserve what occurs to me about the laws and customs of nature, as they concern this subject, to another discourse, or an appendix to this; but to declare to you also, that whereas the three main bodies whose more unobserved operations and changes have the most considerable influence on the qualities we are to treat of, are, the subterranean parts of the globe we inhabit; the stars, whether fixed or wandering, with the æther that is about them; and the atmosphere or air we live in; I foresee that it will be requisite for me to assign the experiments and observations I have collected about these three subjects to other tracts: so that in this essay my chief work will be to take notice to you of some considerations that may be introductory in a more general way to the clearer knowledge of the subject to be discoursed of, to which I may, as time and my occasions may permit, subjoin some particulars, which, though perhaps they do not all of them so directly or properly belong to the solemnly proposed heads of this discourse, yet are not impertinent to the design of it, and on that score may be allowed their places in it.

AND lest you should think, that under the name of cosmical qualities I should introduce chimæras into natural philosophy, I must betimes advertise you that you will meet with divers particles in the following discourse, fit to shew that these qualities are not merely fictitious qualities, but such whose existence I can manifest, not only by considerations not absurd, but also by real experiments and physical phaenomena: and to prevent mistakes, I shall add, that under the name of catholick and unminded causes or agents, I comprehend not only divers invisible portions of matter, but also the established laws of the universe, or that which is commonly called the ordinary course of nature: and when I speak of unobserved agents or causes, I do not always mean that they are not known or taken notice of to be in *rerum natura*, but that they are not vulgarly considered or looked upon as the causes of some particular phaenomena wherein I ascribe to them an interest or efficiency.

BUT before I proceed any farther, it will not be amiss to intimate in this place, that the things on which I founded the purposed notion of the cosmical attributes of bodies, were principally these three;

1. THAT there are many bodies that in divers cases act not unless they be acted on; and some of them act either solely or chiefly, as they are acted on by the catholick and unheeded agents we have been speaking of.

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2. THAT there are certain subtle bodies in the world that are ready to insinuate themselves into the pores of any body disposed to admit their action, or by some other way affect it, especially if they have the concurrence of other unobserved causes and the established laws of the universe.

3. THAT a body by a mechanical change of texture may acquire or lose a fitness to be wrought upon by such unheeded agents, and also to diversify their operations on it upon the score of its varying texture.

THESE three propositions I shall endeavour to confirm distinctly by the ensuing experiments and phenomena; but because divers of these proofs may each of them serve to confirm more than one of these propositions, and because the making out of the two last, which are the most important (and the least probable) is the main design of this discourse, I shall say the less to the first, leaving it for the most part to you to refer to either of the three propositions what you shall meet with belonging to it in what is said upon either of the other two.

### C H A P. III.

To begin then with the first proposition, namely, "That there are many bodies that in divers cases act not unless they be acted on; and some of them act either solely or chiefly, as they are acted on by the catholick and unheeded agents we have been speaking of:" the former part of it will, I presume, be easily granted, it being evident by such gross instances as these, that a wedge will not cleave a block unless it be impelled against it by a hammer (or some equivalent instrument) nor a knife attract a needle, unless it be excited by a magnet: but as to the second, it will not in likelihood be so readily assented to, and therefore having *in transitu* illustrated it by observing to you that concave looking-glasses and convex burning-glasses kindle not other bodies unless they be enabled to do so by the reflected or trajected beams of the sun, I shall proceed to prove it by a couple of instances.

THE one is an iron bar that hath long stood in a window or some other fit place in a perpendicular posture; for though this bar was not when it was first erected, endowed with a magnetism any thing superior to that of other iron bars of the like shape and bigness, yet after it hath very long stood in that position, it will by the operations of invisible agents acquire a farther degree of magnetism than belonged to it as a bar of iron, and is enabled to produce some magnetical phenomena (elsewhere mentioned) that it could not before.

THE second instance is afforded us by what happens to a very flat and exquisitely polished piece of marble; for though of itself it hath no power to help to lift up any other dry body that it is laid upon, yet if it come to be skilfully laid upon another piece of marble as flat and smooth as it, and of a bulk not too unweildy, this upper stone, by virtue of the fabrick of the world, which gives the ambient air fluidity and weight, is enabled, without any other cement or fastening instrument than immediate contact, to raise with itself (in case a man lift it up) the lower marble, though perhaps an hundred times heavier than itself\*: [whereas, if this laying one of these stones upon the other had been done *in vacuo*, I doubt not but no such power had thereby accrued to the uppermost of them].

\* See this experimentally proved in the Continuation of the Author's New Experiments touching the air, Experiment the fiftieth.



## CHAP. IV.

PROCEED we now to our second proposition, which speaks to this purpose; "That there are certain subtle bodies in the world, that are ready either to insinuate themselves into the pores of any body disposed to admit their actions, or by some other way to affect it, especially if they have the concurrence of other unobserved causes, and the established laws of the universe." I need not take notice on this occasion that divers of the ancient philosophers thought that there was a subtler body than the common air, and called æther; and that the Cartesians tell us that there is such a substance diffused throughout the universe which they call, according to the differing sizes of its parts, sometimes *primum elementum* and sometimes *materia cælestis*, to which they attribute the use of pervading all other bodies and adequately filling those pores of theirs that are correspondent in bigness and figure to the differing portions of this insinuating matter: that there may be such a substance in the universe, the asserters of it will probably bring for proofs several of the phænomena I am about to relate: but whether there be or be not in the world any matter that exactly answers to the descriptions they make of their first and second elements, I shall not here discuss, though divers experiments seem to argue that there is in the world an æthereal substance very subtle and not a little diffused. But though these things seem, as I was saying, probable enough; yet the invisible agents I shall here chiefly, though not only, take notice of, will be the air (as it hath a weight and spring) and the magnetical effluvia of the terrestrial globe.

If you take a bar of iron, or rather of steel, and another like it of silver, and having heated each of them red hot and put them to cool directly north and south; though they be both acted upon by the same agent, the fire, and the steel, as to sense, seems such as it was before, yet the texture of these two metals being different, the silver acquires no new quality by what hath been done to it, whereas the ignition of the steel having opened its pores and made its parts more pliable (as may be argued from the swelling of iron heated red hot and its softness under the hammer) it is easily, whilst in this state it lies north and south, pervaded by the magnetical effluvia of the earth which glide perpetually through the air from one pole to another, and by the passage of these streams it becomes endowed with a magnetical property, which some call polarity, whereby, being freely suspended and exactly poised, it will, as it were, spontaneously direct itself towards the north and south, and exercise some operations peculiar to magnetical bodies: and that it may seem the less strange that I should ascribe to so gross and dull a body as the earth the power of invisibly communicating to iron a magnetical virtue, which is thought to be of so spiritual a nature, I shall put you in mind of an experiment that I acquainted you with divers years ago, about the earth's power to impart, in some cases, without the help of a loadstone, a directive faculty to the loadstone itself: for, having by ignition deprived an oblong magnet of its former attractive power by taking it red hot out of the fire, and suffering it to cool north and south, I could at pleasure, by placing either end northward or southward, whilst the stone was refrigerating, make what end I had a mind to, point to the north pole; and when it had done so, I could, by a new ignition and refrigerating of it in a contrary position, make the same end of the stone become its southern pole.

If you take a capacious glass phial with a slender neck, ending in a sharp angle, with only a pin-hole left open at the apex (instead of which vessel, *Hero's* egg, as some call it, though far smaller, and without such a neck, may serve turn) and by suction or otherwise free it from as much of the included air as you can; and if then having stop-

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ped this hole with your finger you immerse it somewhat deep under water, and, lastly, withdraw your finger; the water will, contrary to its own nature (as is vulgarly conceived) spring up with violence and to a good height into the cavity of the phial; which motion of a heavy liquor upwards cannot be ascribed to the motion of the finger; for that did but unstop the orifice and not impel up the water, nor need be attributed to nature's abhorrence of a vacuum, which (whether there be such a thing or not) it is altogether unnecessary to have recourse to in this case; the pressure of the ambient air, proceeding from its weight upon the surface of the water, being sufficient to force up that liquor into the phial, in which the remaining air, by being rarefied upon the score of the absence of that which was taken out, hath its spring too much weakened to be able to resist the pressure of the outward air, as it formerly could do, whereas if this experiment were tried *in vacuo*, the water would not be raised, there being no outward agent to impel it up.

## C H A P. V.

I HAD sometimes the curiosity to consider beans and pease pulled up out of the ground by the stalks in order to an inquiry into their germination; and after having taken notice of their tumidness upon their having imbibed the moisture of the soil and of their way through the ambient earth, not only upwards with their stems, but downwards with their tender roots; I thought fit to try with what strength and force the causes of their intumescence endeavoured to dilate them, whereupon I filled with a quantity of such dry beans as are in *England* wont to be given to horses several phials and bottles, some of glass and some of earth, whereof two or three were of a considerable strength; which done, the intervals between the beans were filled with water, and the vessels were exactly stopp'd with corks strongly tied down with strings that nothing might get out; for I supposed that the water, soaking into the pores of the beans, would alter the figure of the pores and produce in them an endeavour to swell, which being checked by the sides and stopples of the vessels would discover, whether that endeavour were so forcible as I suspected; the success was, that most of these vessels (for in one or two of them we found the strings broke that withstood the raising of the stopples) whether of glass or earth, were burst in sunder.

BUT being desirous to make a nearer estimate how great this expansive force of the swelling beans was, we put a convenient quantity of them into an hollow but strong cylinder of brass, which I had caused to be purposely made for such kind of trials, whose cylindrical cavity was just six inches in length and two in diameter; then having put in water enough to reach the top of the beans, we put into the upper part of the cylinder, which was purposely left unfilled, a wooden plug, made fit for the orifice by being turned into a cylindrical form and a little narrower than the orifice, that it might move freely up and down, though the water should make it somewhat swell; upon the top of this plug, on which leaned a broad and thick piece of wood shaped like a round trencher and made of the same piece with the plug, was placed a common half hundred weight of lead, which yet could not depress the plug too low, being hindered by the breadth of the trencher, made as well to prevent the too great depression of the plug as to afford a convenient basis to the weight. Lastly, having kept the cylinder in a quiet place for a fit space of time (which is in such trials sometimes two or three days, sometimes more or less according to the temperature of the air and quantity of the included matter) we observed, as I expected, that the swelling beans had very manifestly heaved  
up



up the plug and the incumbent weight beyond the former station: and I suspected that if we had had small weights (of a pound or two a piece) conveniently shaped, a heavier weight might have been raised by the same force.

It is not necessary in this place that I mention several particulars relating to the experiment, as how it succeeds in corn, ground and unground, how in dried fruits, as raisins and currants, how in dried pease (which we found to dilate themselves very strongly) and what liquors will or will not cause an intumescence; nor shall I here speak of divers circumstances that may be taken notice of in such trials; only I must not omit this particular, that I had a mind to make some trial whether the force of swelling beans to press or thrust up the incumbent weight, would not in cylinders of different sizes be increased in somewhat near a duplicate proportion to that of the diameters or the areas of the orifices of the differing cylinders (because it is according to the greatness of those areas that the force can be applied upwards) but having not weights enough so shaped as I needed, I could not make such an experiment as I desired; but thus much, however, I discovered in order to my purpose, that the pressure upwards of the drenched beans was very much greater in wider cylindrical vessels than in narrower ones; for having put a convenient quantity of dried beans into a metalline cylinder that wanted a pretty deal of being so deep as six inches, and was not quite four inches broad; when the included beans began to swell they manifestly lifted up such a plug as was lately described (but broader) with weights upon it, amounting to an hundred pounds or better.

WHETHER this may pass for a new physical *vis movens*, I freely leave to you to determine; as also to consider, whether by mechanical contrivances so great a force as may be this way produced, and which slowly and silently proceeds till it hath attained its utmost energy, and may be conveyed into bodies without working any effect before the due time, may not in some cases be made applicable to useful purposes.

I SHALL not now examine whether or how far the foregoing experiment may confirm the Cartesian hypothesis about their *materia subtilis*; nor whether upon the notions which our experiments may suggest, we may be enabled to explicate the force wherewith fermenting liquors do often break the vessels wherein they are too exactly shut up; about which phenomena, and of some others of kin to it, I elsewhere propose some conjectures.

I THINK it fitter in this place to take notice to you of something that more directly belongs to our present subject; namely, that the air, within which name I here comprize the æther that may be harboured in its pores, may in some cases by its constant presence, and in others by its being always at hand, and its readiness to insinuate itself wherever it can get admittance, concur to the production of divers phenomena wherein its co-operation has not been suspected even by philosophers; for, not to mention what I have by experiments purposely devised, that the air's being present to press upon the superficies of liquors is so requisite in suction, that they will not thereby be made to ascend without it; and besides that to the putrefying of some bodies within the time (or even within ten times the time) that nature is wont to putrefy them in, they will not be brought to putrefaction if the air be all the while carefully secluded; besides these things, I say, I found that the light, which appears in some rotten woods, and in some putrefied fishes, did so much depend upon the presence of the air, that if that were quite withdrawn from them the light would disappear, and when they were restored to the contact of the air, they would shine forth again as formerly. [But of this elsewhere.]

I KNOW not whether it will be fit to add, that besides what the air (with the subtler matter that may be mingled with it) may do as a substance, it may perform divers things upon other accounts, as its finer parts may be, though insensibly, moved in physical straight lines; or as it is the subject of swarms of corpuscles put into peculiar though invisible motions. For instance, if I take a sheet of paper and rub it over with oil, or even a fit kind of grease; that which the liquor apparently does is only to pierce or soak into the pores of the paper which before did by their crookedness, or upon some other mechanical account render the paper opacous: but this insinuation of the unctuous body into the pores having altered them as to figure, or to size, or to both, and having by that alteration given the paper a texture disposed to allow due passage to the corpuscles of light, or to transmit their peculiar kind of impulse (whence several naturalists derive light) the motions, as I was saying, or invisible corpuscles in the air, depending upon the constitution of the world do presently act upon the paper, and produce beyond it both a sensation of light, and the representations of a multitude of objects whence the light reflects, and which could not be seen through it before.

I NEED not perhaps tell you, that if a pretty large box be so contrived that there may be towards the one end of it a fine sheet of paper stretched like the leather of a drum-head at a convenient distance from the remoter end, where there is to be left an hole covered with a lenticular glass fitted for the purpose, you may at a little hole left at the upper part of the box see upon the paper such a lively representation, not only of the motions but shapes and colours of outward objects, as did not a little delight me when I first caused this portable darkened room, if I may so call it, to be made: which instrument I shall not here more particularly describe, partly because I shewed it to you several years ago, since when divers ingenious men have tried to imitate mine (which you know was to be drawn out or shortened like a telescope, as occasion required) or improve the practice; and partly because that which I pretended in mentioning of it here, is to shew, that since that almost upon every turning of the instrument this way or that way, whether it be in the town or open fields, one may discover new objects and sometimes new landscapes upon the paper, there must be all day long in all parts of the air where this phenomenon can be exhibited, either certain effluvia emitted every way from the objects or certain motions of insensible corpuscles, which rebounding first from the external object and then from the paper, produce in the eye the images of these objects, so that the air is every where full of visible species which cannot be intelligibly explicated without the local motions of some minute corpuscles, which, whilst the air is enlightened, are always passing thorough it.

You may remember, *Pyrophilus*, that in the clause of the second proposition hitherto discoursed of, I take in the established laws of the universe as a part of the present constitution of this our world; some of those laws contributing much to the operation of those unheeded causes we are treating of. Of these I may another time give you some instances, but for the present it may suffice to take notice of this one, that if you take a bar of iron, and holding it perpendicularly, apply the lowest part of it to the northern point of a well poised magnetical needle, the bar will presently drive it away; but that magnetism by which the bar does it, as it is presently acquired by the posture which it had, so it is as suddenly changed if you invert that posture, as appears by this, that though you hold the bar perpendicular, if it be held under the needle, so that the same part of the bar which before was placed directly over the north point of the needle, be

held



held directly under the same point, the bar will not, as before, drive it away, but, as they commonly speak, attract it: but if this bar has been for a long time kept in an erected posture, as if it be taken from some old window; or if, having been heated and refrigerated, it have very long lain north and south, it will appear endowed with a stronger and more durable verticity, as we elsewhere more fully declare; which seems to proceed from this, that by lying north and south, it lay in the way, which, according to the established laws of nature, the magnetical effluvia of the earth must pass along in steams from pole to pole; whereby they have the opportunity by little and little to work upon the pores of the iron that lies in their way and fit them to give passage to the effluvia of magnetical bodies; in which fitness seems principally to consist the magnetism of iron; whereas, if this metal had all this while lain east and west, instead of north and south, it would have acquired little or no magnetical virtue. And the reason why an erected posture gives a rod or bar of iron a power to drive away the north point of the needle has been probably conceived to be this, that the lower end being nearer the earth does more plentifully participate of the magnetick steams which fly in a closer order there than further off, and by powerfully affecting that part of the iron turn it for a time into the iron's north pole, which, according to the laws magnetical, ought to drive away the north pole of the needle and attract the south; whereas if the bar being inverted, that end which was uppermost becoming the lower, must for the same reason have a contrary operation, unless by having long stood, its verticity be too well settled to be suddenly destroyed or altered by the effluvia of so languid a magnet as the earth: but whether or no this explication be the right one (for I would not contend for its being so) it appears by the requisiteness both of a determinate position of the iron and of its long continuance in that position, to make that metal acquire a durable verticity, that those unheeded magnetical steams which communicate such a magnetism to the iron, move and act according to laws established in nature; which is as much as my design in this discourse makes necessary to be made out.

## C H A P. VII.

It remains now that we discourse of the last of our three grand propositions, namely, "That a body by a mechanical change of texture may acquire or lose a fitness to be wrought upon by such unheeded agents, and also to diversify their operations on it upon the score of its varying texture."

This proposition is of so much affinity with the foregoing, that there are divers cases wherein the same experiments and other arguments may serve for the confirmation of both.

BUT to illustrate a little what I mean by gross and sensible examples, it is a custom we often observe at sea, when we sail with too slack a wind, to take up water with certain instruments and throw it against the sails. At the first proposal this may seem a very improper way to promote the swiftness of the ship, since there is the weight of so much water added to that of the vessel itself; but yet I have seen the seamen make use of it as one of their best expedients when we were closely chased by pirates, nor did I look upon it as irrational; for whereas, when the sails are dry a good part of the wind that blows upon them easily gets thorough those meshes or great pores that are left between the threads of which a sail consists; when it comes to be wetted the imbibed water makes the threads swell every way, and consequently very much streightens the pores or intervals that were formerly left between them; by which means the wind cannot permeate them

as freely as formerly, but by finding a greater resistance in the sail comes to beat more forcibly upon it and consequently drives it, and with it the ship, more strongly on than else it would have done; not to mention the stiffness of the sail acquired by the imbibed sea-water, because I would not stay to take notice of other particulars to which the success of this practice may perhaps be in part ascribed.

To add another instance to the same purpose with the former; suppose an high wind to blow against a chamber, wherein the windows and doors are all shut, the effect will be only to shake a little the room in general, but if one open the casement, though he that does it do properly and immediately but displace some little piece of the iron or other thing that shuts the window, yet this being done in a place where there is a strong current of air, which we call a wind, there will presently follow a blowing up of curtains or hangings and blowing about of dust, straws, feathers, or other light bodies that are not firmly enough fastened nor very ponderous, and yet are too heavy to be blown about.

BUT to proceed to instances that are not so gross, I might take notice that though good common tartar does usually of itself keep dry in the air, nay, and will not easily be dissolved in cold water; yet if it be calcined, though but very moderately, the salt in the remaining coal, the texture being now altered, will readily enough in the moist air (as that of a cellar) run into that liquor that chemists have been pleased to call of tartar per deliquium: but in regard that to make the change the greater, part of the tartar must be driven away by the fire, I shall rather make use of an example easily drawn from an experiment I elsewhere mentioned to another purpose; for having taken a loadstone, and according to the way there delivered, heated it and cooled it, though it had lost so little by the fire that the eye took no notice of its being changed either as to shape or bulk, yet the operation of the fire by changing the invisible texture did so diversly alter the disposition of it in reference to the magnetical effluvia of the earth, that I could presently and at pleasure change and re alter the poles of the stone, making the same end point sometimes to the north and sometimes to the south. The like change of verticity I have, as I elsewhere declare, made mere iron capable of without the help of fire or any other magnet than the earth; and I have also found by trial that a certain heavy stone that is usually thought to be not so much as of a metalline nature, may, by a slight and quick preparation that alters not the shape nor bigness, be enabled to attract and repel the poles of a magnetic needle.

#### C H A P. VIII.

To the instances already given in solid bodies it will not be amiss to annex two or three in liquid bodies, because it may be thought strange by some that considerable changes of texture should, without fire or any new ingredients, be produced in bodies, which, by reason of their fluidity, seem presently to recover their texture if it be disordered. If honey and water be each of them apart put into a convenient vessel, they will both of them retain their nature; and though you mix them together in an undue proportion, so that by reason of overmuch honey the consistence be too thick, or that by being diluted by too great a proportion of water, the solution of honey be too thin, they may continue honey and water; but if those two liquors be duly proportioned (as if you put to one part of honey four or five of water) then their new texture so disposes them to be acted on by the subtle permeating matter or whatever other common agent nature employs to produce fermentations, that the ingredients do no longer continue what they were, but begin



begin to work like new must or beer-wort; and I have tried that so small and short a local motion (as carrying such mixtures a while in a coach) has so excited the liquor as to make it violently force its way out of the vessel or throw off the stopple, that I have wondered at it. And I remember that an eminent merchant of wines who spent divers years in the *Canaries*, being asked by me about some things of this nature, assured me, that in those fortunate islands (as the ancients style them) he had several times observed, that if a pipe of the best sort of Canary were, when it was about a month old, rudely rolled, though but the length of an hall or moderate gallery, so transient and slight a discomposure of the texture would quickly make so great a change in it, that oftentimes a good quantity of wine would be violently thrown out at the bung; or if the pipe were too close stopped, that great vessel itself would oftentimes have the bottom beaten out; by which means he had known several pipes of that rich liquor lost.

We have divers examples of the cracking of common glass when it is too soon, after it hath been removed from the fire, exposed to the cold air, and the subtle bodies that are in it; which would not have cracked it if it had been cooled more slowly, so that its parts would have had leisure to settle into a texture convenient for the passage of those subtle bodies which in that case would harmlessly have permeated it. But I have sometimes shewn the curious a more quick and manifest instance of the importance of the present texture of a body in reference to the catholick and invisible causes that may work upon it: for having taken a plate of so ponderous and solid a body as copper, and heated it red hot, and then suffered it to cool a while upon some more moderately hot place in the fire, though it did not appear at all ignited, when I removed it to a plate or even to a sheet of paper, yet upon its being exposed to the atmosphere, the superficial part would not only crack as in over hastily cooled glass, but would, and that presently, fly off in flakes in good number, and not without noise; so that in a short time I have had the neighbouring part of the paper, on which the brass-plate rested, almost quite covered with little scales, as it were, of that metal.

AND to give you, in favour of what I have been hitherto discoursing, an instance of a very subtle nature, I will not, though I justly might, take notice that in rotten fish and rotten wood the change of texture is oftentimes invisible that will suffice to make the contact of the air, and the subtle corpuscles whereto it gives harbour or passage, confer or lose a power of shining; but I will rather choose to instance in the Bolonian stone, which by calcination acquires this admired property, that if it be but exposed to the sun-beams (to which I have found other strong lights succedaneous) it will not only in a few minutes acquire a luminousness, but for some time after retain it in the dark.

## COSMICAL SUSPICIONS

(SUBJOINED AS AN

## A P P E N D I X

To the DISCOURSE of

## The COSMICAL QUALITIES of THINGS).

IN the former essay, *Pyrophilus*, I proposed to you some things about the subject there treated of that seemed to have in them such a degree of probability as is wont to be thought sufficient to physical discourses, or at least is usually to be met with in them. But in regard the world, whether we take it in the larger sense for the whole universe, or in the more narrow but not less common acceptation, for the globe we men inhabit, is a subject so vast, that not only all demonstrable truths that may be discovered concerning it may be looked upon as important, but even conjectures and suspicions themselves that relate to it in general, if they be not very groundless or extravagant, may deserve not to be altogether passed by in silence; I will adventure to entertain you a while with some thoughts of this nature, especially because they will give me opportunity to alledge in their favour some historical observations, which, whatever the doubts or conjectures be thought of, may appear to be more new than despicable.

It may now, therefore, be not unseasonable to confess to you that I have had some faint suspicion, that besides those more numerous and uniform sorts of minute particles, that are by some of the new philosophers thought to compose the æther I lately discoursed of, there may possibly be some other kind of corpuscles fitted to have considerable operations, when they find congruous bodies to be wrought on by them: but though it is possible, and perhaps probable that the effects we are considering may be plausibly explicated by the æther, as it is really understood; yet I somewhat suspect that those effects may not be due solely to the causes they are ascribed to, but that there may be, as I was beginning to say, peculiar sorts of corpuscles that have yet no distinct name, which may discover peculiar faculties and ways of working, when they meet with bodies of such a texture as disposes them to admit, or to concur with the efficacy of these unknown agents.

THIS suspicion of mine will seem the less improbable if you consider, that though in the æther of the ancients there was nothing taken notice of but a diffused and very subtle substance; yet we are at present content to allow that there is always in the air a swarm of steams moving in a determinate course betwixt the north pole and the south; which substance we should not probably have dreamed of, if our inquisitive *Gilbert* had not hap-



pily found out the magnetism of the terrestrial globe. And few, perhaps, would have imagined, that when an hunted and wounded deer has hastily passed over a little grass, he should leave upon it such determinate, though invisible effluvia, as should for many hours so impregnate the air, as to betray the individual flying and unseen deer, if there was no blood-hounds, upon whose peculiarly disposed organs of smelling these steams are fit to operate. And it is strange that there should be such effluvia for a long time (perhaps a year or two together) residing in the air, that though our senses discern them not, and though they have no operation upon other men, yet if they meet with persons of a peculiar temperament, who by that, and by their formerly having had the plague, have attained a peculiar disposition that fits them to be wrought on by pestilential steams, they may so operate upon them, that some of these persons may be able to discern those steams to be pestilential. To give some countenance to which paradox, I will here annex two or three testimonies, the first of which I find thus set down among my Adversaria. [Above three months before the late great plague began in London, in the year 1665, there came to Dr. M. a patient of his, to desire his advice for her husband; and the doctor having enquired what ailed him, she answered, that his chief distemper was a swelling in his groin, and upon that occasion added, that her husband assured her of his being confident, that the next summer the plague would be very rife in London; for which prediction he gave this reason, that in the last great plague he fell sick of that disease; and he then had a pestilential tumour.

So in two other plagues that since happened, though much inferior to that great one, each of them had a rising in his body to be its forerunner, and now having a great tumour in the forementioned place, he doubted not but it would be followed by a raging pestilence, which accordingly ensued. Having heard much talk of something of this nature, and being this morning casually visited by the doctor, a person of great veracity, I enquired of him how much of it was true, and I received for answer the foregoing narrative.]

THE second is a very remarkable story, which I remember that famous and excellent chirurgeon *Fabricius Hildanus* records of himself; namely, that having had a pestilential tumour during a plague that happened in his youth, if, for many years after, he chanced to go to, or so much as to pass by an house infected with the plague, he was admonished of the particular disease that reigned there by a sensible pain in that part, where he had had a pestilential tumour so long before.

THE third testimony is afforded me by that curious observer of the changes that happened as to the phenomena of diseases at the famous siege of *Ereda*, where this diligent physician, practising much among patients afflicted with malignant and pestilential diseases, was at length infected himself; whereupon he informs his readers; *Annotandum his merito naturæ facultatem ad pestis præservationem momenti esse maximi. Observavi in meipso contaminatos invasente statim inguen dolere vel axillas: afficiebatur aliquando caput, notu inde sudor, & secessus tres quatuorve. Hoc & aliis accidit, qui fideliter mihi retulerunt.*

If these stories were related by ordinary persons of what happened to other men, the oddness of them might well tempt a wary man to suspend his judgment; but the judiciousness of the writers, and the profession they were of, and their relating these as things that did more than a few times happen to themselves, may well be permitted to bring credit to their assertions. And these instances, added to what has been already said, may, I hope, excuse me if I think it not time mispent to consider whether there may not be other, and even unobserved sorts of effluvia in the air, to excite your curio-

sity

sity and attention about which, rather than to declare a positive opinion, is that which is pretended to in what has been lately mentioned.

AND whereas, *Pyrophilus*, I have in the former discourse taken in the structure and established laws of the universe, as an help toward the giving an account of the cosmical attributes of things, I shall here also ingeniously confess to you, that I much fear, whether we have yet attentively enough taken notice either of the number, or the kinds of those laws.

FOR as I am by some notions and observations inclined to think, that there may be a greater number even of the more general laws than have been yet distinctly enumerated, so I think that when we speak of the established laws of nature in the popular sense of that phrase, they may be justly and commodiously enough distinguished; some of them being general rules that have a very great reach, and are of greater affinity to laws more properly so called, and others seeming not so much to be general rules or laws, as the customs of nature in this or that particular part of the world; of which there may be a greater number, and those may have a greater influence on many phenomena of nature, than we are wont to imagine.

AND first, whereas the structure of the world is a main help in our present disquisition, I shall venture to tell you, that though I do not only commend, but in divers cases admire the industry of astronomers and geographers, especially of some later ones; yet they have not met with such difficulties, that they have hitherto presented us, rather a mathematical hypothesis of the universe, than a physical, having been careful to shew us the magnitudes, situations, and motions of the great globes, such as the fixed stars and the planets (under which one may comprize the earth) without being solicitous to declare what simpler bodies, and what compounded ones, the terrestrial globe we inhabit does or may consist of. And as of late years the discovery of the four planets about Jupiter, and the little moon (as some call it) that moves about Saturn, together with the phenomena of comets, have obliged the skillful to alter divers things in the theory of celestial bodies; so I know not but that future discoveries by improved telescopes and other philosophical instruments may reduce us to make changes in the grand system of the universe itself, and in that which we consider as the most important of the mundane bodies to us, the terraqueous globe we live on.

WHAT communication this may have with the other globes we call stars, and with the interstellar parts of heaven, we have very little knowledge of, though I may elsewhere make it probable, that there may be some commerce or other; but without speaking more particularly of that point, I confess I have some time suspected that there may be in the terrestrial globe itself, and the ambient atmosphere, divers, whether laws or customs of nature, that belong to this orb, and may be denominated from it, and seemed to have been either unknown to, or overseen by both scholastical and mathematical writers. And first, I have often suspected, whether there may not be in the mass of the earth some great, though slow internal change (whether originated there, or produced by the help of other mundane globes) by considering that almost in all countries, where observations have been made, there has been a plain and considerable alteration found in that which is commonly called the variation (for it is rather the declination) of the sea compass or magnetick needle, which is the distance by which the needle declines east or west from the true north pole. And whereas formerly, at or

\* near *London* the compass declined, as observations solemnly made, and upon record assure us, in the year 1580, above 11 degrees; in the year 1612, above 6 degrees;



in the year 1633, no less than about 4 degrees; it has of late been found to have very little or no variation. And at a place within half a league of *London*, trying with a long and curious needle, purposely made and poised, I could scarce discern any declination at all, and if the needle declined sensibly any way from the pole, it seemed to do so a little towards the other side of heaven, than that towards which it did decline before. And having † afterwards by the help of a meridian line, much prized for having been accurately drawn by eminent artificers, made an observation in *London* itself; though I made it with two instruments, whereof one was a choice one, differing from the former, and from one another, I could not satisfy myself that I could discern the declination of the needle to exceed half a degree, if it amounted to so much. But since observations of this kind may prove more considerable than we yet know of, and since they ought to be made at distant places, I am contented to add here by way of confirmation, that the *Cape of Good Hope* being one of the eminentest parts of the terrestrial globe in reference to magnetisms, the acquaintance I had with one of the ancientest and most experienced navigators of this part of *Europe*, invited me to address myself to him, purposely to enquire of him, whether he had taken the variation of the compass at the *Cape of Good Hope*; and whether, if at all, he had taken it more than once, he answered, that he had often done it; whereupon asking him what he found the variation to be, and whether he had observed any change of it in his several voyages, he replied, that when he was a young seaman, he observed the variation to be about two degrees westward, and afterwards, during many years that he sailed to and fro betwixt *East-India* and *Europe*, he found the variation to encrease by degrees; and whereas he had learned from ancient writings and the tradition of old seamen, that before his time they had found no variation at all, he about 15 years ago (which was the last time he took it) found it by accurate instruments, to be 6 degrees and about 48 minutes. So that during the time that he practised the seas about the *Cape of Good Hope*, the variation still westward had increased near 5 degrees. Upon these grounds, which I may elsewhere have occasion to confirm by further observations, I cannot but think it probable that there may be agents that we know not of, that have a power to give the internal parts of the terrestrial globe itself a motion; of which we cannot yet certainly tell according to what laws it is regulated, or so much as whether it be constantly regulated by certain laws or no. And what other changes, agents that can produce a change in the terrestrial globe itself may make in this, or that part of it, who can inform us?

In the next place I consider the great uncertainty and irregularity that we have hitherto observed in the weight of the atmosphere by our new statical barometers, and much more sensibly by mercurial ones, without yet having discovered the causes of such considerable alterations in the air (save that in general they proceed for the most part from subterranean steams) whose influences upon other things may be more considerable than we have yet had opportunity to detect.

It is very remarkable what a late and ingenious writer, that lived in some of the American islands, relates about the hurricanes in those parts; namely, that before the Europeans came thither, the inhabitants observed, that they had those fatal tempests once in seven years, and no oftener; afterwards they were troubled with them but once in six years; and in process of time, the unwelcome visits of those winds grew so frequent, that in my relator's time they came once a year; and, as a prodigy, they once observed two in one year; and afterwards three in another. I remember also, that

Monsieur  
de Roche-  
fort.

† A. D. 1669.

meeting with an inquisitive gentleman that had lived in *New England*, I desired to know of him, whether in that part of the country where he resided, there were not a great change made in the very temperature of the climate? whereto he answered me, that there was, for it was grown much milder than formerly; and because I doubted whether this change might not have been either accidental for a year or two, or apparently to the English, whose bodies by degrees might grow more accustomed to the coldness of the country, and less sensible of it; it was answered, that this change had been observed for many years after the English had planted a colony there, and that the change was manifestly perceived by the natives too, by the remissive operations of the cold upon running and standing waters, which were formerly wont to be frozen at such and such times. And I shall add for confirmation, that having one day the honour to be standing by his majesty when he received a solemn address from *New-England*, delivered by the governor of a colony there; that very inquisive monarch, amongst other questions, asking him about the temperature of the air, he told his majesty, in the presence of divers that came from *America* with him, "that the climate had much altered and lost much of its former coldness for divers years, since the English settled there."

WHETHER this decrement of the sharpness of the air will proceed, or how long it will continue, time will discover; but in the mean while, supposing with him the matter of fact to be true, and that the change depends not on any manifest cause; that which is happened already, seems to me very considerable, since I have lighted on a book \* written by † one of the ancient planters of *New England*, by way of description of that country; where, among other things, I find those notable passages. The one in the seventh page: In former times, says he, the rain came seldom, but very violently, continuing its drops, which were great and many, sometimes 24 hours together, sometimes 48, which watered the ground for a long time after; but of late the seasons are much altered, the rain coming oftener, but more moderately, with less thunder and lightnings, and sudden gusts of wind. And the other in the 84th page; where speaking of the heathen natives, he says, They acknowledge the power of the Englishman's God, as they call him, because they could never yet have power by their conjurations to damnify the English either in body or goods; and besides they say, he is a good God that sends them so many good things, so much good corn, so many good cattle, temperate rains, fair seasons, which they likewise are the better for since the arrival of the English; the times and seasons being much altered in seven or eight years, free from lightning and thunder, long droughts, sudden and tempestuous dashes of rain, and lamentable cold winds.

So that by this it appears, that this grateful decrement of the coldness and rudeness of that climate was already taken notice of so many years ago.

To these relations may pertinently be subjoined a passage of the learned *Magnenus* in his ingenious little tract *de Mannâ* ‡; where he very solemnly delivers this notable observation, that in the country he calls *Cenotria*, there was no manna to be found a

\* Intituled, *New England's prospect*.

† Mr. W. Wood.

‡ The book was published thirty-five years since.

§ Sanctorum naturæ interpretum nullus fraxinum inter arbores gummiteras, aut resiniferas recensuit. Illud omnino, quo *Allomatus* sese jactare videtur, ignoravere curiosissimi rerum indagatores, *Plinius*, *Galenus*, *Theophrastus*, & qui medium ætatem impleverunt viri doctrinâ diligentissimæ celebres; quia scilicet illis temporibus multum pluebat in *Calabria* manna, quod à duobus tantummodo seculis legi ceptum. Dicamabo, *Altomate*, cur ante trecentos annos multum manna fuit in *Cenotriâ*: jam certe aderant pagi ibidem urbesque vicine, neque vero seculis curiosam incolarum solertiam nihil plane video, quod pro te adduci possit ad hujus difficultatis evitandas angustias. *Magnenus de Manna*, P. M. 49.



little above three hundred years ago : and that in *Calabria* itself, a province so famous for manna, that the best is denominated thence, and that furnishes a great part of *Europe* with that odd drug, it is but since two ages, or thereabouts, that manna has fallen, or, as he expresses it, rained.

I know not whether it may be worth while to mention, after these more weighty observations, the oeconomical traditions of house-wives, which I should not think worth taking notice of in this place, but that having purposely enquired after the truth of it, of two very sober persons (much versed in the art of making sweatmeats) that have, especially one of them, often tried it ; they seriously affirmed to me that they find the spots made in linen by the juices of fruit (particularly of red currants) in straining bags, will best wash out (nay scarce otherwise) at that time of the year when those fruits are ripe the ensuing year.

To which may be for affinity's sake annexed, what is related by the ingenious French writer of the history *Des Isles Antilles* \*, where he lived divers years ; who, speaking of the fruit they there call *Acajou*, tells us, that the juice of some of the internal parts of it, though reputed an excellent remedy in fainting fits, is of such a nature, that if it chance to fall upon a piece of linen, it turns to a red spot, which lasts till the tree come to be again in flower ; which phenomena, if the length of time, and the heat and temperature of the air usual in the seasons of producing blossoms and ripening of fruits, be found to have little or no interest in their causation, may prove of some use in our present enquiry.

WHATEVER be the true cause of the ebbing and flowing of the sea, yet at spring-tides the motions of such vast masses of matter as the great ocean, and most of the seas, are so constantly co-incident with the new and full moon ; and the more stupendous spring-tides have been in most places so long observed to happen regularly enough about the æquinoxes, that it is worth an enquiry (though I cannot here afford it one) whether these conspicuous phenomena may not somewhat confirm the conjectures we are discoursing of.

AND when I remember how many questions I have asked navigators about the luminousness of the sea ; and how in some places the sea is wont to shine in the night as far as the eye can reach ; at other times and places, only when the waves dash against the vessel, or the oars strike and cleave the water ; how some seas shine often, and other have not been observed to shine ; how in some places the sea has been taken notice of to shine when such and such winds blow, whereas in other seas, the observation holds not ; and in the same tract of sea, within a narrow compass, one part of the water will be luminous, whilst the other shines not at all : when, I say, I remember how many of these odd phenomena belonging to those great masses of liquor I have been told of by very credible eye-witnesses (whose narratives to me you may elsewhere meet with) I am tempted to suspected that some cosmical law or custom of the terrestrial globe, or at least of the planetary vortex, may have a considerable agency in the production of these effects.

NOR am I sure that some subterranean changes, or some yet unobserved commerce between the earth and other mundane globes, has not an interest in the origin, continuance, and expiring of those diseases that physicians call new, which invade whole countries (and sometimes greater portions of the earth) and last very many years, if not some ages, before they come to be extinct : of which sorts of diseases divers learned men have reckoned up divers, and whereof the venereal pox at least, as to its origin and

\* *Histoire Naturelle des Isles Antilles. Liv. I. Chap. 6.*

spreading, is but too manifest and unhappy an instance; whereto, according to some eminent doctors, we may add the rickets, a disease which, though scarce known in other countries, is here in *England* so fatal to children, which first (as is affirmed) discovered itself among us within the memories of multitudes of men yet alive: but of this perhaps more elsewhere.

If I should now further descend to the peculiar phenomena of particular regions, I must launch out into a discourse I could not have the leisure to finish; and therefore I shall only advertise you of two suspicions more, that I hold not unfit to intimate to you, about the established laws and customs of nature.

THE first of them is this, that I doubt those that are thought the grand rules whereby things corporeal are transacted, and which suppose the constancy of the present fabrick of the world, and course of things, are not altogether so uniformly complied with as we are wont to presume; at least, as to the lines, according to which the great mundane bodies move, and the boundaries of their motions; for what reason the wise author of nature pleased to permit that it should be sometimes, as it were, over ruled by the boisterousness (if I may so call it) and exorbitant motions of unruly portions of matter, I must not in this place (though I do it in another) inquire: but when I consider the nature of brute matter, and the vastness of the bodies that make up the world, the strange variety of those bodies which the earth does comprize, and others of them may not absurdly be presumed to contain; and when I likewise consider the fluidity of that vast interstellar part of the world wherein these globes swim, I cannot but suspect there may be less of accurateness, and of constant regularity, than we have been taught to believe, in the structure of the universe, and a greater obnoxiousness to deviations than the schools, who were taught by their master *Aristotle* to be great admirers of the imaginary perfections of the celestial bodies, have allowed their disciples to think. And in effect, to speak only of the noblest of them, the sun, and to pass by about his motions the observation of the exactest astronomers, that natural days are not all of equal length (whatever the vulgar of philosophers suppose to the contrary); and not to take notice of the great dispute betwixt the eminentest astronomers, even of our times, about the anomaly attributed to the motion of the sun's apogee: to pass over these things, I say, the sun himself doth not only, from time to time, do what divers of our later astronomers stile to vomit out great quantities of opacous matter (which are called his spots) some of them bigger, perhaps, than *Europe* or *Asia*, but has had almost his whole face so darkened with them (as about the end of *Cæsar's* and the beginning of *Augustus's* government) that for about a year together he was, as it were, under an eclipse. To which if we add those celestial comets (for I dispute not now about sublunary ones) their number, vastness, duration, odd motions from orb to orb (as the ancients would have spoken) and other phenomena (whatever the causes of them be) it will appear, that even in the celestial part of the world all is not so regular and unvariable as men have been made to believe.

I HAD some doubts whether this might not be much confirmed by what has been related by some navigators that have been in the south-sea, about certain black clouds said to move as regularly in the antartick hemisphere, as the neighbouring stars themselves; to which some of our English seamen (whether first or no, I know not) have added, certain white clouds in the same hemisphere move no less regularly. Of these relations, I say, I considered whether some use might not be made to my present purpose; but having made the best enquiry I could of those few persons of note I could meet with, that were likely to inform me, I do not yet see cause to alledge these phenomena by way of arguments. But yet, since I find that even pilots who have been frequently



frequently in some parts of the *East-Indies*, have not (whether because they failed not far enough to the southern pole, or upon some other score) taken notice of them, I shall subjoin as a part of natural history, not obvious to be met with, the best account I could procure of them; which was from an observing captain of an East-India ship, with which he lately adventured to unfrequented parts of the south-sea.

THE substance of his answers to me about the forementioned phenomena was this, that he had divers times seen in the southern hemisphere, and in that part of the milky way, which is not to be seen upon our horizon (for he says, the galaxy is either completely, or almost a circle) two or three places that look like clouds, and move about the earth regularly with the white part of the circle in 24 hours. But by what he replied to some further questions that I asked him, I gathered, that if these be the black clouds that navigators have spoken of, those that gave them the name of clouds were probably much mistaken; since, he answered me, that these are not black, but of a deep blue, which makes me suspect them to be but perforations, if I may so speak, of the milky way, by which I mean parts of the azure-sky that are suffered to be seen by the discontinuations of the parts of the galaxy. And to this account of the dark clouds, his further answers gave me this of the white ones; which, he says, some call the Magellanick clouds, about which he related;

THAT he had divers times seen towards the south-pole, the clouds that some few navigators mention to be there, and to move about the pole in 24 hours.

THAT he began to discover them plainly, when he was in about 18 degrees (as I remember) of south latitude.

THAT they were white, in number three (though two of them be not very distant from each other) the greatest being far from the south-pole; the other not many degrees remoter than that star, which of the \* conspicuous ones, they reckon to be nearest to the pole; though it be about eleven degrees distant from it.

BUT from this account of his I dare not, as I was intimating, conclude these to be such clouds as they are taken for, because, for aught I know, if they were looked on through a good telescope, they would be found constellations of small and singly inconspicuous stars, like those of the galaxy, the belt of Orion, &c. but to be resolved about these matters, it is not amiss to expect further observations, the proposed conjectures being made but upon a supposition of the truth and sufficiency of the relations.

AND thus much for the first of the two suspicions that I above intimated I would propose to you: the other is very different from it, and might seem contradictory to it, but that they belong not to the same cases; for though I lately told you I suspected that in some things, especially relating to the lines, according to which, and the limits within which some great masses of matter are supposed to perform their motions, there is more accurateness fancied than there really is; yet I shall now add, that there are cases wherein I am not quite out of doubt, but that we may sometimes take such things for deviations and exorbitancies from the settled course of nature, as, if long and attentively enough observed, may be found to be but periodical phenomena, that have very long intervals between them; but because men have not skill and curiosity enough to observe them, nor longevity enough to be able to take notice of a competent number of them, they readily conclude them to be but accidental extravagancies, that spring not from any settled and durable causes; for the world, like a great animal, producing some effects but at determinate seasons; as nature produces not beards in men till they have attained such an age, and the menses (as they call them) use not to happen to wo-

\* This wary expression keeps my relator from being contradicted by a curious modern astronomer, who tells of a star not three degrees distant from the southern pole; but then he says too, that it is a star of but the fifth magnitude.

men before they come to such years, nor to last beyond such other years of their life; as may be also observed within a far shorter compass of time in the growth and falling of flags horns and bucks: if the first man had lived but one year in the world, he would perhaps have thought the blossoming of trees in spring, and their bearing fruit in summer, but an accidental thing, and would have looked upon the eclipse of the sun as a prodigy of nature; observing, that though every new moon the sun and she came very near together, yet neither before nor after was there any such terrible phenomena consequent thereupon. And we ourselves may easily remember what strange conjectures we had of the strangely varying appearances of Saturn, for divers years after our telescopes first discovered them to us.

BUT most remarkable is that celestial phenomenon afforded us by the emerging, disappearing, and re-appearing stars of this age; which have been observed in the girdle of *Andromeda*, and in or about the swan's breast (which is said to have been seen in the year 1600, and to have vanished in 1621) and especially that, which having about 25 years ago appeared for a while in the whale's neck among the fixed ones, and afterwards by degrees disappeared, was looked upon by those astronomers of that time, who did not out-live it, as a celestial comet: but afterwards an ingenious English gentleman of my acquaintance having observed here (as well as the vigilant curiosity of some few later astronomers hath taken notice of elsewhere) the return of the like phenomenon in the same part of heaven; it begat much wonder in all (which was increased by the slow disappearing of it) and in some curious men a resolution to have a watchful eye upon that part of the sky. Since when the justly famous *Bullialdus*, and besides some eminent foreign virtuosi (whose names I know not) divers excellent persons of our own nation having taken notice of it in the wonted place (where I had sometimes the satisfaction of seeing it); these observations, and especially the last disappearance of a star judged to have been placed among the fixed ones, and estimated to be of the fourth (if not the third) magnitude, have somewhat confirmed me in the suspicion I am now treating of. For if this and the other new stars do continue to return periodically to the same part of heaven, where they have been already long ago seen; as at least for as much as concerns this, its gradual increasing after it first begins to shew itself, and decreasing afterwards, seem to promise; then I may with somewhat more of probability than before, suspect that there may be vortices beyond the concave surface of what we call the firmament; which suspicion, if true, would much disfavour the hypothesis we now have about the system of the world, and will favour what I conjectured as possible about periodical phenomena. And however, if either the new star, without departing from its place, be only sometimes by degrees overspread and hid by spots, like those I formerly mentioned to have obscured the sun, which are afterwards by degrees dissipated, as I at first suspected; or if it have a dark hemisphere as well as a light one (or rather a greater part of its globe obscure than luminous, as *Bullialdus* ingeniously conjectures) and by turning slowly about its own center and axis, doth sometimes obvert to our eyes its luminous part, and sometimes its dark part (as Jupiter is said to do its belt-like spots, whence it must gradually both appear and disappear; according to either of these two hypotheses (though not so much as in that which preceded them) there will be reason to question the great uniformity imagined to be in the celestial bodies and motions; and to favour what has been proposed about periodical mutations in the mundane globes; especially since these phenomena argue, that even those stars we call fixed, and have looked upon as so invariable, are subject to mutations great enough to be taken notice of by our naked eyes at so immense a distance. I shall not here prosecute this discourse, because I would not anticipate what I foresee I shall have occasion to say about

the



the terrestrial effluvia, with their causes and effects, in another discourse \*, but I think myself obliged to mind you in this place, that doubts and suspicions are the only things promised by the title of this discourse; and therefore I shall not quarrel with you, if you conjecture, that though the last proposed suspicion may prove well grounded in some cases, yet in some others, the exorbitancies of the matter may, if they chance to be repeated, occasion a new custom, that may have the force of a law in this, or that part of the mundane globes, particularly in this terrestrial one we inhabit; as waters, by their frequent overflowings of the banks that cannot contain them, do sometimes make themselves new passages by their own deviations, and as it were, affect to run in the channel they once made. And as it happens also in animals, that noxious humours having once found a vent at an issue or an ulcer, do constantly take their course that way; which brings into my mind this odd observation, that having occasion to pass some years ago out of *England* into *Ireland*, traversing the maritime county of *Waterford*, the convoy that went with me shewed me once in my way, at a pretty distance off, a mountain, from whose higher parts there ran precipitously a river (which by my estimate was pretty broad) that within but two or three years before, at furthest, first broke out without any manifest cause from a great bog, that had been immemorably at the top of that mountain, and to the wonder of the inhabitants, after the first eruption of the water, had supplied the country with a river ever since: the circumstances of which new phenomenon I would gladly, at a nearer distance, have observed, but the convoy was not fond of a curiosity so dangerous in an enemy's country.

OTHER instances to the same purpose I cannot now conveniently stay to present you, having already made the conjectural part of this essay disproportionate to the other: and I hope there is already enough said in this latter part to answer my design, which was to excite your curiosity to seek after some certainty touching the things doubted of; and strive to enable yourself by watchful observations, somewhat to ease me of the troublesome suspicions I have confessed to you, by telling me whether they are altogether groundless or not.

\* The reference here-made is to a Tract about the Effects and Causes of some unheeded Changes in the Air.



OF THE  
 T E M P E R A T U R E  
 OF THE  
 SUBTERRANEAL REGIONS,  
 As to HEAT and COLD.

A D V E R T I S E M E N T.

**T**HE two following tracts were designed to have been accompanied by three or four others, whereof the first treated about the temperature of the regions of the air, as to heat and cold, and had been premised to the two that now come forth, had it not been judged more proper to reserve them to accompany some other papers concerning the air. To the following tract about the submarine regions, it is thought fit to adjoin some relations about the bottom of the sea; to which was to have been added some observations concerning the saltness of the sea; but in that treatise, some blanks having been left for particulars, which the author could not seasonably find among his loose papers to fill them up with, these that now appear, having no dependance on them, it was not thought fit they should stay any longer for them.

BUT about these several tracts, this general advertisement is to be here given, that being historical pieces, consisting chiefly (though not only) of such particulars as the author must owe to the informations of others, he would not stake his reputation for the truth of every one of them; contenting himself to have performed what can be reasonably expected of him; which is, that he should carefully make his inquiries from credible persons, who, for the most part, deliver their answer upon their own knowledge; and that he should faithfully set down the accounts he procured from such relators.

*Of the TEMPERATURE of the SUBTERRANEAL REGIONS, as to  
 HEAT and COLD.*

C H A P T E R I.

**I**F when I used to visit mines, I had thought of writing on the subject I am now about to treat of, and had designed to satisfy myself about the temperature of the subterranean air, as much as I did about the other subjects I was then concerned to be informed of,



of, I think I should have enabled myself to deliver much more upon my own observation than I shall now pretend to do. But though for the reason newly intimated, and because of my being particularly subject to be offended by any thing that hinders a full freedom of respiration, I was not solicitous to go down into the deep mines; yet, after having discoursed of the temperature of the air above ground, I presume it may not be improper or unwelcome to say something of the temperature of the subterranean regions and of the air reaching thither: for deep mines being places which very few have had the opportunity and fewer have had the curiosity to visit, and of which I have scarce found any thing at all observable by classick authors, and by other writers but very little, especially that I think probable enough to make use of; I presume it will not be unacceptable to you, if, of regions so little frequented and less known, I report what I have been able to learn (by diligent enquiry purposely made) from the credible relations of several eye-witnesses differing in nation, and for the most part unacquainted with each other.

THOUGH I do not think it absurd to suspect, that in some places of the earth the peculiar constitution of the soil and other circumstances may make it reasonable to assign those places fewer or more regions than three; yet, speaking in the general, the ternary number seems not inconvenient to be assigned to the subterranean regions; not so much upon the score of the analogy that by this division will be established between the regions of the earth and of the air, as because there seems to be a reason of the division included in the division itself. And indeed experience appears to favour it in the subterranean cavity that I have hitherto been able to procure an account of from any ocular witness, and (very few excepted) one of the deepest that we yet know of in the world. And since it has been received for a rule among philosophers, that which is perfectest or completest in its kind ought to be the standard whereby the rest are to be measured or estimated, I shall begin the remaining part of this essay by a relation that I obtained from a chemist that had purposely travelled into *Hungary* and other places, to visit the mines those parts are justly famous for; and who bringing me the honour of a compliment from a prince, to whom he belonged, gave me the opportunity of asking him divers questions, his answers whereunto (which I presently after put into writing) afforded me the ensuing account.

## C H A P. II.

THAT very near the orifice of the groove he felt the air yet warm; but afterwards descending towards the lower parts of the groove he felt it cold until he came to such a depth as he had scarce attained by a quarter of an hour's descent, and that the cold he felt during this time seemed to him considerable, especially when in descending he had reached to a good depth.

THAT after he had passed that cold region he began by degrees to come into a warmer one, which increased in heat as he went deeper and deeper; so that in the deeper veins he found the workmen digging with only a slight garment over them; and the subterranean heat was much greater than that of the free air on the top of the groove, though it were then summer.

[WHAT is here mentioned of a cold region in the earth has been since confirmed to me by an ingenious physician upon an observation made in another Hungarian mine (near a town whose name I remember not) that was not of gold, but copper, and of much lesser deepness than that newly spoken of; for this relator answered me, that in going

going down he felt a considerable degree of cold; and when I asked whether he found the like in his return upwards, he told me he observed it then too: and when I further inquired after the extent of this cold region, he replied, that not expecting to be asked about such circumstances, he had not taken particular notice of them; but thus much information my questions procured me, that he began to feel the above-mentioned coldness, when he could receive no more light at all by the mouth of the groove; and that this cold region lasted till he came somewhat near the bottom, which was estimated to be about an hundred fathom or more distant (in a straight line) from the top.

THIS relation agrees well enough for the main with that short but considerable one of *Morinus*, which I elsewhere cite, who, above forty-five years ago, visited the deep Hungarian mines in the month of July, and takes notice, that when he came down to the burrows, as he calls them, he did not find any heat as at the mouth of the well, but the beginning of a very cold as well as considerably thick region; though I easily believe him when he confesses that he felt it much the colder, because he had left off his own cloaths and put on the slight garments used there by the diggers. He further informs his reader, that when they had descended about 80 fathoms beneath the surface of the earth, he began to feel a breath of an almost lukewarm air; which warmth increased upon him as he descended lower, pleasing him not a little, because it freed him from the troublesome scents of his former coldness; adding, that the overseer of the mine, who conducted him, affirmed to him, as also the officers of other Hungarian mines unanimously did, that in all their mines, at least all the deep ones, after a thick tract of cold earth there succeeds a lower region that is always hot; and that after they arrived at such a depth they felt not any more cold, but always heat, how deep soever they dig; and to add upon the by, though this learned man lay much weight upon antiperistasis, yet in the next page to those that contain what I have been just now relating, he either very candidly or inconsiderately takes notice that they informed him, that in their mines, whether more or less deep, they observed, that at some times in the year a somewhat intenser heat was felt; and the two times that he expressly names are those oppositely qualified seasons of summer and winter.

HAVING laid down these general narratives, I now proceed to consider the earth's regions in particular; about which the sum of what I yet have to propound may be conveniently enough comprized in the four following propositions.

### C H A P. III.

#### P R O P O S I T I O N I.

“THE first region of the earth is very variable both as to bounds and as to temperature.”

THE former part of this observation will not be difficult to prove, since it will be easily granted that the manifest operation of the sun-beams is *ceteris paribus* greater, and reaches further in hot climates than in cold ones; in the midst of summer than in the depth of winter.

THE second part of the observation may be proved by the same arguments as the first; to which may be added, as to some places, the solidity or porousness of the earth; as also the nature of some salts, marshallites, and other bodies contained in it, which by their natural temperature may dispose the soil to coldness or heat, as I shall have occasion to shew when I come to speak of the second region.



IN the mean time I have this to observe further, that in this first region the air is usually more temperate, as to cold and heat, than that above the surface of the earth; and that this region is not wont to be considerably deep, both parts of which observation are capable of being made good by the same reasons, and therefore I shall endeavour to prove them jointly.

THAT in the uppermost region of the earth it should be less cold than above the surface seems reasonable to be allowed upon this consideration, that the subterranean cavities of the earth are sheltered by the thickness of the sides from the direct action of the sun-beams, the winds, &c. and is also kept from an immediate, or at least from so full a contact of the external air, when that is vehemently either heated or refrigerated.

AND first as to the heat of the sun, that that does much less powerfully affect such places as are sheltered from its action by solid bodies, may appear by the conservatories of ice and snow, wherein frozen water is kept in that state during all the heat of summer, and that oftentimes in cavities that are at no considerable depth beneath the superficies of the earth: nay I remember, that having had occasion (for the perfecting of some conclusions I was trying) to keep ice many weeks after the frosty weather was gone and a milder season was come in, I was able to do it, contrary to the expectation of some curious men, without either digging to a notable depth in the ground or building any substantial structure over the cavity. For wanting conveniencies, I contented myself, though it were in a champain place, with a pit somewhat broad at the bottom, of about four feet deep or less, whose mouth was sheltered only by a little low thatched hovel that was wide open to the north, and only screened the mouth or vent of the little pit from the direct beams of the sun: and though I will not deny that in deep conservatories of snow, the natural coldness of the earth, especially in some places, may contribute to the effect; yet I remember, that discoursing once with a traveller and scholar that was born in hot countries, of a conjecture of mine, that in an arched building whose walls were sufficiently thick and whose air were carefully kept from all avoidable intercourse with the external air, one may, without digging so much as a man's depth into the ground, make a sufficient conservatory for ice in very open and unsheltered places, and even such as *Salisbury Plain* itself; discoursing, as I began to say, with this traveller about this conjecture, he told me, that at a place he named to me, in the southern part of *France*, whose heat seemed to me to exceed that of divers parts of *Italy*, some curious persons that were resolved at any rate to have ice in summer, though the soil were such that they could not dig four feet without meeting with water, were yet able to make use of conservatories, by covering the brick building they made over their pits with clay and sand to a very considerable thickness, and taking care that the only place that should permit access to the outward air should be a small northern door to go in and out at, fitted to shut exactly close, and fenced with a little porch furnished with another door: and by this means he affirms these gentlemen to reserve the included ice, not only all the summer long, but sometimes for two or three years together, the heat of that region making many of their winters too mild to recruit them with ice.

To all these things I shall add, that even where the intercourse is not quite debarred, but left free enough betwixt the subterranean and the superior air, the operation of the sun beams may be very much less in a cavity though but shallow beneath the surface of the ground than above it. For besides that trials have informed me that liquors, that differ in little else than in consistence, will not so easily pervade each other as a man would suppose, unless some external motion hasten their intimate mingling with one another; I remember, that one morning pretty late, having had the curiosity to descend into a pit where they were digging out iron ore, though this cavity had no very narrow ori-

fice, and was dug directly downwards, and exceeded not ten or twelve feet in depth, yet I found not the heat at all troublesome whilst I staid there, though the pit was in an open field, unshaded by trees, and though the air abroad were much heated at that time of the year, which was in that season (or at least very near it) that is wont to be called the dog-days.

## C H A P. IV.

AND as we have shewn that the subterranean air, even in the first region, is usually much less heated than the superterrestrial air, so we may easily observe, that that inferior air is (*cæteris paribus*) wont to be much less refrigerated by the grand efficient of intense cold than the superior air.

I WILL not urge on this occasion what I have observed by a surer way than for aught I know has been before practised, about the smocking of some springs in frosty weather; because I do not know but that those springs may have come from or passed a good way through some place very deep beneath the surface of the directly incumbent ground, and perhaps from a soil peculiarly fitted to warm them; whence the water may have derived a warmth considerable enough not to be quite lost, till it began to spring out of the ground, where it needed only not to be quite cold, to appear to smoke; the intense coldness of the air making those exhalations visible in frosty weather which would not be so in milder, as is evident in a man's breath, which appears like a smoke in such weather, though it be not visible in summer.

THAT therefore, which I shall propose in favour of our observation, is first taken from the nature of the thing, which may persuade us that the subterranean air being, though comparatively cool, yet indeed moderately warm in summer, ought not to be affected with winter's cold so much as that contiguous to the surface of the earth, from whose immediate contact it is by a thick arch of earth, if I may so call it, defended; and that the cold reigns most in the free air and the superficial parts of the terrestrial globe, may appear by water's beginning to freeze at the top, not at the bottom; to which reason, from the nature of the thing, I shall add only this from experience, that we see that in cellars that are arched and carefully kept close from the communication of the outward air, beer and other liquors may be kept from freezing in frosty and snowy weather; as I have observed in a cellar that was but shallow but well arched, in a winter that was sharp to a wonder and froze stronger liquors than beer in another cellar very near it that differed not much from it in depth, but had not so thick and solid a roof: and that not only here in *England* where the cold is less violent, but even in *Russia* itself where it is wont to be so extreme, it reaches not near so deep as one would think, I learned by inquiry purposely made of an ingenious physician that lived at *Moscow*, who answered me that others, and he himself, did in that city keep all the winter long, not only their wine but their beer from freezing, in cellars that were not above twelve or fourteen feet deep, but well covered above and carefully lined with planks of fir, without any entrance but a small trap-door (commonly at the top) which was fitted so exactly to the orifice it was to close, as to exclude, as much as was possible, all communication between the internal and external air, that the latter might not affect the former with its coldness.

I HAVE indeed suspected, that in some cellars the comparative warmth we find there may be partly due to subterranean exhalations that are pent up in them, and perhaps too in some measure from the steams of the fermenting or fermented liquors lodged in those places:



places: and I was somewhat confirmed in this suspicion by an information my inquiries obtained from the newly mentioned doctor, who told me upon his own observation, that in one of the cellars he made use of at *Moscow*, having occasion to open the above mentioned trap-door after the cellar had for a good while been kept very close shut, there came out at the vent that was thereby given a copious steam in the form of smoke, which to them who had their bodies affected with the external air, was very sensibly warm, and was almost unfit for respiration; which circumstance increased my suspicion that there might be among these steams some of the nature of those that have been observed to come from fermenting liquors, especially wine, and so abound in some cellars as almost to stifle those that ventured into those vaults, and to kill some of them outright; which effects the long abode of subterranean steams in stagnating air, even in many places where no metalline ores at all nor other noxious minerals have been found, has enabled that air to produce, of which divers sad instances have been given within less than a mile of this place, upon men's first going down into pits or wells that had not in a long time been opened or made use of; but this is here mentioned only upon the by; nor have we any necessity to fly to subterranean exhalations for the comparative warmth that good cellars in general afford in frosty weather, since that phenomenon may be accounted for by the reason formerly given, that the closeness of the cavity and the thickness of the sides and roof keep it from being vehemently affected with the cold of the ambient air.

I know it is pretended that the warmth we speak of proceeds from an antiperistasis; but not now to engage in a controversy that would take up too much time, it may here suffice to represent, that in our case there appears no necessity of recurring to it, the phenomenon being solvable by the region newly cited, which may be confirmed by this experiment, that in the vaulted cellar above mentioned wherein beer was kept from freezing in an almost prodigiously sharp winter, the included air, though sensibly warm to those that came out of the free air, had not so intended its native heat as the asserters of antiperistasis would have expected, being colder than the free air commonly is in that place, not only in the heat of summer, but in other seasons, when the weather is temperate, as I was assured by comparing my own observations made at other times, with the account brought me by a skilful person whom I employed in that cellar at late hours in one or two of the sharpest nights of the forementioned cruel winter, with the same excellent sealed weather-glass that I had long kept suspended within a stone's cast of that place.

## C H A P. V.

HAVING said thus much about the earth's uppermost region, I now proceed to that which lies next beneath it, whose temperature I cannot so conveniently give an account of in less than two propositions, whereof the first is this:

### P R O P O S I T I O N II.

“THE second region of the earth seems to be for the most part cold in comparison of the other two.”

This proposition may be confirmed partly by reason and partly by experience.

T t 2

AND

AND first, it seems consonant to reason, that since the earth is naturally a body consisting of gross and heavy parts that are usually much less agitated than those of our organs of feeling, it should, as to sense, be cold; and that therefore that quality may be justly ascribed to it in that region, where, by virtue of its situation, it is kept from being considerably affected either by the heat of the superior air, or by that of the deep parts of the earth; which upper and lower heat are the two agents that seem of all others the most likely to put its parts into an unusual motion, and thereby change its natural temper.

THAT our proposition is also confirmable by experience, may be gathered from the relations set down in the former part of this discourse.

AND here it will be proper to take notice of the advertisement intimated in the close of our above delivered proposition, that this coldness ascribed to the second region of the earth is to be understood comparatively to the other two. For otherwise that even this earth is not, as many naturalists would have it, the *summum frigidum*, I gather from this, that I could never hear of any ice met with there at any time of the year, though snow or hail may be produced in the middle region at differing, and sometimes quite opposite seasons of the year; nay, I have not found by the answers that were made me by those that have descended far enough into this region, that they found the cold any where very great, or that in some places they have found it at all considerable; as we shall see in the explication of the next proposition. I know not whether it will much strengthen what has been said, if I add, that I learned by enquiry of such persons as I lately mentioned, that at the mouth of deep grooves, in mines, the steams that ascend do often feel warm; though the outward air, where the observation is made, be affected with the heat of summer: but though this probably argues, that if the middle region of the earth through which these steams must ascend were very intensely cold, they would be so refrigerated in their passage as to feel rather cold than hot at their appearing above ground, especially in summer; yet I shall not lay much weight (for some may perhaps be allowed it) upon this argument, because I have not yet tried how far a warm steam may be altered in its passage through a cold conduit; not to mention, that in the earth, the passage, by being directly upwards, may be much the nimbler traversed.

## C H A P. VI.

THE second proposition relating to the temperature of the second region of the earth may be delivered in these terms:

### PROPOSITION III.

“In several places, which, by reason of their distance from the surface of the earth, one would refer to the middle region of it, the temperature of the air is very differing at the same times of the year.”

I chose to express myself thus to prevent some ambiguities and objections which I foresaw that shorter, but less clear and full expressions might give occasion to.

In the proof of our proposition both experience and reason may distinctly be employed. And to begin with experience,

WHEREAS in the above-recited descent into the Hungarian mines there was observed a notably cold region of a considerable thickness, I have purposely procured accounts from



from divers persons that have here in *England* had occasion, some of them frequently, to descend into deep pits or grooves of differing minerals, without finding by the narratives they made me that they took notice of any notably cold part that they passed thorough, unless I particularly asked a question about such a thing. But for aught I could gather from their spontaneous relations, they felt in summer-time a remission of the heat of the external air as soon as ever they began to descend; which warmth did not so far decrease as to terminate in any notable coldness before they came into a deeper part of the earth, where they are never troubled with that quality: and some of these relations I had from professed miners, and was curious that the relations I procured should be of subterranean parts seated in very differing parts of *England*, as well as of places not all, or most of them having veins of one and the same mineral. And I learned by particular inquiry from a practical mathematician that was often employed about lead mines, that at such depths, as (according to *Morinus*) the second region of the earth reaches to, he himself observed it to be sensibly warm at all seasons of the year (for about that circumstance I was peculiarly solicitous to be satisfied).

Nor is it unconsonant to reason that the middle region of the earth, in the sense meant in the proposition, should not be of the same temperature in all places; not only because of the differences which the climate may produce by reason of its being very much hotter or very much colder in one place than in another; but from the peculiar constitution of the soil, to the consideration whereof I shall here confine myself.

Now this temperament of the soil itself may be diversified not only by its greater or lesser compactness (upon which account some soils are rocky or stony and others light and spongy) but from the nature of the springs or subterraneous liquors that may abound in it or strain through it into the groove or pit we suppose the observer to be in; and that especially by the minerals, particularly salts and marchasites, that grow near the sides of the well, or are brought thither by the waters.

To illustrate this, give me leave to consider that nature does not regulate herself under ground by our imaginary divisions; but, without taking notice of them, produces marchasites, salts, and other minerals, most frequently perhaps in what we call the lower region of the earth; but yet sometimes too in our upper region, and oftentimes in our middlemost region. Let us then suppose, that in some places of this last named region there be a mine of that earth that naturally abounds with embryonated nitre or with some other salt that is apt, especially being dissolved or moistened with water (a thing very familiarly to be met with in mines) to send out a refrigerating effluvium, or by its contact to cool the air. Let us also suppose, that by the sides of another well of the same depth there are store of unripe minerals that are in the process of generation, or rather a great quantity of marchasitical earth, if I may so call it, that is such a substance as I have met with in more than one place copiously impregnated, and, as it were, blended with minerals of a marchasitical nature, and yet of so open and loose a texture as not only water would in a few hours, but air also would not in very many, evidently work upon it. And, since, during the time that marchasites are slowly dissolving, it has been observed, according to what we have elsewhere delivered \*, that many of them will conceive a very considerable degree of heat, will it not be very probable that the temperature of the earth in the place that abounds with these marchasitical minerals will be very warm in comparison of the temperature of the other place, where the soil does plentifully produce nitrous and other refrigerating bodies, though both the places be supposed to be

\* The tract here pointed at is a Discourse of subterranean fires and heats.

at the same distance from the surface of the earth, and consequently in the same subterraneous region.

UPON the like grounds it may also be suspected, that in the same places the temperature may not be always the same, even upon the account of the soil; for I elsewhere shew, that some saline earths, especially nitrous, and some minerals that partake of the nature of marshes, admit a kind of maturation, and perhaps other changes that seem to be spontaneous; and that such changes happen the more notably in those parts of such bodies that are exposed to the air, as those are that chance to be placed at the sides of the deep wells we are talking of; which things being pre-supposed, it will not be absurd to conceive that the mineral, to which either heat or cold is to be referred, may be more copious, ripe, and operative at one time than at another; or, that at length all the earth capable of being, as it were, assimilated by the mineral rudiments harboured in it, may be consumed, or the mineral itself may arrive at a perfection of maturity which will make its texture so close as to be unfit to be penetrated and wrought upon, as before, by the water or other liquor that occasioned its incalcescence.

#### C H A P. VII.

I OMIT to speak of the transient changes that may be occasioned in the temperature of the second region of the earth by several accidents, and especially by the subterranean exhalations that in some places and times copiously ascend out of the lower regions of the earth; nor shall I insinuate upon any of the other causes of a more durable difference of temper in some parts of the second region, such as may be the vicinity of subterranean fires in the third region that heat the incumbent soil; because I would hasten to the third and last part of this discourse, which yet I must not do without premising this advertisement, that I think myself obliged to speak the more hesitantly and diffidently about the temperature of subterranean air, because mineralists have not had the curiosity to examine it by weather-glasses, which would give us much more trusty informations than our sense of feeling powerfully pre-affected by the cold or heat of the external air. I did indeed send fit instruments to some days journey from this place, to examine the air at the bottom of some of our deep mines, but through some unlucky casualties upon the place the attempt miscarried. But when I shall (God assisting) recover an opportunity that I have since wanted, I hope an accurate sealed weather-glass, joined with a portable baroscope, will give me better information than mineralists have yet done: I say a sealed weather-glass, because though common thermoscopes had been employed by miners, I durst not rely upon them; being persuaded by trials purposely made, as well as by the reason of the thing, of the fallaciousness of such thermoscopes; for in them the included air is liable to be wrought upon, not only by the heat and coldness, but by the weight or pressure of the external air: so that if a thermoscope be let down from a very considerable height, at the top of which the station of the pendulous liquor be well marked, that liquor will be found to have risen when the instrument rests at the bottom, as if the included air were manifestly refrigerated, though the temper of the external air may be in both places alike, the cause of the pendulous liquor's rising being indeed, that the aerial pillar incumbent on the stagnant liquor is higher and heavier at the bottom where the instrument rests than that which leaned upon it, at its first or upper station nearer the top of the atmosphere. From whence it will be easy to conclude, that at the bottom of a deep groove where the atmospherical pillar that presses the stagnant water will be much longer



longer and heavier than at the top, the air may appear by the instrument to be colder in places where it is really much hotter, the increased weight of the incumbent air being more forcible to impel up the pendulous liquor, than the endeavour of expansion procured in the included air by the warmth of the place is to depress it.

## C H A P. VIII.

THAT which challenges the third and last part of my discourse is the lowermost region of the earth, about whose temperature I shall comprize what I have to say in the following proposition :

## P R O P O S I T I O N IV.

“ THE third region of the earth has been observed to be constantly and sensibly warm, but not uniformly so, being in some places considerably hot.”

I MENTION, that the recited temperature has been observed in the lower region, because I would intimate that I would have the proposition understood with this limitation as far as has been yet (that I know of) observed : for almost all the deep grooves that mineralists have given us accounts of, and wherein men have wrought long enough to take sufficient notice of the temperature of the air, have been made in soils furnished with metalline ores or other minerals, without which men would not be invited to be at so great a charge as that of sinking so very deep pits and maintaining workmen in them ; so that experience has yet but slenderly, or at least not sufficiently informed us of the temperature of those parts of the third region of the earth that are not furnished with ponderous minerals, and consequently has not informed us of the temperature of the lowermost region in general, as will better appear by what I shall ere long represent.

HAVING premised this advertisement about our proposition, we may proceed to the distinct proof of the two parts or members it consists of.

AND to begin with the first, whatever the peripateticks teach of the innate coldness of the earth, especially where it is remotest from the mixture of the other elements, yet having purposely enquired of several persons that visited and also frequented the third region in differing countries, soils, and at differing depths under ground, and seasons of the year, I did not perceive that any of them had ever found it sensibly and troublefomely cold in the third region of the earth ; and on this occasion I remember I had some light suspicion, that at least in some cases the narrowness of the cavities wherein the diggers were in divers places reduced to work, might make the warmth they felt proceed in great part from the steams of their own bodies, and perhaps of the minerals, and from the difficulty of cooling or ventilating the blood in an air clogged with steams : and I was the rather induced to think this possible, because I had (even in metalline mines that were but shallow and very freely accessible to the air) observed a strong smell of the metal abounding there.

I HAVE likewise found by several trials, that the exhalations that proceed from the bodies of animals do so vitiate the air they abound in, as to make it much less fit for their respiration, and to be apt to make them sick and faint ; wherefore I thought it not altogether unfit to inquire whether the heat of the subterranean air in such places as have been newly mentioned might not be referred to these causes, but I was answered in the negative ;

tive ; especially by an inquisitive person that had been in the deepest and hottest mines that have been visited by any acquaintances of mine.

THIS way of accounting for the subterranean warmth being laid aside, it seemed, I confess, somewhat difficult to conceive how it should be produced ; yet two principal causes there are, to which I think we may probably refer the temperature of those places where the air is but moderately warm, to which a third is to be added, when we come to give an account why some places are troublefomely hot.

AND first, why the coldness of winter should not be felt in the lowermost region of the earth may be, that the air there is too remote from the superterrestrial air to be much affected with those adventitious causes of cold that make that quality intense in the air above ground ; but because this reason shews rather why it should not be in the earth's lower region much colder in winter than in summer, but not why it should be in all seasons warm there, I shall add as a conjecture, that the positive cause of the actual warmth may proceed from those deeper parts of the subterranean region which lie beneath those places which men have yet had occasion and ability to dig. For it seems probable to me, that in these yet unpenetrated bowels of the earth there are great store-houses of either actual fires or places considerably hot, or (in some regions) of both, from which reconditories (if I may so call them) or magazines of hypogeal heat, that quality is communicated, especially by subterranean channels, clefts, fibres, or other conveyances, to the less deep parts of the earth, either by a propagation of heat through the substance of the interposed part of the soil (as when the upper part of an oven is remissly heated by the same agents that produce an intense heat in the cavity) or by a more easy diffusion of the fire or heat through the above-mentioned conveyances (as may be exemplified by the pipes that convey heat in some chemical structures) or else (which is perhaps the most usual way) by sending upwards hot exhalations and mineral steams, which by reason of the comparatively heavy minerals they consist of, and by reason of their being less dispersed nearer the places whence they proceed, are usually more plentiful in the deeper parts of the earth, and somewhat affect them with the quality that they brought from the work-houses where they were formed, and that they retain for some time after.

#### C H A P. IX.

THAT manifest steams oftentimes are found in grooves, especially in deep ones, is evident by the damps that infest most of them, and that in distant regions, as in several provinces of *Germany*, *Bohemia*, *Hungary*, &c. as also in several parts of *England*, in grooves, some of which I have received relations of from the mine-men themselves ; by which it appears that several of these exhalations ascending from the entrails of the earth are sulphureous and bituminous in smell, and in some grooves (one whereof I elsewhere mention myself to have visited) these steams are apt actually to take fire.

THE warmth of many subterranean exhalations, I think, may be made further probable by some other observations. For though these newly mentioned are not to be rejected, and may be employed for want of better, yet I have several times questioned whether I ought to acquiesce in them alone ; for I do not think the easy inflammableness of bodies to be always a sure proof of the actual sensible warmth of the minute parts it consists of, or may be reduced into. For though salt-petre be very inflammable, yet being by a solution in fair water reduced to invisible corpuscles, it highly refrigerates that liquor : nor have I observed its fumes (when far from the fire) to have any heat sensible to our touch.

And



And the like may be said of the exhalations of highly rectified spirit of wine; which yet we know is itself totally inflammable; nay I know not, whether (for a reason elsewhere declared) copious exhalations may not ascend from the lower parts of the earth, and yet be rather cold than hot; for, in another paper, I mention a way by which I made a mixture that plentifully enough emitted steams, of whose being rather of a cold than a hot nature there was this probability, that the mixture whence they ascended, even whilst its component ingredients were briskly acting upon one another, was not only sensibly but considerably cold.

ONE main thing, therefore, that induces me to assent to the opinion whereto the former instances do but incline me, is, that having purposely inquired of an observing man that frequented deep mines (wherein he had a considerable share) he answered me, that he plainly observed the fumes that came out of the mouths of the deep pits to be actually and sensibly warm, and that in a warm season of the year. And *Morinus* (above cited) speaking of the deep Hungarian mines, makes it the first epithet of the copious exhalation that ascended from the bottom, that it was hot; and a few pages after he says, that at the mouth of the well the ascending fumes were sensibly hot in summer itself: and the same arguments that I have elsewhere given to shew that there are very hot places, and, as it were, æstuary in the bowels of the earth, may serve to make it probable that the steams ascending thence may be actually warm.

THAT also in many places of the earth where no grooves are dug and no visible exhalations are taken notice of, they may yet pervade the soil, and exercise some operations of warmth, may be probable by this, that the experienced *Agricola* himself reckons it among the signs of a latent mineral vein, that the hoar-frost does not lie upon that tract of the surface of the earth under which a vein (though perhaps very deep) runs. The like directions I have known given by the skilful in *England* for the discovery of places that contain coal-mines. And I remember a near relation of mine shewed me a great scope of land of his, which (though in an outward appearance likely to be as cold as any place thereabouts) he affirmed would not suffer snow to lie upon it above a day or two in the midst of winter.

THE probability of which relation was confirmed to me by the answer I received from a very ingenious gentleman who lives among mines, and is not a little concerned in some of them. For having inquired of him what he had observed about the lying or not lying of the snow on the mineral soils near the place of his residence, he replied, that in some of them he did not take notice of any peculiar indisposition to let the ice and snow continue on them, which I conceive may proceed either from the want of such minerals in the subjacent parts as were then in the state of incalcescence; or else from this, that (according to what we have elsewhere observed about the snow on *Etna*) the direct ascension of the hot steams was hindered by some layers of rocks or stone through which the steams could not penetrate, or could do it but so slowly as to lose their actual warmth by the way. But this gentleman added, that in other places, near that of his abode, and such as he knew to have mineral veins beneath them, he observed that the snow (nor the ice) would scarce continue at all upon the surface of the ground, even in an extraordinary cold winter.

It will be a considerable instance to our purpose, if it be indeed true which some learned men have written, that near the gold mines in *Hungary* the leaves of the trees (especially those that respect the ground) are oftentimes found ennobled with a golden colour from the metalline exhalations of the gold mines; which, one would think, must by reason of their ponderousness need a considerable heat to elevate them, especially into the open air, but though doubting of this relation, as not made by mineralists or accurate observers, I

inquired about it of a person whose curiosity carried him purposely to visit those mines, I was answered that he could not be a witness to the truth of the observation; yet he told me an observation (which I elsewhere mention) that doth not discountenance that tradition.

If it be objected that what has hitherto been said about latent fires and heats in the bowels of the earth will give an account of the warmth only of those places that are within reach of the action of such magazines of heat, which probably may be wanting in many places of the earth; I shall readily confess, that, as I first made this objection to myself, so I do not yet discern it to be unreasonable; and that, for aught I know, if men had occasion to dig as deep, and be as far conversant in many other low places of the earth where there are no signs of minerals, as they have done where the hopes of actual discovery of veins of metals and other minerals worth working have invited them, divers places in the third region of the earth would be met with that would be destitute of the warmth that has hitherto been generally found in places of the same region that either abound with minerals themselves, or are near some of the deep and latent æstuaries above mentioned.

AND as for those parts of the third region of the earth, which men feel not only warm but troublesomely hot, that incommodious degree of heat seems not (at least in some places) to be derivable from the two above-mentioned causes, which must (to produce so considerable an effect) be assisted by a third cause more potent than themselves; which seems to be the incalcescence there is produced in many mines and other places by the mutual action of the component parts promoted by water of immature and more loosely contexted minerals, especially such as are of a marchasitical nature: that such an incalcescence may by such a way be produced in the bowels of the earth, I have elsewhere shewn (in my discourse of subterranean fires and heats) by the examples of such incalcescences producible in mineral bodies here above ground: that marchasites, which for the most part abound in vitriol, are bodies very fit to procure this subterranean heat, may be confirmed not only by the sulphureous and saline parts they abound with, and by this, that many of them may be wrought on, as we have tried, both by simple water and even by moist air, which argues the resolubleness of their constitution; but also by this, that having purposely inquired of a gentleman that went out of curiosity to visit one of the deeper Hungarian mines, he confirmed to me what I had otherwise been informed of, by answering me, that in the lower parts of the mine he had gathered vitriol that appeared above ground to be of a golden nature; and that in a cave that is on one side of the groove in the deep gold mine near *Cremnitzo*, the corrosive smell is so strong and noxious that men have not dared to dig out the native gold it richly abounds with, being deterred by the ill fate of divers that ventured to work in it. Adding, that though he passed by it in great haste, yet he could not avoid the being offended by the noisome exhalations. And on this occasion, it will not be, I presume, disliked, if I illustrate what I was saying of immature minerals, by subjoining, that having asked this chemist, whether the vitriol he found very deep under ground were all solid, or some of it soft? he affirmed, that as he gathered it he found some of it soft; and to satisfy my curiosity to know whether it continued that yielding consistence; he farther told me that it was soft in the deeper part of the mine, but when he had brought it into the superterrestrial air, it hardened there, and appeared to have divers golden streaks in it.



C H A P. X.

ONE thing there is, which must not be here omitted, though it will probably be great news to those that philosophize only in their studies, and have not received information from any that visited the deeper parts of the earth. The phenomenon is this, that the diggers in mines, having found by unwelcome experience, that in deep grooves, the air (unless ventilated and renewed) does in a short time become unfit for respiration, have been put upon this expedient, to sink, at some convenient distance from the groove where the miners work, another pit, by some called a vent pint, that usually tends directly downwards (though sometimes it make angles) to which our English mine-men in do several parts of this kingdom give differing names, whereof the most significant seems to be that given it in the lead mines of *Derbyshire*, where they call it an air-shaft, and are wont to make it 40, 50, and sometimes 80 or 100 paces off; and, as one of the chief and skillful miners there informed me, as deep as the groove or well; (though I find that the best German and some English miners think a less depth will often suffice) from this air-shaft to the groove the men work in there passes a channel, or, if I may so call it, ventiduct, to convey the air from the former to the latter; which is that, that *Agricola* sometimes (for he employs not the term always in the same sense) denotes by his coniculus; and which, though differing named by our miners in several parts of *England*, is & VI. de Lib. V. in the above mentioned lead mines called a drift, because the air does usually in the form re metall. of wind drive through it, and thereby enables the workmen to breathe freely and conveniently enough at the very bottom of the well. On this occasion I remember that a very observing man, who much frequented these mines, told me, that at the depth of no less than about 200 yards, he found, that by the help of the air-shaft, the air was not only very commodious for respiration, but temperate as to heat and cold. And when I further asked what time of the year it then was? he told me it was about the latter end of August, and the beginning of September.

Now that which seems to me to deserve a farther and accurate observation about the motion and temperature of the air in these artificial under-ground cavities, is a relation of *Agricola's* which (though he be the most classick author we have about mines) has not, that I know of, been taken notice of, in him. For this experienced writer, though in his treatise \* *de ortu & causis subterraneorum*, he only says, indefinitely, that by means of the coniculus or drift, which connects the air-shaft and the well, that air which comes in at one of those two, passes out at the other; yet in his fifth book, *de re metallicâ*, he gives a more particular and odd account of the course of the air in these not over-clear terms, *aer autem exterior se sua sponte fundit in cava terræ, atque cum per ea penetrare potest, rursus evolat foras. Sed diversâ ratione hoc fieri solet; etenim vernis & æstivis diebus in altiore puteum insluit, & per cuniculum vel fossam latentem permeat, ac ex humiliore effluit; similiter iisdem diebus in altiore cuniculum infunditur, & interjecto puteo defluit in humiliorem cuniculum, atque ex eo emanat. Autumnali verò & hyberno tempore contra in cuniculum vel puteum humiliorem intrat, & ex altiori exit: verum ea fluxionum aeris mutatio in temperatis regionibus fit in initio veris, & in fine autumnus: in frigidis*

\* Idcirco serobes, putei cuniculi effossi complentur exteriore aere. Atque ipsum in eos influere imprimis hyemali tempore evidens est in duobus puteis, ad quorum utrumque ex modico intervallo cuniculus aliquis pertinet. Nam aer in unum continuo laet fit, rellaque per cuniculum permeat & transit ad alterum; atque ex eo rursus evolat foras.

*autem, in fine veris & in initio autumnii.* To which he adds, † that which is more remarkable, that the air in both the mentioned times, before its wonted course come to be durably settled, uses to be for the space of a fortnight liable to frequent changes, sometimes flowing into the upper or higher groove or drift, and sometimes into the lower, and passing out at the other. If this observation constantly hold, though but in some deep mines, it may hint some odd inquiries about considerable and periodical changes in the subterranean parts of the earth, or in the air, or in both; which, though they have not yet been considered, deserve to be so. I have endeavoured to learn whether any such thing has been observed in some deep lead mines, whence I have procured divers informations about other particulars. But a very observing person, that had the chief hand in contriving the subterranean structures there, assured me that both winter and summer, the current air went constantly the same way; the air entering in at the mouth of the air-shaft, and coming out at the perpendicular groove, which takes its denomination from a cave (or *cava putealis*) usually built over the orifice of it, to shelter the workmen from rain, and other inconveniencies.

AND since the writing of this, I found in *Morinus* (his relation already mentioned) a passage that may somewhat illustrate the darkly expressed observation of *Agricola*; for the lately mentioned author writes, that in the deep Hungarian mines he visited, the outward air passed, first, through the boroughs, and so through by-ways, if I may so call them, that tended not directly downwards, reached at length to the bottom of the well, or perpendicular groove, whence, together with the steams proceeding from the mine, it ascended straight upwards. But *Morinus* taking no notice at all of *Agricola's* observation about the differing course of the subterranean air at differing seasons of the year, though, as I find by what he writes elsewhere, it was summer when he visited the mines, and so what he reports, agrees well with one part of what *Agricola* seems to say; yet, as to the other and principal part of his observation, he says not any thing. And the sensible heat he ascribes to the steams ascending out of the perpendicular well, leaves it somewhat dubious, what interest the rarefaction of the air by the subterraneous heat may have in the phenomena we have been discoursing of.

BUT to return to what I was saying before I had occasion to mention *Morinus*; which perhaps it will not be impertinent to add, that I learned by inquiry, that the air-shafts and the wells were in these mines much of a depth; but I hope before long to have accounts of what happens in other mines, in other parts of *England*, as to the course of the subterranean air, especially when its issuing out of the well or the air-shaft depend not on the changes of the winds that blow above ground; and I wish the curious would employ the like endeavours in other countries.

FOR indeed, what I have hitherto discoursed in this treatise, is accommodated but to the scant information I have hitherto received; and therefore ought to be rectified, or confirmed, by farther informations, if they can be procured.

In the mean time, I think I may probably enough gather from the passed discourse, that though in some mines, three subterranean regions, and their distinguishing attributes, may be not inconveniently assigned; yet generally speaking of the whole body of the terrestrial globe, as far as we know it, both the bounds and the temperature of the regions of the earth, as well as those of the air, are various and uncertain enough.

† Sed aer utroque tempore, antequam cursum suum illum consuetum constanter teneat plerumque quatuordecim dierum spatio crebras habet mutationes, modo in altiores puteum vel coniculum influens, modo in humiliorem.



And much less have we any certain knowledge of the temperature of the more inward, and, if I may so speak, the more central parts of the earth; in which, whether there be not a continued solidity, or great tracts of fluid matter, and whether or no differing regions are to be distinguished, and what their number, order, thickness, and qualifications may be, we are as yet ignorant, and shall, I fear, long continue so; for it is to be noted (with which observation I shall conclude) that what has been hitherto discoursed belongs only to the temper of those subterranean parts, to which men have been enabled to reach by digging. It is true, indeed, that some mines, especially in Germany and Hungary, are of a stupendous depth, in comparison of the generality of ours, and of the more obvious cavities of the earth; yet I find it boasted in a discourse, written purposely of the various mines in the world, that the rich mine at *Sucberg* is 400 yards deep: and they are scarce believed, that relate one Hungarian mine, which they visited to be 400 fathom; which, though double the depth of the former, reaches not to half a mile. But the deepest of all the mines that I have as yet read or heard of from any credible relator, is that which the experienced *Agricola*, in the tract he calls *Bermannus*, cap. 12. mentions to be at *Cotteberg*. But this itself, though it reach to above 500 fathom, that is, 3000 feet, yet this prodigious depth does not much exceed half a mile, and falls short of three quarters\*, and how small a part is that of the whole depth of the terrestrial globe? whose semi diameter, if we admit the recent account of the learned *Gassendus*, is reckoned at 4177 Italian miles; in comparison of which, as I was saying, how small a thing is a depth, that falls very short of a single mile?

\* Licet variae de ambitu terrae opiniones sint, nobis tamen propemodum constat, esse ipsam miliarium Italicorum 26255, quod in maximo ad terrae superficiem circulo respondeant unigradui miliaria, proximè 73: &c. Gassend. Instit. Astronom. lib. 2. cap. 13.



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 T E M P E R A T U R E  
 OF THE  
 S U B M A R I N E R E G I O N S,  
 As to HEAT and COLD.

C H A P. I.

**T**HOUGH the Aristotelians, who believe water and air to be reciprocally transmutable, do thereby fancy an affinity between them, that I am not yet convinced of; yet I readily allow of so much affinity betwixt those two fluid bodies, as invites me (after having treated of the temperature of the aerial regions) to say something of that of the submarine regions; which name of submarine, though I know it may seem improper, I therefore scruple not to make use of, because even among the generality of learned men, use has authorized the name of subterraneous places; for as these are not by this name, and indeed cannot in reason be supposed to be beneath the whole body of the earth, but only the superficial parts of it; so by the appellation of submarine regions it is not to be supposed that the places so called are below the bottom of the sea, but only below the surface of it.

BUT to come from words to things, I presume it will not be expected, that I, that never pretended to be a diver, should give of the regions I am to treat of, an account built on my own observations; and I hope it may gratify a reasonable curiosity about a subject, of which classick authors are so very silent, and about which philosophers seem not so much as to have attempted any experiments (for want of opportunities and means to make them). I offer the best information I could supply myself with, by purposely conversing with persons that have dived, some without, and some by the help of engines. To which I have added some reports that I judge fit to be allowed, made me by persons that had conversed with the divers upon those African and Indian coasts, where the most famous and expert are thought to be found.

AND I the rather report the answers and relations my enquiries procured, because the informations they give us concern a subject considerable as well as vast, about which nevertheless I among many others am not in a condition to satisfy at all my curiosity by trials of my own making; and because also, what I shall say will probably spoil the credit of the vulgar error, that in all deep water, of which the sea is the chiefest, the



lowermost are still the warmest parts, unless in case that in some very hot climates, or seasons, the superficial ones happen to be a little warmed by the extraordinary or violent heat of the sun.

C H A P. II.

THOUGH the air and the earth have been discriminated as to temperature, into three regions; yet the informations I have hitherto met with, invite me to assign to the sea no more than two. The former of which may be supposed to reach from the superficies of it, as far downwards, as the manifest operation of the variously reflected and refracted beams of the sun, or other causes of warmth penetrate; from which to the bottom of the sea, the other region may be supposed to extend.

ACCORDING to this division, the limits of this upper region will not be always constant; for in the torrid zone, and other hotter climates, it will, *ceteris paribus*, be greater than in the frigid zone or in the temperate zones; and so it will be in summer than in winter; and in hot weather than in cold; supposing in these cases the heat to come from the sun and air, and not, as sometimes it may do, from the subterranean exhalations.

THE same causes are likewise proper, as it is manifest, to alter the temperature, as well as the bounds of this region; but this temperature may also be changed, in some few places, by at least two other causes; the one is the differing constitution of the soil that composes the shore, which may affect the neighbouring water, if it do extraordinarily abound with nitre, loosely textured marshaltes, or other substances capable considerably to encrease or lessen the coldness of the water. Another, though unfrequent cause, may be the figure and situation of the less deep parts of the shore, which may in some sort reverberate the heat that proceeds from the sun; and upon such an account may either add to the warmth, or allay the coldness, that would else be found in the neighbouring water. For whatever the schools are wont to teach about the interest of the attrition of air in the heat produced by the sun beams, I have elsewhere shewn by experiments, that those beams may considerably operate upon bodies placed quite under water.

BESIDES these two cases, that may occasion exceptions to the general observation; I intimated by the words, *at least*, that there might be others; because to mention now but one example, though it seem probable from what I have elsewhere delivered concerning the subterranean fires and heats, that may in some places be met with, even beneath the bottom of the sea, that the phenomenon I am going to recite may be reduced to the causes newly intimated; yet I am not absolutely certain, but that in this case, whereto some others may perhaps be found resembling, some other cause than those hitherto mentioned may produce or concur to the effect. The relation here meant is afforded us by the following passage, taken out of the voyage of Monsieur *de Monts*, into *New France* (whereof he went to be governor) where the relator thus recites his observation: about the eighteenth day of *June* we found the sea-water during three days space very warm, and by the same warmth our wine also was warm in the bottom of our ship; yet the air was no hotter than before. And the 21<sup>st</sup> of the said month, quite contrary, we were two or three days so much compassed with mists and cold, that we thought ourselves to be in the month of *January*, and the water of the sea was extreme cold; which continued with us, until we came upon the bank, by reason of the said mists, which outwardly did procure this cold unto us. This effect he attributes to  
a kind

a kind of antiperistasis in the following part of his narrative; which I shall not now either transcribe or examine.

## C H A P. III.

AND thus much being briefly noted touching the upper region of the sea, and the requisite cautions (that may perhaps extend further than it) being premised, it remains, that I take notice of the temperature of the lower region, which, in one word, is cold; unless in some few places to be presently mentioned. For water being in its natural or most ordinary state a liquor whose parts are more slowly agitated than those of men's organs of feeling, must be upon that account cold as to sense; and consequently it need not be strange, that those parts of the sea which are too remote to be sensibly agitated by the sun-beams, or wrought upon by the warmth which the air and upper parts of the earth may from other causes receive, should be felt cold by those that descend into it; unless in those few places where the coldness may be either expelled or allayed by hot springs, or subterrrestrial exhalations, flowing or ascending from the subjacent earth, or the lower parts of the shore, into the incumbent or adjacent parts of the water.

To justify my ascribing of this coldness to the second, or lower region of the sea, I shall now subjoin some relations I procured from persons that had occasion to go down into it, or otherwise take notice of its temperature in very differing regions of the world, and at very unequal depths.

AND first as to the temperature of the lower region in the northern sea, I had the opportunity to converse often, and sometimes to oblige a man bold and curious enough, who for some years got the best part of his subsistence by descending to the bottom of the sea in an engine (whose structure I elsewhere describe) to seek for and recover goods lost in ship-wrecked vessels. This person I diligently examined about divers submarine phenomena, about which his answers may be elsewhere met with; and as to the temperature of the lower parts of the sea (the knowledge of which is that alone that concerns us in this place) he several times complained to me of the coldness of the deep water, which kept him from being able to stay in it so long as he might have been put into a condition of doing by the goodness of his engine; for I remember that he related to me, that he staid once betwixt an hour or two, at a depth that was no greater than 14 feet and a half upon the coast of *Sweden*, in a place that was near the shore; and I afterwards learned that he staid much longer in a deeper place (use having probably made the cold more supportable to him). He told me then, that about two years before, he was engaged by a good reward to go down with his engine to the bottom of the sea to fetch up some goods of value out of a ship that had been cast away there within about a mile's distance from a very little island, and, if I mistake not, about six miles from the shore. He further answered me, that though he felt it not at all cold on the surface of the water (his attempt being made in *June*) yet about the depth of the ship, it was so very cold, that he felt it not so cold in *England's* winter and frosty weather. And he told me, that an excessive cold was there felt, not only by him, but by very sturdy men, who, invited by his example, would needs also go down themselves to participate and promote the hoped-for discovery. He told me also, that the upper water did but cool and refresh him; but the deeper he went, the colder he felt it, which is the more considerable, because he had sometimes occasion to stay at 10 fathoms or even 80 feet under water. And I since found, that he informed divers virtuosi that purposely consulted him, that he found the coldness of the water encrease with its depth; and gave that for



the reason why he could not stay so many hours as otherwise he might, at the bottom of the sea; adding, that before his engine was well fitted, he was once so covered over with it, that he was forced to touch the ground with his hands and feet, and the neighbouring parts, to which he found a coldness communicated by the fundus he leaned upon; though the closeness of his disordered engine made the other, and (whilst he was in that posture) upper parts of his body, of a very differing temper.

An inquisitive person of my acquaintance, that made a long stay in the *Northern America* (at about two or three and forty degrees of latitude) and diverted himself often with swimming under water, answered me, that though he scarce remembered himself to have dived above two fathoms beneath the surface of the sea, yet even at that small depth, he observed the water to encrease in coldness, the lower he descended into it; which argues, that though the sun-beams do often penetrate plentifully enough to carry light to a great depth under water, yet they do not always carry with them a sensible heat; and that, at least, in some places, the upper region of the sea reaches but a little way.

THE coldness of the climate in these western parts of *Europe*, and the want of considerable inducements to invite men to dive often to any great depth into our seas, has kept me from being able to procure many observations about the temperature of their lower region; but upon the hotter coasts of *Africa* and the *East Indies*, the frequent invitations men have to dive for coral, pearls, and other submarine productions, have made it possible for me to get more numerous observations; some of which I shall now annex.

#### C H A P. IV.

MEETING with a person of quality who had been present at the fishing of coral upon the shore of *Africa*, and who was himself practised in diving, I inquired of him, whether he found the sea upon the African coast to be much colder at a good depth, than nearer the surface; whereto he answered me, that though he had seldom dived above three or four fathoms deep, yet, at that depth, he found it so much colder than nearer the top of the water, that he could not well endure the coldness of it.

AND when I farther asked him, whether, when he was let down to the bottom of the sea, in a great diving bell (as he told me he had been) he felt it very cold, though the water could not come immediately to touch him; he replied, that when the bell came first to the ground, he found the air in it very cold, though after he had staid a-while there, his breath and the steams of his body made him very hot.

THAT also at a greater depth in those hotter climates, the sea-water is sensibly cold, may be thus made probable: inquiring of a famous sea-commander, who had been upon the African coast, to what depth he was wont to sink his bottles to preserve his wine any thing cool in that excessive hot climate, he answered me, that in the day time he kept it in a tolerable temper so as to be drinkable, by keeping it in the bottom of the ship, and in sand; but in the morning he had it cool enough by sinking his bottles over night into the sea, and letting them hang all night at 20 or 30 fathom deep under water.

INQUIRING also of an intelligent gentleman that was employed to the river of *Gambra*, and sailed up 700 miles in it, in a small frigate, whether he had observed that in the sea, even of those hot climates, wine may be preserved cool; he told me, that it might, and, that by the means I hinted to him, which was to let down, when the ship came to an anchor in the evening, several bottles full of wine (they used that of *Madeira*) exactly

stopped to ten, twelve, or fourteen fathoms deep; whence being the next morning drawn up, they found the wine cool and fresh (as if the vessels had been in these parts drawn up out of a well) provided it were presently drank, for if that circumstance were omitted, the heat of the air on the upper part of the water would quickly warm the liquor.

I REMEMBER too, that having met with a man of letters, that sailed to the *East-Indies* in a Portugal-caract, I learned by inquiry of him, that it was the practice in that great vessel for the captain and other persons of note, whilst they pass through the torrid zone, to keep their drink, whether wine or water, cool, by letting it down in bottles to the depth of 80, 90, and sometimes 100 fathom or better, and letting it stay there a competent time; after which, he told me, he found it to be exceeding cool and refreshing.

LASTLY, to satisfy myself as far as I could, to how great a depth the coldness of the sea reached; meeting an observing traveller, whose affairs or curiosity had carried him to divers parts, both *East* and *West Indies*, I inquired of him, whether he had taken notice of any extraordinary deep soundings in the vaster seas, to which being answered, that some years ago, sailing to the *East Indies*, in a very great ship, over a place on the other side the line, that was suspected to be very deep, they had the curiosity to let down 400 fathom of line, and found they needed no less. Whereupon I inquired of him, whether he had taken notice of the temperature of the sounding lead as soon as it was drawn up; to which he told me, that he and some others did; and that the lead, which was of the weight of about 30, or 35 lb. had received so intense a degree of coldness, as was very remarkable; inasmuch, that he thought that if it had been a mass of ice, it could not have more vehemently refrigerated his hands; and when I asked in what climate this observation was made, he told me it was in the antarctick hemisphere, but at a great distance from the line. As indeed I concluded by some circumstances he mentioned to me, that it was about the 35th degree of southern latitude.

#### C H A P. V.

THESE are the chief relations I have hitherto been able to procure about the temperature of the sea; which, if they be so confirmed by others, as that we may conclude they will generally hold, it will not be irrational to conceive, that in reference to temperature, those two fluids, air and water, may have this in common, that where their surfaces are contiguous, and in the neighbouring parts, they happen to be sometimes cold, sometimes hot, as the particles they consist of chance to be more or less agitated by the variously reflected sun-beams, or more or less affected by other causes of heat. But that part of the air which they call the second, and is superior to the first, as also the lower region of the sea, being more remote from the operation of those causes, do retain their natural, or more undisturbed temperature, which, as to us men, is a considerable degree of coldness, the agitation of their small parts being usually in those regions much inferior to that of the spirits, blood, and other parts of our organs of feeling; so that the regions of the water and air seem to answer one another, but in an inverted order of situation; and the analogy might perhaps be carried further, if I had time and opportunity to do it in this place. And here I shall not dissemble, that I was somewhat perplexed by meeting with a traveller that had visited the East-Indian coast, near the famous Cape of *Comory*; for asking him some questions touching the neighbouring sea, I gathered from his discourse, that he concluded from that of some divers, that the sea

near



near *Ceylon* was warmer at the bottom than at the top; and when I thereupon asked him whether this happened not in their winter, he replied, that it was indeed winter, though not with us, yet with them. It occurred indeed to my thoughts on this occasion, that perhaps in a part of the torrid zone so near the line as about 8 degrees, if the sea were not of a considerable depth, the heat of the two not far distant shores of *Coromandel* and *Ceylon* might have no small influence upon the temperature of the water. I considered also, which did not a little weigh with me, that in divers parts of the *East-Indies*, and even in a region bordering upon *Coromandel*, where an ingenious acquaintance of mine lived some years, it has been observed, that winter and summer are not so much discriminated by cold weather and hot, as by very rainy weather and very dry: nay, in some places the sultry heat of the climate is more complained of in what they call their winter than their summer; so that there will be no necessity to recur to an antiprismalis occasioned by the coldness of the winter. I thought too, that it may perhaps be without absurdity suspected, that as the bottom of the sea in this place had a peculiar constitution, that fitted it more than others for the copious production of pearls; so there might be some peculiarity in the nature of the subjacent soil, or there may be some subterranean fire or heat beneath it, which may occasion an unusual warmth in that part of the sea, by which cherishing warmth, perhaps, such abundance of shell-fishes teeming with pearls may be invited to settle there, rather than in any of the neighbouring places. But with all these conjectures I should not have been so well satisfied, as with the answer I afterwards obtained by a gentleman, whose curiosity had carried him to be an assiduous spectator of the famous pearl-fishing, near the island of *Manar*, between that and the coast of *Coromandel*, which reaches near, if not fully to the Cape of *Comory*. For this person having had much conversation with the divers for pearls, not only learned from them, that they found the water very sensibly cold at the bottom, which in some places he estimated to be 80 or 100 fathom deep; but observed divers of them at their return to the boats, to be ready to shake with cold, and hasten to the fires that were kept ready for them in little cabbins upon the shore; which relation being accompanied with divers circumstances of credibility, and arguing, the person that made it to have been acquainted with the report above-mentioned, and had met with some that had dived in the place whereto it had relation, made me conclude, that as to that report, something extraordinary had happened in that place; or that there was some mistake of him to whom it was made; or that divers did not descend to a sufficiently considerable depth.

If I had been furnished with opportunity, I would have engaged some ingenious navigators to examine the temperature of the submarine regions, both of differing seasons of the year, especially the hottest part of summer, and coldness of winter, and with hermetically sealed weather-glasses, in order to the discovery of such particulars as these, whether there be in some seas any such varying differences of temperature, as may invite us at least in some places, to make more than two submarine regions: whether the submarine coldness do at the bottom of the sea, or elsewhere, either equal or surpass that degree, which we here find sufficient to freeze common water: whether the parts of the sea-water are still the colder, as they are the deeper; and whether or no this increase of coldness be regular enough to be reducible to any settled proportion. But for the resolving of these and the like questions, I did not carelessly intimate, that a sealed weather-glass was to be employed; for I take a common one to be altogether unfit for such purposes, not only because the sea-water would mingle with such liquors as are wont to be employed in it, for that inconvenience I could easily remedy, by substituting,

as I have several times done in other cases, mercury instead of ordinary liquors; but chiefly because the incumbent sea-water would gravitate upon the stagnant liquor of the weather-glass, and thereby render its informations false or uncertain; according to what I have had occasion to observe in another tract.

WHEREFORE, that there may not in this place be any need to recur, I shall add a slight experiment that I made for the satisfaction of some ingenious men not well acquainted with hydrostaticks, or not rightly principled in them: and this trial I shall the rather mention, because many will not allow water to press upon mercury immerged therein, this being a far more ponderous liquor than that; and others will expect that the included air, having no place to escape out at, should resist the ascension of the subjacent mercury, more than indeed it will. We made then a small weather-glass differing from common ones, besides the bigness, in that it was furnished with mercury instead of water; and in that we employed to contain the stagnant mercury a glass phial with a narrow neck, wherein, by a piece of cork or two, the stem of the glass ball was well fastened, that this globular part of the instrument might not be lifted up when it was under water. Then having by applying cold water to the outside of the ball endeavoured to reduce the air to the same temper with the water, or at least to an approaching degree of coldness; and having taken notice of the station of the mercury in the shank or stem above-mentioned, we did, by strings tied about the neck of the small phial, let the instrument gently down into a large tall glass body, filled with fair water, that the liquor and vessel being both transparent, we might easily perceive the motions of the mercury in the slender pipe; by which means it appeared, that as the thermometer descended deeper and deeper into the water, the mercury was pressed up higher and higher in the stem; and that it may not be suspected that this ascension proceeded only or chiefly from the refrigeration of the air by the water, I shall add to what I have just now noted, that though the coldness of the water may well be supposed uniform, as at least to sense, yet the whole instrument being leisurely removed sometimes to the upper surface of the water, sometimes to the lower, the rising and falling of the quicksilver in the slender pipe was suivable to the depth of its surface, or its distance beneath that of the water. (The like experiment we might have tried with a thermometer furnished with water, and let into oil, or with deliquated salt of tartar and pure spirit of wine, instead of mercury and water; if we had been furnished with sufficient quantities of those liquors, and had judged it to be requisite.) But this circumstance I thought fit to admonish the spectators of, that it is not to be expected that the mercury should rise as much in proportion when it is (for example) a foot under water, as when it is but two or three inches; because, according as the instrument is let down deeper, and the air crowded into a less room, the spring of that compressed air becomes the stronger, and makes the more resistance; which advertisement agreed well with the experiment, whose other phenomena I pass over as not pertinent to this place, where I would only justify what I said of the unsuitableness of weather-glasses made (though with other liquors) after the common ways for making the submarine trials I proposed.

BUT till such artificial observations can be obtained, we may from what has been above delivered probably gather, that though the lowermost of the submarine regions be very sensibly cold, yet water, at least that of the sea, does not by these phenomena appear to be the *summum frigidum*. Though I have been several times able to produce ice in salt-water, yet I find not by any observation, that there has been ice met with, and generated at the bottom of the sea, under which the earth has been found unfrozen by our divers; and appears to be soft at depths exceedingly surpassing the greatest they have reached;



as is evident by the mud, gravel, &c. fetched from the bottom of the sea by sounding plummet, let down to 80 or 100 fathom, or even a greater depth, whereof examples may be met with in the journals of navigators: nay, my curiosity procured me this account, from the sober commander of a ship that came this year from the remoter parts of the great ocean, that at about 35 degrees of southern latitude the tallow with which his sounding lead was anointed, brought him up grey sand from the immense depth of no less than two hundred and twenty fathom. But to this observation it is just to annex this caution, that we cannot safely conclude from men's finding no ice at the bottom of the sea, that the cold there cannot be very intense; for, as I have found by more than one relation (\* elsewhere recited) that, whatever the schools surmise, the sea is at least as salt at the bottom, as at the top; so I have more than once tried, that salt water will, without freezing, admit a much greater degree of cold, than is necessary to turn fresh water into ice.

# RELATIONS

ABOUT THE

BOTTOM of the SEA.

## SECTION I.

**I** DO not pretend to have visited the bottom of the sea; but since none of the naturalists whose writings I have yet met with, have been there any more than I; and it is great rarity in those cold parts of *Europe* to meet with any men at all, that have had at once the boldness, the occasion, the opportunity, and the skill to penetrate into those concealed and dangerous recesses of nature, much less to make any stay there; I presume it will not be unpleasant, if about a subject, of which, though none of those very few naturalists, that write any thing at all, write otherwise than by hear-say, I recite in this place what I have learned by enquiry from those persons, that among the many navigators and travellers I have had opportunity to converse with, were the likeliest to give me good information about these matters.

It would be needless here to take notice that the sea is usually cold and salt at the bottom; nor to repeat those other things that I have already delivered in other discourses; I shall therefore begin what I have to say in this, by relating that one of the chief

\* Notes about the saltness of the sea.

things that I was solicitous to enquire after about the bottom of the sea, was, the inequality I supposed to be in the soil; for though the surface of the sea, when it is not agitated by the winds, appears very plain and level, and though it be indeed, at least in this, or that particular sea, spherical and (physically speaking) concentric to the earth; yet I could not think it probable, for reasons not necessary to be here discoursed of, that the bottom, the superficies of the ground, or of the vessel that contained it, should be either flat or level, or regularly concave.

To satisfy myself about this matter, I enquired of a person that had visited the famous pearl-fishing at the little island of *Manar* (near the rich isle of *Ceylon*) in the *East-Indies*, and had by his stay there much opportunity to see divers at their work, and converse with them. By the answers of this man, who was a scholar, I learned that the divers had assured him, that they found the floor of the sea, if I may so call it, in divers places, exceedingly unequal, in some places being flat, in others asperated with crabby rocks a considerable height, and elsewhere sinking into precipitous depths, in which they found it very cold.

BESIDES the recited testimonies of the divers, I enquired of several pilots and other navigators that had made long voyages, what gradual or abrupt inequality they had observed at their soundings in very neighbouring places; it being easy to be gathered from thence, whether the sea were there uniformly deep, or did at least, with some regularity, alter its depth by degrees; or whether, as I suspected, there were not at the bottom of the sea hilly places, and steep precipices, and, perhaps, deep vallies or wells, as we observe in the discovered part of the terrestrial globe.

By these inquiries I obtained several observations, whereof the most material are those that follow:

FIRST, an ancient sea-commander, that had many years frequented *Africa* and the *Indies*, told me, as others had done before, that when they sailed in the ocean very far from sight of land, they did not often put themselves to the trouble of sounding; but that as far as they had sounded, he had usually found the depth of the sea to increase or decrease gradually, without very great irregularities, excepting some places, instancing particularly in the excavation that makes the bottom of the sea, within sight of the *Cape of Good Hope*, where though for the most part, he found the water to deepen more and more, as he sailed farther from shore; yet in one place, he and others had met with a bank (as he conceived it to be) at a considerable distance from the surface of the water; so that though when they were as they imagined near the edge of that bank, they found but a moderate number of fathoms, yet when sailing a very little way farther they had gone beyond it, they found the sea of an immense depth. In short I gathered from his answers, that in the greater seas he had found, for the most part, the ground at the bottom to fall away by degrees; but nearer the shores, that is, within a moderate number of leagues, he observed in divers places, that the submarine ground was very unequal, and had as it were, hills and precipices.

A MAN of letters that had sailed both to the *East* and *West Indies*, and in divers other regions besides, and had made some of his voyages in ships of such great burthen, as obliged the mariners to be very frequent and careful in sounding, informed me, that sometimes at considerable distances from shore, he had observed the sea to be 20, 30, or perhaps 40 fathom deeper when they cast the sounding lead from one side of the ship, than it had been just before, when they had sounded from the other; and from other things that he told me, I found myself much confirmed in the above proposed opinion.

HEARING of a sea-captain of extraordinary skill in maritime affairs, that was come home this year from *East India*, his reputation made me endeavour to have a little conference



ference with him about the subject of this discourse ; but his occasions hastening him to another place before I could fend to him, I procured from the chief persons that employed him a sight of some notes touching his last voyage, which he had left with them, hoping to find there something at least about the foundings of so accurate a seaman ; and accordingly I met with a passage very pertinent to my purpose, and worthy to be here transcribed.

FEBRUARY 12. After our observation, (he means a former one very agreeable to this) seeing the ground under us, we heaved the lead, and had but 19 fathom rocky ground, then haled by N. N. E. the wind at N. W. and found our water to shoal from 19 to 10 and 8 fathom hard coral ground, then suddenly deepened again from 8 to 20 and 22 fathom sandy ground, and then suddenly saw rocks under us, where we had but 7 fathom, and the next cast 14 fathom again : and so having run N. N. E. from 6 in the morning until 12 at noon about 19 mile, we deepened our water, from 16 to 25, and the next cast, no ground with 35 fathom of line.

LASTLY, having opportunely met with an ancient navigator who passes for the most experienced pilot in our nation for an East-Indian voyage ; I asked him about his own observations concerning these unequal foundings, I was answered, that he had not only met with them elsewhere, but, that not far from the mouth of our channel, he had sometimes found the bottom of the sea so abrupt, that in sailing twice the length of the ship, he had found the water deepened from 30 fathom to a hundred, if not also much more.

SINCE I received these relations, having the honour to discourse with a noble person who has divers times deservedly had the command of English fleets, and is no less curious than intelligent in maritime affairs, I took the opportunity to inquire of his lordship whether he had not observed the bottom of the sea to be very unequal in neighbouring places ? To which he replied, that he had found it exceedingly so. And to satisfy me that he spoke not upon mere conjecture, he told me, that sailing once with his fleet, even in our channel, he perceived the water to make a rippling noise (as the seamen call it) as the *Tbames* does under *London Bridge*, so that he was afraid they were falling upon some shoal, the water being 12 or 14 fathom deep, and going on a little farther, he cast out the plummet again, and found it about 30 fathom. He added, that he made divers such observations, but took notice of such rippling waters only when the tide was ebbing ; and yet in a deep sea meeting with the like appearance in the upper part of the water, and thinking it improbable that there should be any shoal there, he ordered the depth to be sounded, and found it to exceed 30 fathoms ; and after he had passed on a very little farther, he found the sea so deep, that he could not fathom it with his ordinary line.

## SECTION II.

ANOTHER thing observed at the bottom of the sea is the great pressure of the water there against any other bodies. For whatever men may philosophize in their studies, and may conclude from the principles that are generally received about the non-gravitation of water in its proper place, yet experience seems very little to favour that general doctrine.

FOR first, I remember, that having caused a pretty large cylinder of glass that was open only at one end, to be so depressed into a large glass-vessel full of water, with a conveniently applied weight of lead, that none of the air could get out, I could easily discern

discern through the liquor and vessels, which were all transparent, that as the inverted cylinder descended deeper and deeper, the external water compressed the imprisoned air, and ascended higher and higher in the cavity of the cylinder, against whose side we had beforehand placed a row of marks whereby to take notice of the gradual ascent and descent of the internal water.

SECONDLY, having inquired of two several observing persons, whereof one had with a diving engine visited the bottom of the sea in a cold northern region; and the other had done the like in an engine much of the same sort upon the coast of Africk; I found their relations to agree in this, that the deeper they descended into the sea, the more the air they carried down with them was compressed, and the higher the water ascended above the lip or brim of the engine into the cavity of it.

BUT I shall now add a more considerable experiment or two to the same purpose. For discoursing one day with an engineer of my acquaintance that had been often at sea, and loved to try conclusions, of a way I had thought of, to make some estimate of the pressure of the water at a considerable depth beneath the surface, and shew that the pressure is great there; he told me, he could save me the labour of some trials by those he had made already, and assured me, that having divers times opportunity to sail near the straits mouth over a place where the sea was observed to be of a notable depth, he had found, that if he had let down, with a weight into the sea, not a strong round glass bottle, but a phial, such as the seamen use to carry their brandy and strong waters in; such a vessel, which might contain a pint or quart of water, would, when it come to be sunk 40 fathom under water, if not sooner, be so oppressed by the pressure of the incumbent and lateral water, as to be thereby broken to pieces.

He also averred to me, that having exactly closed an æolipile of metal, and with a competent weight sunk it to a great depth in the sea, as to forty, fifty, or sixty fathom deep, when he pulled it up again he found to his wonder that the great pressure of the water had in divers places crushed it inwards. And though I had some suspicion that the coldness of the sea at such a depth might, by weakening the spring of the included air, something contribute to the effect, yet I did not admire the event, having divers years before had a thin æolipile of copper crushed inwards by the pressure of a much lighter fluid than sea-water.

### SECTION III.

ANOTHER thing observed in the bottom of the sea is the tranquillity of the water there, if it be considerably distant from the surface. For though the winds have power to produce vast waves in that upper part of the sea that is exposed to their violence; yet the vehement agitation diminishes by degrees as the parts of the sea, by being deeper and deeper, lie more and more remote from the superficies of the water. So that the calm being less and less disturbed towards the bottom of the water, if that lie considerably deep, the water is there either calm or scarce sensibly disturbed.

BUT that is for the most part to be understood of places at some distance from the shore; for oftentimes, in those that are too near it, the progress of the waters being rudely checked, and other circumstances concurring, the commotion of the water is so great, that it reaches to the very bottom, as may appear by the heaps of sand, the amber, and in some places the stones that are wont to be thrown up by the sea in and after storms.



THE above-mentioned calmness of the sea at the bottom will (I doubt not) appear strange to many, who admiring the force of stormy winds and the vastness of the waves they raise, do not, at the same time, consider the almost incomparably greater quantity and weight of water that must be moved to make any great commotion at the bottom of the sea, upon which so great a mass of salt-water, which is heavier than fresh, is constantly incumbent. Wherefore, for the proof of the proposed paradox, I will here set down a memorable relation, which my inquiries got me from the diver, elsewhere mentioned, who by the help of an engine could stay some hours under water.

THIS person then being asked, whether he observed any operation of the winds at the bottom of the sea, where it was of any considerable depth? answered me to this purpose, that the wind being stiff, so that the waves were manifestly six or seven feet high above the surface of the water, he found no sign of it at 15 fathom deep; but if the blasts continued long, then it moved the mud at the bottom, and made the water thick and dark. And I remember he told me, which was the circumstance I chiefly designed, that staying once at the bottom of the sea very long, where it was considerably deep, he was amazed at his return to the upper parts of the water to find a storm there, which he dreamt not of, and which was raised in his absence, having taken no notice of it below, and having left the sea calm enough when he descended into it.

FOR farther confirmation, I shall add, that having inquired of a great traveller who had assisted at a rich pearl-fishing in the *East-Indies*, whether he had not learned by his conversation with the divers, that storms reach not to the bottom of the sea, if it be of any considerable depth; he answered, that he had seen the divers take the water when the sea was so very rough, that scarce any vessels would hazard themselves out of ports; that those returning divers told him, that at the bottom they had found no disturbance of the water at all; which is the more considerable, because of the situation of that place where they dive for pearls; for this is near the shore of *Manar*, and that itself is seated between the great island of *Ceylon* and the vast cape of *Comori*; and though it may be much nearer the former, is not yet far distant from the latter; which situation and the neighbourhood of the vast Indian ocean, on the one side of *Ceylon* and the great gulph of *Bengala* (antiently *Sinus Gangeticus*) on the other, makes the place where the pearls are fished for exceeding likely to be subject to very troubled seas.

IT will perhaps be thought no slight addition to the foregoing arguments, if I here add, that meeting one day with an ancient and expert seaman, whom his merit had advanced to considerable employments in his profession, I was confirmed by the enquiries I made of him, not only in the opinion I had about the calmness of the bottom of the sea, but also, that the operation of good gales of wind does oftentimes not reach to near so considerable depths into the sea, as hath been hitherto supposed, even by navigators themselves. For he assured me, that having sometimes sailed in great ships that drew much water, as about 12 or 15 feet, he had dived to the keel of the ships when they were under sail, and observed the agitations of the water to be exceedingly diminished and grown very languid even at that small distance from the upper part of the waves; and he farther answered, that when in *America* he learned to dive of the Indians, they taught him by their examples, to creep along by the rocks and great stones that lay near the shore, at the bottom of the water, to shelter themselves from the strokes and other ill effects of the billows, which near the shore, and where the sea was so shallow as it was there, did often hurt and endanger swimmers and unskilful divers. But when they were, by this means, got farther from shore and into deeper water, they would securely leave the shelter they had till then made use of, and swim within a few yards of the surface of the sea and commotions of the upper parts of the water.

BUT lastly, for further satisfaction, I had the opportunity to make inquiry about this matter of a great sea-commander, who has both an extraordinary curiosity to make marine



observations and an unusual care in making of them accurately, I found the opinion countenanced by his answer, which was in short, that he had lately been at a place where the sea was often tempestuous enough, and that they found by a sure mark, that the storm did not reach with any efficacy four fathom beneath the surface of the water.

ABOUT the tranquillity of the lower parts of very deep waters I had a suspicion which, though I fear it might seem somewhat extravagant, because I have not met with it in authors; yet I thought it worth examining for the use it might be of, if resolved, in reference to the ebbing and flowing of the sea.

I MADE, therefore, a solicitous inquiry whether the tides did reach to, or near the bottom of the deeper seas, but found it exceeding difficult, by reason of men's want of curiosity, to obtain any satisfaction about a problem that most navigators I have conversed with did not seem to have so much as dreamed of. But thus much I found, indeed, by inquiring of an engineer, who was curious of marine observations, that a famous sea-commander of his acquaintance, being also a great mathematician, had affirmed to this relator, that he had divers times observed, that when he let down his plummet to a great depth, but yet not to reach ground, it would be quickly carried by a motion quite contrary to that of the shallow, whence they sounded, and very much quicker than it; but I had this only at second hand. Also, if I misremember not, I was informed by a skilful observer, that commanded many of our English men of war, that he had, near the Sound, observed the upper and lower parts of the water to move with a considerable swiftness quite different ways; but not having committed this relation to writing, I dare not build much upon it. And among the answers I had received and written down concerning those matters, all that I can yet find among my adversaria is a relation which, though single, will not be unworthy to be transcribed in this place, because the person who gave it me is one of the ancientest and most experienced pilots of our nation.

THIS person, therefore, assured me, that sailing beyond the *Cape of Good Hope*, into the *South Seas*, he made trials of the motion of the upper part of the water above the lower, where sometimes casting out a large and heavy plummet, he let it down to several depths short of 50 fathom, without any sensible operation upon the motion of the boat or shallow he stood in to make the trial; but when he let down the plummet lower, to about an hundred fathom or more, then he found, that though the plummet reached not to the bottom of the water, yet upon the score of the standing water beneath, the superior water would make the boat turn towards the tide or current, as if it lay at anchor, and the water would run by the side of the boat at the rate of about three miles an hour; thus far this diligent observer: but how far the inequality of the soil at the bottom of the sea, and how far various depth of the water, and some other circumstances may alter the case, and make it hard to determine what ought to be ascribed to tides, and what to currents, are things which I will by no means be positive in, till I can meet with further information.

[SINCE the writing of this, happening to meet with one that spent some time at a famous eastern pearl-fishing, and asked him, whether he had inquired of the divers about the problem lately proposed, and whether the sea were there deep enough to make observations of that kind; to the latter part of which question he replied, that in some places it was of a very considerable depth, and fit to make the observation in; and to the former he answered, that he had inquired of the divers, who affirmed to him, that sometimes at the bottom of the deep waters there seemed to be a stagnation of the sea for a great depth, so that till such a height they could rise directly upwards, but that at other heights they would be carried away by the less deep waters; so as to be found, when they came to emerge, a great way off from that point of the surface, which was perpendicular to that place at the bottom, whence they began to ascend.]



NEW

## PNEUMATICAL EXPERIMENTS

ABOUT

## RESPIRATION.

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the 8th, 1670.

## TITLE I.

*Observations made about the lasting of ducks included in the exhausting receiver.*

NATURE having, as zoologists teach us, furnished ducks and other water-fowl with a peculiar structure of some vessels about the heart, to enable them, when they have occasion to dive, to forbear for a pretty while respiring under water without prejudice; I thought it worth the trial, whether such birds would, much better than other animals, endure the absence of the air in our exhausted receiver. The accounts of which trials were, when they were made, registered as follows:

## EXPERIMENT I.

WE put a full grown duck (being not then able to procure a fitter) into a receiver, whereof she filled, by our guess, a third part, or somewhat more, but was not able to stand in any easy posture in it; then pumping out the air, though she seemed at first (which yet I am not too confident of, upon a single trial) to continue well somewhat longer than a hen in her condition would have done; yet within the short space of one minute she appeared much discomposed, and between that and the second minute, her struggling and convulsive motions increased so much, that her head also hanging carelessly down, she seemed to be just at the point of death; from which we presently rescued her

her by letting in the air upon her; so that this duck being reduced, in our receiver, to a gasping condition, within less than two minutes, it did not appear, that, notwithstanding the peculiar contrivance of nature, to enable these water-birds to continue without respiration for some time under water, this duck was able to hold out considerably longer than a hen or other bird, not aquatick, might have done; and to manifest that it was not closeness and narrowness of the vessel in reference to so bulky an animal that produced in the subject of our trial the great and sudden change above-recited, we soon after included the same bird in the same receiver, and having by a special way cemented it on very close, we suffered her to stay thus shut up with the air for five times as long as formerly (by our guests, helped by a watch) without perceiving her to be discomposed; and she would probably have continued longer in the same condition, if my patience and leisure would have held out so long as she could have done in that prison.

## EXPERIMENT II.

HAVING at the season of the year procured a duckling that was yet callow, we conveyed her into the same receiver wherein the former had been included, and observed, that, though for a while she appeared not much disquieted, whilst the air was pumping out of the glass, yet before the first minute was quite ended, she gave manifest tokens of being much disordered; and the operation being continued a while longer, she grew so much worse, that several convulsive motions she fell into before a second minute was expired, obliged us to let in the air upon her, whereby she quickly recovered.

N. B. I DETERMINE not whether it be proper in this place to add, that when the receiver was pretty well exhausted, the included bird appeared to the spectators manifestly bigger than before the air was drawn, especially about the crop, though that was very turgid before. And to manifest, that in this duck, as in the former, the convulsions that used to be immediately followed by death, proceeded from the withdrawing of the ambient air, and not from the clogging of it; we kept the same duckling in the same receiver very close, to keep out all external air, and to keep in the excrementitious steams of her body for above 6 minutes, without perceiving her to grow sick upon her imprisonment; which yet lasted above thrice the time that sufficed to reduce her in the absence of the air to a gasping condition.

N. B. It not being intended that ducks and other water-fowl should any more than other birds, live in an exceeding rarefied air, but only be able to continue upon occasion a pretty while under water, it may suffice that the contrivance of those parts which relate to respiration, be so far fitted for the purpose, as we shall see it is, when we come to the tenth title.

## T I T L E II.

### *Of the phenomena afforded by vipers included in an exhausted receiver.*

CONSIDERING that vipers are animals endowed with lungs (though of a different structure from those of men, dogs, cats, and birds, &c.) and that their blood is, as to sense, actually cold; I thought it might, upon both these accounts, be very well worth trying, what effect the withdrawing and absence of the air would have upon animals so constituted. I therefore made divers trials, some of which did not displease me; but I know



not by what misfortune the memorials of them were lost, except two or three, which were not perfect, that I shall here subjoin.

### EXPERIMENT I.

We included a viper in a small receiver, and as we drew out the air, she began to <sup>Jan. 2.</sup> swell, and afforded us these phenomena. <sup>1663,</sup>

1. It was a good while after we had left pumping before the viper began to swell so much as to be forced to gape, which afterwards she did.
2. THAT she continued, by our estimate, above two hours and half in the exhausted receiver without giving clear proof of her being killed.
3. THAT after she was once so swelled as to be compelled to open her jaws, she appeared slender and lank again; and yet very soon after appeared swelled again, and had her jaws disjoined as before.

### EXPERIMENT II.

WE took a viper, and including her in the greatest sort of small receivers, we emptied the glass very carefully, and the viper moved up and down within, as if it were to seek for air, and after a while foamed a little at the mouth, and left of that foam sticking to the inside of the glass, her body swelled not considerably, and her neck less, till a pretty while after we had left pumping; but afterwards the body and neck grew prodigiously tumid, and a blister appeared upon the back. An hour and an half after the exhaustion of the receiver, which we then by trial found to be pretty stanch, the distended viper did give by motion manifest signs of life; but we observed none afterwards. The tumor reached to the neck, but did not seem much to swell the under-chap, both the neck and a great part of the throat, being held betwixt the eye and the candle, were transparent enough where the scales did not darken them. The jaws remained mightily opened, and somewhat distorted; the epiglottis with the rimula laryngis, which remained gaping, was protruded almost to the farther end of the nether-chap. As it were from beneath this epiglottis came the black tongue and reached beyond it, but seemed by its posture not to have any life, and the mouth also was grown blackish within, but the air being re-admitted after 23 hours in all, the viper's mouth was presently closed, though soon after it was opened again, and continued long so; and scorching or pinching the tail made a motion in the whole body that argued some life.

### EXPERIMENT III.

To these experiments upon vipers I shall add one made upon an ordinary harmless snake. April 25.

WE included such an animal, together with a gage, in a pretty portable receiver, which, being exhausted and well secured against the ingress of the air, was laid aside in a quiet place, where it continued from 10 or 11 of the clock in the forenoon, till about nine the next morning; and then my occasions calling me abroad, I looked upon the snake, which, though he seemed to be dead, and gave no signs of life upon the shaking of the receiver; yet, upon holding the glass a convenient distance from a moderate fire, he did in a short time manifest himself to be alive by several tokens, and even by putting forth:

forth his forked tongue. In that condition I left him, and, by reason of several avocations, came not to look upon him again till the next day early in the afternoon; at which time he was grown past recovery, and his jaws, which were formerly shut, gaped exceeding wide, as if they had been stretched open by some external violence.

## T I T L E III.

*Of the phenomena afforded by frogs in an exhausted receiver.*

Sept. 9, 1662. THE same considerations that induced me to make several trials upon vipers, did also invite me to make several upon frogs; the success of some of which the following notes will declare.

## E X P E R I M E N T I.

WE took a large lusty frog, and having included her in a small receiver, we drew out the air, and left her not very much swelled, and able to move her throat from time to time, though not so fast, as when she freely breathed from the exsuction of the air. She continued alive two hours, that we took notice of, sometimes removing from the one side of the receiver to the other, but she swelled more than before, and did not appear by any motion of her throat or thorax to exercise respiration, but her head was not very much swelled, nor her mouth forced open. After she had remained there somewhat above three hours (for it was not  $3\frac{1}{2}$  hours, *i. e.* 3 hours and a half) perceiving no sign of life in her, we let in the air upon her, with which the formerly tumid body shrunk very much, but seemed not to have any other change wrought in it; and though we took her out of the receiver, yet in the free air itself, she continued to appear stark dead. Nevertheless, to see the utmost of the experiment, having caused her to be laid upon the grass in a garden all night, the next morning we found her perfectly alive again.

## E X P E R I M E N T II.

June 29, 1660. ABOUT 11 of the clock in the forenoon we put a frog into a small receiver, containing about  $15\frac{1}{2}$  ounces troy weight of water, out of which we had tolerably well drawn the air (so that when we turned the cock under water, it sucked in about  $13\frac{1}{2}$  ounces of water); the frog continued in it (the receiver all the while under water) lively enough until about 5 of the clock in the afternoon, when it expired. The frog at the first seemed not to be much altered by the exsuction of the air, but continued breathing both with her throat and lungs.

## E X P E R I M E N T III.

Sept. 6, 1662. WE included into a pretty large receiver a couple of frogs newly taken, the one not above an inch long, and proportionably sleeker; the other very large and lusty. Whilst the air was drawing out, the lesser frog skipped up and down very lively, and, somewhat to our wonder, clambered up several times to the sides of the receiver, insomuch, that  
he



he sometimes rested himself against the side of the glass. When his body seemed to be perpendicular to the horizon, if not in a reclining posture, he continued to skip up and down a while after the extraction of the air; but within a quarter of an hour (measured by a minute watch) we perceived him to lie stark dead with his belly upwards. The other frog, that was large and strong, though he began to swell much upon the withdrawing of the air, and seemed to be distressed by his frequently leaping up after the air was drawn out, which he did not before, yet being, as we said, very lusty, he held out half an hour, at which time it was remarkable, that the receiver, though it had held out against the pressure of the outward air during that space of time, notwithstanding that a piece of it had been cracked out, and was mended, with a cloth dipped in cement, yet, at the end of the half hour the weight of the outward air suddenly beat it in, and thereby brought the imprisoned frog a reprieve, which hindered us from bringing the experiment to an issue.

## EXPERIMENT IV.

WE took a small frog, and having conveyed her into a very small portable receiver, Sept. 11. we began to pump out the air. At first she was lively enough, but when the air began to be considerably withdrawn, she appeared to be very much disquieted (leaping sometimes after an odd manner, as it were, to get out of the uneasy prison, but yet not so, but, that after the operation was ended, and the receiver taken off, the frog was perfectly alive, and continued to appear so. (if I am not mistaken) near an hour, though the abdomen was very much, and the throat somewhat extended; this latter part having also left that wonted panting motion that is supposed to argue and accompany the respiration of frogs. At the end of about three quarters of an hour, after the removal of the receiver from the pump, the air was let in; whereupon the abdomen, which by that time was strangely swelled, did not only subside, but seemed to have a great cavity in it, as the throat also proportionably had; which cavities continued, the frog being gone past all recovery.

## EXPERIMENT V.

A LARGE frog was conveyed into a plated receiver, and the air being withdrawn, her April 14. body by degrees was distended; as appeared very notably, when by a casual springing of a leak the air got in again, and made her look much more lank and hollow than ever. The receiver, with the gage, were kept under water near seven hours, because I was obliged to stay long abroad; at the end of which, coming home, I found the receiver stanch, but the frog dead and exceedingly swelled; upon the letting in of the air, she became more hollow and lank than ever.

N. B. I HAVE purposely, both under this title, and some others, subjoined some trials, whose events are not altogether such, as others recited under the same head, would invite one to expect; but I purposely do it, not only to be true to the impartiality I proposed to myself in writing these narratives, but to awaken the curious to consider and observe what variety of phenomena in such trials may be attributed to the season of the year wherein they are made; and to strength, bulk, age, peculiar constitutions, &c. that relate to the respective animal on which the experiments are made; besides what things may on other account be fit to be also considered.

T I T L E

## T I T L E IV.

*Of the phenomena afforded by a new kittened kitling in the exhausted receiver.*

BEING desirous to try whether animals that had lately been accustomed to live, either without any, or without a full respiration, would not be more difficult or slowly killed by the want of the air, than others, which had been longer used to a free respiration; we took a kitling that had been kittened the day before, and put it into a very small receiver (that we guessed to hold about a pint or less) that it might be the sooner exhausted. As soon as the pump began to play, I took notice of the time, and found by a watch, that marks minutes and quarter minutes, within one minute, or little more, after the air first began to be withdrawn, that the little animal, who, in the mean time had gasped for life, and had some violent convulsions, lay as dead, with his head downwards, and his tongue out; but upon letting in of the air, he did in a trice shew signs of life, and being taken out of the receiver, quickly recovered; and to allow him the benefit of his good fortune, we sent for a kitling of the same age and litter, which being put into the same receiver, quickly began, like the other, to have convulsions, after which he lay as dead; but observing very narrowly, I perceived some little motions, which made me conclude him alive; which I soon found I had cause to do. For though we continued pumping, and could not perceive that the engine leaked more than in the former experiments; the kitling began to stir again, and after a while had stronger and more general convulsions than before; until at the end of full six minutes after the exclusion of the air was begun, the animal seeming quite dead, the outward air was re-admitted into the receiver, which not reviving him as it had done the other, he was taken out of the vessel, and lay with his mouth open, and his tongue lolling out, without any sensible breathing and pulsation; until having ordered him to be pinched, the pain, or some internal motion, produced by the external violence done to him, made him immediately give manifest signs of life, though there was yet no sensible motion of the heart or the lungs; but afterwards gaping and fetching his breath in an odd manner, and with much straining, as I have seen some fœtuses do, when cut out of the womb, he, little by little, within about a quarter of an hour, recovered; wherefore, thinking it severe to make him undergo the same measure again, we sent for another, kittened at the same time, and inclosing that also in the receiver, observed that divers violent convulsions, as it were, gasping for breath, into which he began to fall at the second or third suck, ended in a seeming death, within about a minute and a half; but, being made more diffident by the late experiments, I caused the pump to be plied, and the rather, because I had a mind to observe, whether, when the air was from time to time drawn away, there would not, upon the opening of the stop-cock to let it out, appear some sudden swelling, greater or less, of the body of the animal, by the spring and expansion of some air (or aerial matter) included in the thorax or the abdomen. Such an inflation (though not great) we thought we observed; but until farther trial, I dare not acquiesce in it. A while after, notwithstanding our continuing to pump, the kitling gave manifest signs of life, which was not until it had endured divers convulsions as great as those of the first fit, if not greater. When 7 minutes from the beginning of the exhaustion were completed, we let in the air; upon which the little creature that seemed stark dead before, made us suspect that he might recover; but though we took him out of the receiver, and put aqua vitæ into his mouth, yet he irrecoverably died in our hands.

THESE



THESE trials may deserve to be prosecuted with farther ones, to be made not only with such kittens, but with other very young animals of different kinds; for by what has been related, it appears that those animals continued 3 times longer in the exhausted receiver, than other animals of that bigness would probably have done.

T I T L E V.

*Some trials about the air usually harboured and concealed in the pores of water, &c.*

IT might assist us to make the more rational conjectures about the phenomena of divers of our experiments, (if we knew (something near) what quantity of aerial substance is usually found in the liquors we employ about them, especially in that most common of them, water; and therefore, though it be very difficult (if at all possible) to determine the proportion of the air that lurks in water, with any thing of certainty, many circumstances making it subject to vary very much; yet, to make the estimate, I easily could, where none at all, that I know of, hath been hitherto made by any man, I considered that it might afford us some light, if we discovered, at least, what proportion, as to bulk, the air latent in a quantity of water would have to the liquor it came from, when the aerial particles should be gathered together into one place. For though, about this union, and the spring that may be consequent to it, some doubts may be suggested, which I have not now time to discuss; yet I supposed, that, at least, some discoveries may by this way be made, though not of the true proportion between the air and the water, yet about two or three particulars, in due time to be taken notice of.

To find instruments, which would any way accommodate our purpose, proved a very difficult work; so that among other things that we were fain to do, this was one, that to evince how little the air latent in water, did appear to lessen the bulk of that water, if it were suffered to fly away in an open tube; we suffered it to escape in an exhausted receiver, without any artifice to catch it; by which trial the water did not part with any thing of its bulk that made a diminution sensibly to the eye. Wherefore we endeavoured to make this loss visible by some other trials, of which I can find but a few hasty memorials among my loose entries.

A CHEMICAL pipe sealed at one end, and 36 inches, or somewhat less, in length, was filled with water, and inverted into a glass vessel, not two inches in diameter, but  $\frac{1}{4}$  of an inch, or little more in depth. These glasses being conveyed into a fit receiver, and the air being leisurely pumped out, and somewhat slowly re-admitted, the numerous bubbles that had ascended during the operation, constituted at the top an aerial aggregate, mounting to  $\frac{8}{10}$ , wanting about an hundredth part of an inch.

PRESENTLY after the tube (by and by to be described) was filled again with the same water, and inverted; and the water being drawn down to the surface of the vesseled water, and the air let in again, the water was impelled up to the very top, within a tenth and half a tenth of an inch. These are two experiments.

THE tube for measuring the air latent in water was 43 inches and  $\frac{1}{2}$  above the surface of the stagnant water; the air collected out of the bubbles at the top of the water was the first time  $\frac{1}{4}$  of an inch, and somewhat better; the second time we estimated it but  $\frac{1}{2}$  an  $\frac{1}{10}$ . The first time the water in the pipe was made to subside full as low as the surface of the restagnant water; the second time the lowest we made it subside seemed to be four or five inches above the surface of the water in the open vessel.

MATTER of fact thus recited would afford divers difficulties worthy to be considered, which I have not leisure to discuss; especially, the odd thing that happens to the aerial particles of water; for though, whilst they lay concealed in the water, they took up so little room in it that it was insensible; and when they were permitted to escape out of the tube, the water was not manifestly diminished by their recess; yet, when they were associated at the top of the tube, their aggregate did sometimes maintain a place that was considerable enough in reference to the capacity of the whole tube, though I must here advertize that this aggregate did, at the top of the tube, possess more room than its bulk did absolutely require; because it was somewhat defended from the pressure of the atmosphere by the weight of the subjacent cylinder of water, which might be about three or four feet long.

QUERE, Whether any considerable proportion of bubbles will be afforded by the same liquor, if it be suffered to continue in the glass for some competent time after it has been once, or oftener, freed from bubbles already?

QUERE, How far it may be worthy our consideration, whether, in common water, there may not be concealed air enough to be of use to such cold animals as fishes; and whether it may be separable from the water that strains through their gills?

BUT though I was at first content to make use of this way of estimating the air concealed in water; yet, when I came where I could be a little better accommodated with glasses, I bethought myself of a small instrument that would much better disclose the wonderful plenty of the aerial particles I designed to discover. The structure and use of this glass may be easily enough understood by the recital of the first experiment that was made with it, whereof take the following transcript.

WE provided a clear round glass, furnished with a pipe or stem of about nine inches in length, the globulous part of the glass being on the outside about three inches and half in diameter; the pipe of this glass was within an inch of the top, melted at the flame of a lamp, and drawn out for two or three inches as slender as a crow's quill, that the decrement of the water upon the recess of the air harboured in its pores, might, if any should happen, be the more easily observed and estimated. Above this slender part of the pipe, the glass, as was before intimated, was of the same largeness, or near it, with the rest of the pipe, that the aerial bubbles, ascending through the slender part, might there find room to break, and so prevent the overflowing or loss of any part of the water.

THIS vessel being not without difficulty and some industry filled, till the liquor reached to the top of the slender part, where not being uniformly enough drawn out, it was somewhat broader than elsewhere; we conveyed the glass, together with a pedestal for it to rest upon, into a tall receiver, and pumping out the air, there disclosed themselves numerous bubbles, ascending nimbly to the upper part of the glass, where they made a kind of froth or foam, but, by reason of the above-mentioned figuration of the vessel, they broke at the top of the slender part, and so never came to overflow.

THIS done, the pump was suffered to rest a-while to give the aerial particles lodged in the water time to separate themselves and emerge, which when they had done a pretty while, the pump was plied again for fear some air should have stolen into so large a receiver; these vicissitudes of pumping and resting lasted for a considerable time, till at length the bubbles began to be very rare and we weary of waiting any longer; soon after which the external air was let into the receiver, and it appeared somewhat strange to the spectators, that notwithstanding so great a multitude of bubbles as had escaped out of the water, I could not, by attentively comparing the place where the surface of the water rested at first, to which a mark had been affixed, with that where it now stood; I could not, I say, discern the difference to amount to above, if so much as an hair's breadth;

and



and the chief operator in the experiment professed, that, for his part, he could not perceive any difference at all.

THUS far for the narrative of the trial made by water; but that was not the only liquor into whose aerial particles I designed, by our little instrument, to inquire, and therefore filling a glass of the same shape, and much of the same bigness, with claret wine, and placing it upon a convenient pedestal, in a tall receiver, we caused some of the air to be pumped out; whereupon, in a short time there emerged, through the slender pipe, so very great a multitude of bubbles that were darted, as it were, upwards, as did not a little both please and surprize the beholders; but it forced us to go warily to work, for fear the glass should break, or the wine overflow. Wherefore we seasonably left off pumping before the receiver was any thing near exhausted, and suffered the bubbles to get away as they could, till the present danger was over-passed; and then from time to time we pumped a little more air out of the receiver, till we were weary, the withdrawing of a moderate quantity of air at a time sufficing, even at the latter end, to make the bubbles not only copiously, but very swiftly to ascend (by a minute watch) for above a quarter of an hour together.

THE little instrument made use of about these trials, being designed to examine, among other things, the quantity of bubbles lurking in several liquors, is to be applied to spirit of wine and chemical oils, that are more subtle liquors than wine itself. And some circumstances of our trials made us think that it might be worth examining what kind of substance may be obtained by this way of handling aerial and spirituous corpuscles. But of the other uses of our instrument elsewhere.

## T I T L E VI.

*Of some phenomena afforded by shell-fishes in an exhausted receiver.*

### EXPERIMENT I.

AN oyster being put into a very small receiver, and kept in long enough to have successively killed three or four birds or beasts, &c. was not thereby killed, nor, for aught we could perceive, considerably disturbed; only at each suck we perceived that the air contained between the two shells broke out at their commissure; as we concluded from the foam which at those times came forth all round that commissure. About twenty-four hours after, coming to see in what condition this oyster was, I found that both this and another that had been put at the same time into the receiver, were alive; but how long afterwards they continued so, I did not observe.

### EXPERIMENT II.

THAT same day we put a pretty large craw-fish into a pretty large receiver, and found, that though he had been injured by a fall before he was brought thither, yet he seemed not to be much incommoded by being included, till the air was in great measure pumped out; and then its former motion presently ceased, and he lay as dead, till upon the letting in a little air into the receiver, he began forthwith to move afresh; and upon the withdrawing the air again, he presently, as before, became moveless. Having

Z z 2

repeated

repeated this trial two or three times, we took him out of the receiver, where he appeared not to have suffered any harm.

### EXPERIMENT III.

BUT I thought it not unlikely that there may be some such inequality in the strength or vivacity of animals, as to such kind of experiments as ours, that it might be well worth while in several cases to reiterate our trials. And on this occasion I shall here add, that having put an oyster into a phial full of water, before we included it in the receiver, that through the liquor the motion of the bubbles, expected from the fish, might be the more pleasantly seen and considered; this oyster proved so strong, as 'to keep itself close shut, and repressed the eruption of the bubbles,' that in the other did force open the shells from time to time, and kept in its own air as long as we had occasion to continue the trial.

### EXPERIMENT IV.

MOREOVER a craw-fish that was thought more vigorous, being substituted in the place of the former craw-fish, though once he seemed to lose his motion together with the air, yet afterwards he continued moving in the receiver, in spite of our pumping, whether, because there was some unperceived leaking, that hindered a sufficient exhaustion of the air; or because this particular animal was more strong or vivid than the other, we could not positively determine.

### TITLE VII.

#### *Of the phenomena of a scale-fish in an exhausted receiver.*

THE following experiment is far from being the first that was made on a scale-fish in our vacuum; but in regard that in the receivers wherein those trials were made, the external air could not be kept out near so long, and so well as in the vessel I am about to mention; I judged it well worth the pains to observe, what would happen to a fish in an exhausted vessel, where it should be kept for some hours together from all supply of fresh air. And therefore I made several trials to that purpose, whereof that which I think the most considerable was registered as follows:

WE took a receiver shaped almost like a bolt-head, containing by estimation near a pint, and the globulous part of it being almost half full of water, we put into it, at the orifice (which was pretty large) a small gudgeon about three inches long, which, when it was in the water, swam nimbly up and down therein. Then having drawn out the air so well, that we guessed by a gage, that about nineteen parts of twenty, or more might be exhausted, we secured ourselves, that the regrefs of the air should not injure our experiment; about which we observed these particulars.

FIRST, The neck of the glass being very long, though there appeared great store of bubbles all about the fish, yet the rest of the water, notwithstanding the withdrawing of so much air as has been mentioned, emitted no froth and but few bubbles.

SECONDLY,



**SECONDLY,** The fish both at his mouth and gills did for a great while discharge such a quantity of bubbles as appeared strange, and for about half an hour or more (for much longer I had not an opportunity to watch it) whenever he rested awhile new bubbles would adhere to many parts of his body (as if they were generated there) especially his fins and tail, so that he would appear almost beset with bubbles; and if, being excited to swim, he was made to shake them off, he would quickly, upon a little rest, be beset with new ones as before.

**THIRDLY,** Almost all the while he would gape and move his gills, as before he was included; though towards the end of the time that I watched, it often happened, that he neither took in, nor emitted any aerial particles, that I could perceive.

**FOURTHLY,** After a while he lay almost constantly with his belly upwards, and yet would in that posture swim briskly as before.

**FIFTHLY,** Nay, after a while he seemed to be more lively than at first putting in; whether by reason that by discharge of so many bubbles, which, by their distension, perhaps put him to pain, he found himself relieved, or for some other cause, I examine not.

HAVING occasion to go abroad, I returned about an hour and a half after he had been sealed up, and found him almost free from bubbles, and with his belly upwards, and seeming somewhat tumid, but yet lively as before. But an hour and a quarter after that, when rising from dinner, I went to look upon him again, he seemed to be moveless, and somewhat stiff; yet, upon shaking the glass, observing some faint signs of life in him by some languid motions, he attempted to make when excited to them, I opened the receiver under water, to try if that liquor and air would recover him; and the external water, rushing in till it had filled the vacant part of the ball and the greatest part of the stem too, the fish sunk to the bottom of it, with a greater appearance than ever of being alive; in which state, after he had continued a pretty while, I made a shift, by the help of the water he swam in, to get him through the pipe into a basin of water, where he gave more manifest signs of life, but yet for some hours lay on one side or other without being able to swim or lie on his belly, which appeared very much shrunk in, as if something during the time of its being sealed up had been broken in his body, or his belly had been exceedingly distended, beyond restitution to its former tone.

ALL the while he continued in the basin of water, though he moved his gills as before he had been sealed up, yet I could not perceive that he did, even in his new water, emit, as formerly, any bubbles, though two or three times I held him by the tail in the air, and put him into the water again, where at length he grew able to lie constantly upon his belly, which yet retained much of its former lankness; and though it be now about or above twenty-four hours, since he was first included, he continues yet alive.

(**POSTSCRIPT.** He lived in the basin eight or ten days longer; though divers guageons since taken died there in much fewer days).

## T I T L E VIII.

*Of two animals included, with large wounds in the abdomen, in the pneumatical receiver.*

## E X P E R I M E N T I.

Sept. 12. A SMALL bird, having the abdomen opened almost from flank to flank, without injuring the guts, was put into a small receiver, and the pump being set to work, continued for some little time without giving any signs of distress; but at the end of about a minute and a half from the beginning of the exhaustion, she began to have convulsive motions in the wings; and though the convulsions were not universal, or did appear violent, as is usual in other birds, from whom the air is withdrawn by the engine, yet at the end of two full minutes, letting in the air, and then taking off the receiver, we found the bird irrecoverable; notwithstanding which we did not find any notable alteration in the lungs, and found the heart, or at least the auricles of it, to be yet beating, and so it continued for a while after.

## E X P E R I M E N T II.

Sept. 12. WE took also a pretty large frog, and having, without violating the lungs or the guts, made two such incisions in the abdomen, that the two curled bladders or lobes of the lungs came out almost totally at them, we suspended the frog by the legs in a small receiver; and after we had pumped out a good part of the air, the animal struggled very much, and seemed to be much disordered; and when the receiver was well exhausted she lay still for a while, as if she had been dead, the abdomen and thigh very much swelled, as if some rarefied air or vapour forcibly distended them; but as, when the frog was put in, one of the lobes was almost full, and the other almost shrunk up, so they continued to appear after the receiver had been exhausted; but upon letting in of the air, not only the body ceased to be tumid, but the plump bladder appeared for a while shrunk up as the other, and the receiver being removed, the frog presently revived and quickly began to fill the lobe with air.

## T I T L E IX.

*Of the motion of the separated heart of a cold animal in the exhausted receiver.*

WITHOUT discussing the opinions of learned men about the connection and dependency of the motions of the blood and beating of the heart, I thought it might give me a sufficient inducement to make the following experiment, that several sorts of animals would be presently killed in our vacuum by the withdrawing of the air; and even the insects mentioned in the formerly published digression about respiration, though they also were not totally deprived of life by the absence of the air, yet they were of visible motion; wherefore some good hint or other being to be hoped for from the discovering whether or no a separated heart, which is but a part of an animal, would continue its motion



motion in our vacuum; we made some trials to that purpose, whose success I find thus set down.

EXPERIMENT I.

THE heart of an eel being taken out and laid upon a plate of tin in a small receiver, when we perceived it to beat there, as it had done in the open air, we exhausted the vessel, and saw, that, though the heart grew very tumid, and here and there sent forth little bubbles, yet it continued to beat as manifestly as before, and seemed to do so more swiftly, as we tried, by numbering the pulsations it made in a minute, whilst it was in the exhausted receiver, and when we had re-admitted the air, and also when we took it out of the glass, and suffered it to continue its motion in the open air. The heart of another eel, being likewise taken out, continued to beat in the emptied receiver as the other had done.

EXPERIMENT II.

THE heart of another eel, after having been included in a receiver, first exhausted, and then accurately secured from leaking, though it appeared very tumid, continued to beat there an hour; after which, looking upon it, and finding its motion very languid, and almost ceased, by breathing a little upon that part of the glass where the heart was, it quickly regained motion, which I observed a while; and an hour after, finding it to seem almost quite gone, I was able to renew it by the application of a little more warmth. At the end of the third hour, coming to look at it once more, a bubble, that appeared to be placed between the auricle and the heart, seemed to have now and then a little trembling motion; but I found it so faint, that I could no more by warmth excite it, so as plainly to perceive the heart to move, wherefore I suffered the outward air to rush in, but could not discern, that thereby the heart regained any sensible motion, though assisted with the warmth of my breath and hands.

T I T L E X.

*A comparison of the times, wherein animals may be killed by drowning or withdrawing of the air.*

To help myself and others to judge the better of some difficulties concerning respiration, I thought it might be useful that we compared together the times wherein animals may be killed by that want of respiration, which, in those that are drowned, is caused by the water that suffocates them, and that other want, which proceeds from withdrawing the ambient air. Of the latter of these, a sufficient number of instances is to be met with among our other experiments, and therefore I shall now subjoin about the former the more trials, because this comparison hath not, that I know of, been yet thought on by any.

## E X P E R I M E N T I.

Sept. 10. A GREEN-FINCH, having his legs and wings tied to a weight, was gently let down into a glass-body filled with water; the time of its total immersion being marked; at the end of half a minute after that time, the strugglings of the bird seeming finished, he was nimbly drawn up again, but found quite dead.

## E X P E R I M E N T II.

WHEREUPON a sparrow, that was very lusty and quarrelsome, was tied to the same weight and let down after the same manner; but though he seemed to be under water more vigorous than the other bird, and continued struggling almost to the very end of half a minute from the time of his being totally immerfed (during which stay under water there ascended, from time to time, pretty large bubbles from his mouth) yet notwithstanding that as soon as ever the half minute was completed, he was drawn up, we found him, to our wonder, irrecoverably gone.

## E X P E R I M E N T III.

A SMALL mouse, being held under water by the tail, emitted from time to time divers aërial bubbles out of his mouth, and at last, as one of the spectators affirmed, he saw at one of his eyes; being taken out at the end of half a minute and some seconds, he yet retained some motions; but they proved but convulsions, which at last ended in death.

“ By what is related under the first title, it does not appear that water-fowl, at least, that ducks could, in our receivers, endure the want of air much longer than other birds; but now to shew that the contrivance of nature is not insignificant, as to the enabling them to continue much longer under water without fresh air than the land-birds above mentioned, it will not be amiss to subjoin the two following experiments.”

## E X P E R I M E N T IV.

WE took the duck mentioned in the first title, and so tied a considerable weight of lead to her body, as it did not hinder her respiration, and yet would be sure to keep her down under water; which we had found that a small weight would not do by reason of her strength, nor yet a great weight, if tied only to her feet, in such a middle-sized tube as ours was, because of the height of her neck and beak. With the above-mentioned clog, the duck was put into a tub full of clear water, under whose surface she continued about a minute, by my watch, quietly enough, but afterwards began to appear for a while much disturbed; which fit being over, our not perceiving any motion in her made us, at the end of the second minute, take her out of the water to see in what condition she was, and finding her in a good one, after we had allowed her some breathing-time to recruit herself with fresh air, we let her down again into the tub, which in the mean time had been filled with fresh water, left the other, which had been troubled with



with the steams and foulness of the duck's body, might either hasten her death by its being infected with them, or hinder our discerning what should happen by its being opacated by them.

THE bird being thus under water, did, after a while, begin, and from time to time continue to emit divers bubbles at her beak. There also came out at her nostrils divers real bubbles from time to time; and when the animal had continued about two minutes or better under water, she began to struggle very much, and to endeavour either to emerge or change postures; the latter of which she had liberty to do, but not the former. After four minutes the bubbles came much more sparingly from her; then also she began to gape from time to time (which we had not observed her to do before) but without emitting bubbles; and so she continued gaping until near the end of the sixth minute, at which time all her motions, some of which were judged convulsive, and others, that had been excited by our rousing her with a forceps, appeared to cease, and her head to hang carelessly down as if she was quite dead. Notwithstanding which, we thought fit, for greater security, to continue her under water a full minute longer, and then finding no signs of life, we took her out, and being hung by the heels and gently pressed in convenient places, she was made to void a pretty quantity of water, of which, whether any had been received into the lungs themselves, we had not time and opportunity to examine. But all the means that were to recover the bird to life, proving ineffectual, we concluded she had been dead a full minute before we removed her out of the water; so that, to sum up the event of our experiment, even this water bird was not able to live in cold water without taking in fresh air, above six minutes; which is but  $\frac{1}{10}$  of an hour.

#### EXPERIMENT V.

THE duckling mentioned in the first title, and second experiment, having a competent weight tied to her legs, was let down into a tub of water which reached not above an inch or two higher than her beak: during the most part of her continuance there came out store of bubbles at her nostrils; but there seemed to come out more and greater from a certain place in her head, almost equidistant from her eyes, but somewhat less remote from her neck than they. Whilst she was kept in this condition, she seemed frequently to endeavour to dive lower under the water, and after much struggling and frequent gaping, she had divers convulsive motions, and then let her head fall down backward, with her throat upwards. To which moveless posture she was reduced at the end of the third minute, if not a little sooner; but a while after there appeared a manifest, but tremulous motion in the two parts of her bill, which continued for some time, but afforded no circumstances whereby we could be sure that they were not convulsive motions; but these also ceasing upon the end of the fourth minute, the bird was taken out and found irrecoverable.

#### EXPERIMENT VI.

A VIPER that was kept so many hours in an exhausted receiver till it was concluded to be stark dead, and to have been so for a good while, was nevertheless resolutely hindered by me from being thrown away, till I had tried what could be done by keeping it all night in a glass-body upon a warm digestive furnace: whereupon this viper was

found the next morning not only to be revived, but to be very lively, so as to invite me to make with her, without seeking for another, the following experiment.

WE put her into a tall glass-body, fitted with a cork to the orifice of it, and depressed with weight, so that she could come at no air. In this case we observed her from time to time; and after she had been ducked awhile, she lay with very little motion for a considerable space of time. At an hour and a quarter she often put out her black tongue, at near four hours she appeared much alive, and, as I remember, about that time also put out her tongue, swimming all this while, as far as we observed, above the bottom of the water. At the end of about seven hours or more, she seemed yet to have some life in her, her posture being manifestly changed in the glass from what it was awhile before; unless that might proceed from some difference made in her body, as to gravity and levity. Not long after she appeared quite dead, her head and tail hanging down movelessly, and directly towards the bottom of the vessel, whilst the middle of the body floated as much as the above-mentioned cork would permit it.

HASTE maketh me pretermit the mention of divers things suggested by what hath been delivered upon the present title. But this one thing would be taken notice of, that though some of the above-mentioned animals seem, by the relations we have given of them, to have been a little sooner destroyed by drowning, than any we have mentioned were by our engine, that is no sure proof that suffocation does kill animals faster than the deprivation of air they are exposed to in our engine. For in drowning, that which destroys is applied to its full vigour at the first, and all at once; whereas our receivers being made for several purposes, the deprivation of the air that they make cannot be made all at once, but the air must be pumped out by degrees; so that till the last the receiver will be but partly emptied. For confirmation of which, I have this to alledge, that, having in the presence of some virtuosi provided for the nonce a very small receiver, wherein yet a mouse could live some time, if the air were left in it, we were able to evacuate it at one suck, and by that advantage we were enabled, to the wonder of the beholders, to kill the animal in less than half a minute.



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E X P E R I M E N T S  
C O N C E R N I N G  
R E S P I R A T I O N

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A P R E F A C E concerning these E X P E R I M E N T S.

**T**HOUGH, to shun prolixity, the preface which the author had made to all he wrote about respiration, have been purposely omitted; yet there are some few points so necessary to be taken notice of, that it is thought unfit to leave them wholly untouched; for the following experiments being not at first written for the press, and thrown by for many years, till they were very hastily gathered together, and in some places supplied with others, little less hastily annexed, to make some necessary supplies, the reader must not expect in such a casual tract (which the author confesses to be one of the most imperfect and immethodical of all his compositions) any thing but novelty and truth, and an earnest desire to be serviceable in an inquiry so important to mankind, to the curious in general, and especially to physicians, who, by the encouraging mention they have made of his former endeavours in this kind, have invited him to add these many new experiments to those few, they had hitherto exercised their wits upon; and to leave them the more freedom to do so, he purposely forbore to confirm or confute any hypothesis, or so much as propose any of his own; declaring it to be his aim, not to espouse or make a party, but to commu-

nicate to the curious some matters of fact that are new, and in an historical way impartially delivered. No more of preface is now to be added, but that it is thought fit, for prevention of ambiguity, to give this advertisement, touching the ground of the title of *Vacuum Boylianum*, to be met with in these experiments; that as learned men, both English and foreigners, in their writings, have familiarly, for distinction-sake, employed the titles of *Machina Boyliana* and *Experimenta Boyliana*; so the author that writ these, for the most part in haste, and for his own memory, did for dispatch-sake, call the absence of the air procured in his receivers, our vacuum; whence by analogy was framed the *Vacuum Boylianum*, which he therefore thinks the less improper, because, to call it vacuum absolutely, would be judged by many a declaring himself a vacuist, who does not yet own the being either of their opinion, or a downright plenist; or else he must be troublesome to the reader and himself, by frequently explaining what sort of vacuum he understands; whereas he declares once for all, that by the *Vacuum Boylianum*, he means such a vacuity or absence of common air, as is wont to be effected or produced in the operations of the *Machina Boyliana*.

## T I T L E X I.

*Of the accidents that happened to animals in air, brought to a considerable degree, but not near the utmost one, of rarefaction.*

IN the generality of our pneumatical experiments upon animals, it suited with our purposes, to rarefy the air as much, and for the most part as fast as we could; but I had other trials in design, wherein an extraordinary degree of rarefaction, but yet not near the highest, to which the air might be brought by our engine, seemed likeliest to conduce to my inquiries, and particularly seemed hopeful to afford some light, in reference to those diseases and distempers that are thought primarily to affect the respiratory organs, or to depend upon something amiss in respiration.

WHEREFORE, having gages, by the help of which such experiments might be much better performed than else they could, I attempted several of them; some of whose successes I find in the following memorials.

## E X P E R I M E N T I.

Aug. 16. A LINNET being put into a receiver, capable to hold about four half pints of water, the glass was well closed with cement and a cover, but none of the air was drawn out with the engine or otherwise. And though no new air was let in, nor any change made in the imprisoned air, yet the bird continued there three hours without any apparent approach to death; and though it seemed somewhat sick, yet being afterwards taken out, it recovered, and lived several hours.

## E X P E R I M E N T II.

Aug. 18. FROM the above-mentioned receiver about half the air was drawn out, a linnet being then in the glass, and in that rarefied air (which appeared by a gage to continue in that state) the bird lived an hour and near a quarter before it seemed in danger of death;  
after



after which, the air being let in without taking off the receiver, she manifestly recovered, and leaped against the side of the glass; being taken out into the open air, she flew out of my hand to a pretty distance.

### EXPERIMENT III.

WE conveyed into a receiver, capable to hold four half pints of water, a lark, toge-Sept. 9. ther with the gage, by the help whereof we pumped out of the receiver three quarters of the air that was in it before; then heedfully observing the bird, we perceived it to pant very much, so that a learned physician (from whom I yet dissented) judged those beatings to be convulsive; having continued thus for a little above a minute and a half, the bird fell into a true convulsive motion that cast it upon the back. And although we made great haste to let in the air; yet, before the expiration of the second minute, and consequently, in less than half a minute from the time immediately preceding the convulsion, the lark was gone past all recovery, though divers means were used to effect it.

### EXPERIMENT IV.

PRESENTLY after we put into the same receiver a green-finch, and having withdrawn Sept. 9. the air until it appeared by the gage there remained but half, we presently began to observe the bird, and took notice, that, within a minute after, she appeared to be very sick, and shaking her head, threw against the inside of the glass a certain substance, which I took to be vomit, and which afterwards appeared so; upon this evacuation the bird seemed to recover and continue pretty well (but not without panting) until about the end of the fourth minute, at which growing very sick, she vomited again (shaking her head as at first) but much more unquestionable than before, and soon after, eat up again a little of her vomit; at which time (whether that contributed to her recovery or no) she very much recovered. And though she had in all three fits of vomiting, yet for the last seven or eight minutes that we kept her in the receiver, she seemed to be much more lively than was expected, which may in part be attributed to a little air that by accident got in, though it were immediately pumped out again. At the end of a full quarter of an hour from the first exhaustion of the receiver, the bird appearing not likely to die in a great while, and the engine being needed for other uses, we took out the bird, and thereby put a period to the experiment.

### EXPERIMENT V.

I now thought it fit to try, whether, though a viper would not hold out very many hours in air, brought to as high a rarefaction, as we could bring it by our engine, yet to that cold and vivacious animal, a very small proportion of air, in comparison of what was necessary to hot animals; would not suffice to keep it alive for a considerable time; the narration of the experiment I find registered as follows:

A VIPER lately bought of the person, that at this season uses to take new ones almost April 12. from day to day, was included, together with a gage, in a portable receiver capable to hold about three pints and half of water. This vessel being exhausted and secured against the regrefs of the air, the imprisoned animal was observed from time to time; and observed

## *Pneumatical Experiments about Respiration.*

served not only to be alive, but nimbly to put out and to draw back its tongue about 36 hours after it was shut up; for which reason we continued the vessel longer in the same shady place, where, at the end of 60 hours, looking upon her, as I was going to bed, she appeared very dull and faint, and not likely to live much longer; and the next morning, being by some occasions carried abroad, and coming to look upon the glass presently after dinner, I found her stark dead, with her mouth opened to a strange wideness; wherefore suffering water to be impelled by the outward air into the cavity of the receiver, to observe how far that vessel was then emptied of air, we found by the water that was driven in and afterwards poured out again, and measured, that 4 parts of 5, or rather 5 of 6 of the vesselled air (if I may so call that which was shut up in the receiver) had been pumped out; so that in an air so rarefied as to expand itself to 5 or 6 times its former and usual dimensions, our viper was able to live 60 hours that we are sure of, and perhaps might a pretty while longer.

### *A digressive experiment concerning respiration upon very high mountains.*

To illustrate what I have taken notice of in the printed experiments about the unfitness for respiration, observed by the learned *Acosta* in the high mountains of *Pariacaca*, I shall here add, what I have had the curiosity and occasion to learn from divers travellers, whom I purposely consulted about these matters; whereof you will easily believe, that not many of them have had opportunity to give accounts. Meeting with an ecclesiastical person that had visited those high mountains of *Armenia* (on one of which, because of their height, the tradition of the natives will needs have the ark to have rested) I asked him, whether those mountains are as really so high as is given out, and whether at the top of that he visited, he found any difficulty of breathing. To the first part of which question he answered, that they were really exceeding high (which he might well judge of, having been upon some of the most famous both in *Europe*, *Asia*, and *Africa*) and that he could not come to the top because of the unpassable snows; and to the second part he replied, that whilst he was in the upper part of the mountain, he plainly perceived that he was reduced to fetch his breath much oftener than he was wont, and than he did before he ascended the hill, and after he came down from it. And upon my inquiring whether or no that difficulty of breathing might not be accidental or peculiar to him, he told me, that he himself having expressed some wonder to find himself so short-winded, the people told him, that it was no more than happened to them, when they were so high above the plain; it being a common observation among them. And I was the more inclined both to make inquiry about these matters, and to believe what he said, because what he related of their being covered with snow, and of an odd temperature of air, I had learned before from a traveller of another nation than this person, and a stranger to him.

THE same churchman being asked by me, whether he had not in some part of *Europe* made the like observation (of the difficulty of breathing) told me, that he had done it upon the top of a mountain in the country of *Cevennes*, in or near the province of *Languedoc*; which may serve to confirm what I am about to relate from the mouth of a learned traveller that was upon the top of one of the Pyreneans, that is not very remote from the mountains we speak of.

THIS gentleman, who was a person curious and intelligent, being brother-in-law to one of the chief lords of those parts, was, by him, invited, about the beginning of September, to visit a neighbouring mountain that is at least one of the highest of the Pyreneans, which



which is commonly called *Pic de Midi*, upon whose top, where a tent was spread for them, they staid many hours. His answers to the other questions I asked him are elsewhere related: all that concerns this place being, that I find this set down among my adversaria, viz. I also inquired of him, whether they found the air at the top as fit for respiration as common air, which he told me they did not, but were fain to breathe shorter and oftener than usual; and because I suspected that might come from their motion, I asked, whether they observed it to cease, when they came down to the bottom of the hill, which he told me they plainly did; besides that they staid many hours at the top, too long to continue out of breath.

BUT that I may not here conceal any thing that may conduce to the discovery of the truth in the matter under consideration, I shall here add, that I did sometimes think it worth further inquiry, whether the sickness, if not also the difficulty of breathing, that some have been obnoxious to, in the uppermost parts of *Pariacaca*, and perhaps some other high mountains, may not be imputed, not so precisely to the thinness and rarity of the air, in places so remote from the lowermost part of the atmosphere, as to include certain steams of a peculiar nature, which, in some places, the air may be imbued with? In favour of which suspicion, I remember, that inquiring once of an intelligent man who had lived several years in the Island of *Teneriff*, whether he had been at the top of the *Pic* of that name, and what he had there taken notice of about the air? he answered me, that he had attempted to go up to the top of the mountain, but that, though some of the company were able to do so, he and some others, before they had reached so high, grew so sick upon the operation they felt of the sharp air, and sulphurous exhalations which infected it, that they were fain to stay behind their companions, he having already found this effect of those piercing steams upon his face, which, when he made me this relation, was of a fair complexion, that the skin began to be of a pale yellow, and even his hair to be discoloured.

## T I T L E XII.

*Of the observations produced in an animal, in changes as to rarity and density made in the self-same air.*

IN the experiments hitherto recited, the animals that were recovered from a gasping condition, have been so, by letting in fresh air upon them, and not the same that had been withdrawn from them. Wherefore I thought it very requisite to try whether the same portion of air, without being renewed, would, by being expanded much beyond its usual degree, and reduced to it, serve to bring an animal to death's door, and revive him again; since by the success of such a trial it would notably appear, that the bare change of the consistence of the air, as to rarity and density, may suffice to produce the above-mentioned effect.

BUT to devise a way to put this experiment in practice appeared no easy matter; since it required a receiver that should be transparent, and be capable of changing its bulk, without suffering any air to get in or out.

To surmount these difficulties, the first thing I thought on was, to take a fine limber and clear bladder of a sheep or hog, made more transparent by being anointed with oil, which was done on the outside, that the smell of it might less offend the animal to be included. Then we clipped off as much of the bladder at the neck as was judged absolutely necessary to make an orifice capable of letting in a mouse; that sort of animals being,

being, by reason of their smallness, the fittest of those furnished with lungs and hot blood we could procure. And whereas it seemed very difficult, when the neck of the bladder was cut off, to make up so large an orifice without wrinkles, at which the rarefied air may escape; to obviate this inconvenience, we provided a round stick somewhat less than the orifice, that, the wood being laid over with a close and yielding cement (for pitch, or the like common stuff will not always serve the turn) we might be able to tie the bladder fast and close enough upon the thus fitted stopple.

AND now to reduce these things to practice, and by their help make our designed experiment, we included a mouse into a receiver made according to this way, leaving in the bladder as much air as we thought might suffice him for a long time, as the experiment was to last. Then putting this limber or extensible receiver, if I may so call it, into an ordinary one of glass, and placing this engine near a window, that we may see through both of them; the air was by degrees pumped out of the external receiver (as for distinction sake I shall call it) and thereupon the air included in the bladder did proportionably expand itself, and so distend the external receiver, till being arrived at a degree of rarefaction, which rendered it unfit for the included mouse's respiration, I perceived, though with some difficulty, in this animal, the signs of his being in great danger of sudden death. Whereupon the outward air being hastily let into the external receiver, compressed the swelled bladder to its former dimensions, and thereby the included air to its former density, by which means the fainting mouse was quickly revived. Having given him some convenient time of respite, the experiment was reiterated with the like success, and we doubted not, but the third trial we made would have ended as the two former did; but that, whilst we were considering of the sickness of the mouse, which, by reason of some opacity that could scarce be avoided in the wrinkled bladder, was not as to its degree so easily taken notice of, it grew irrecoverable by the subsequent condensation of the air.

N. B. The confirmation of this by further experiments will properly fall under another title.

### T I T L E XIII.

*Of an unsuccessful attempt to prevent the necessity of respiration by the production or growth of animals in our vacuum.*

HAVING had frequent occasions to observe how quickly those animals, whose blood is actually warm, did expire in our vacuum; and that even those animals, with lungs, whose blood was actually cold, were not able to live any considerable time there; I thought it very well worth while, and yet extremely difficult, to try, whether there might not be some ways yet unpractised, either to make such animals as nature endows with lungs, live without respiration, or at least, to bring such insects, and other animals, as can already live without air, to move also without it in our vacuum.

THEREFORE considering with myself what happens to infants and other young animals in the womb, and even after they come from thence, if they continue to be wrapped up in the secundines; though as soon as they are brought into the free air, they may be presently killed by being kept from breathing; considering also, what I elsewhere relate of the slow expiration of a very young kitling in our vacuum, together with the long want of respiration, which custom enables some divers to endure; considering these things, I say, though I know that somewhat may be objected to shew, that these instances



stances are not altogether full to my purpose; yet they, among other things, invited me to think that the least unlikely projects that occurred to my barren invention, would be these that follow.

FIRST, I thought fit to try whether the seeds of respiring animals might be either hatched or otherwise brought to produce young ones, in our vacuum. For, if that could be compassed, I should obtain my end.

NEXT, in case of my failing in the former attempt, and that, which is to be after a few lines proposed, I thought fit to try, whether, at least, I could not bring the eggs of insects to hatch or be animated; or aurelias, as they call them, that were already alive, turn, according to the course of nature, into winged insects, as flies, or butter-flies; of which trials, and those of the former sort, the account properly belongs to another place, where I relate the success of these and other attempts to produce plants and animals in our vacuum.

BUT thirdly, considering that nature has so ordered it, that frogs, though when they are grown big enough to deserve that name, they be amphibious animals endowed with lungs; yet before they attain to that pitch, they live wholly in the water like fishes; I thought it the most expeditious and least improbable attempt we could make, to try whether or no this animal, being as a fish brought to live, either in our vacuum, or at least in highly rarefied air, would not continue to do so after its lungs should be perfectly formed. Wherefore, though I foresaw, and foretold the difficulty that would be met with in the prosecution of this experiment, namely, that the ærial bubbles that would be disclosed in such soft bodies upon the withdrawing of the pressure of the ambient air, would so violate the slight texture of those tender animals, as to hinder them from living long, or moving freely; yet I thought it very fit to attempt the trial, whereof I find this account among my adversaria.

## EXPERIMENT I.

WE took a good company of tadpoles, and put them with a convenient quantity of water into a portable receiver of a round figure, and observed, that at the first exsuction of the air they did rise to the top of the water, though most of them subsided again, till the next exsuction raised them. They seemed by their active and wrigling motion to be very discomposed. The receiver being exhausted, they continued restless, moving all of them in the top of the water; and though some of them seemed to endeavour to go to the bottom, and dived some part of the way, especially with their heads, yet they were immediately buoyed up again. Within an hour or little more, they were all moveless, and lay floating on the water; wherefore I opened the receiver, upon which the air rushed in, and almost all of them (which were many) presently sunk to the bottom, but none of them recovered to life.

## EXPERIMENT II.

A LITTLE after these, we included a lesser number of tadpoles in a smaller glass, which was also exhausted with the like circumstances with the former. And when I found the other tadpoles to be dead, I halted to these, which did not, except perhaps one, give any sign of life, but upon letting in the air, these having not been long kept

from it, some few of them did recover, and swam up and down lively enough for some time; though after a while they also died.

### EXPERIMENT III.

SOME years after I repeated the same experiment in a portable receiver of a convenient kind; and though after the exhaustion was perfected, the tadpoles did for a while move briskly enough on the top of the water, none of them appearing able to dive or swim under water, yet coming to look on them at the end of an hour, they seemed to be all of them quite dead, yet continued floating. And though within half an hour after that, I let in the air upon them, yet all the effect of it was, that the most of them immediately sunk to the bottom, as the rest of them did a very little while after; none of them, that I could observe, recovering any vital motion.

### EXPERIMENT IV.

THERE remains an experiment, which I often judged as well more hopeful as more noble, if I could procure an opportunity to bring my design to a trial, which I have found it very difficult to do; nevertheless I was able to do it once, though not fully as I desired, yet not altogether without success.

WE procured then, and with much ado, some of those odd insects, which I elsewhere describe, whereof gnats have by some ingenious men been observed to be generated about the end of August or beginning of September. These, for some weeks, live altogether in the water, as tadpoles do, swimming up and down therein till they are ripe for a transmigration into flies; which itself is so great a rarity in nature, as makes these little creatures recompence to our curiosity the trouble they often give our faces and hands. Supposing then, that if I could get some of these, and include them, being of those insects they call *aquatilia*, and so minute as they are, they may live a great while in the receiver without air, and in the mean while attain the period, which, according to nature's course, is wont to turn them into flies, which might come forth winged creatures into a medium not furnished with common air, as others of their kind enjoy; supposing, I say, that these insects would afford me some information about these particulars, having upon much watching met with four or five of them after a shower of rain, that dropped from a house into a vessel laid on purpose for it, we included them with some of their water into a small glass receiver, which being very exactly closed, we kept in a south-window, where these little creatures continued to swim up and down for some few days, without seeming to be much incommodated by so unusual an habitation; and at the end of that time, and much about the same day, they divested the habit they had, whilst they lived as fishes, and appeared with their exuviae or cast coats under their feet, and shewing themselves to be perfect gnats, that stood without sinking upon the surface of the water, and discovered themselves to be alive by their motion, when they were excited to it; but I could not perceive them to fly in that thin medium; to which inability whether the viscosity of the water might contribute, I know not, though they lived a pretty while, till hunger or cold destroyed them. Something in this experiment may deserve serious reflections; which I cannot spare time to offer at.



*A digressive experiment, concerning the expansion of blood and other animal juices.*

For some purposes, relating partly to respiration, and partly to other inquiries, I thought fit to endeavour to obtain what information could be procured, of the consistence and disposition to expand itself of blood and other animal liquors; in pursuance of which the ensuing trials, among others, were undertaken.

THE warm blood of a lamb or a sheep, being taken as it was hastily brought from the butcher's, where the fibres had been broken, to hinder the coagulation, was in a wide-mouthed glass put into a receiver, made ready for it; and the pump being early set on work, the air was diligently drawn out; but the operation was not always, especially at first, so early manifest, as the spirituousness of the liquor made some expect; yet this hindered not, but, after a long expectation, the more subtle parts of the blood would begin to force their way through the more clammy ones, and seem to boil in large clusters, some as big as great beans or nutmegs; and sometimes, to the wonder of the by-standers, the blood was so volatile, and the expansion so vehement, that it boiled over the containing glass; of which, when it was put in, it did not, by our estimate, fill above a quarter. Having also included some milk warm from the cow, in a cylindrical vessel of about four or five inches high, though the operator were induced to pump a great while before any intumescence appeared in the milk, yet afterwards, when the external air was fully withdrawn, the white liquor began to boil in a way that was not so easy to describe, as pleasant to behold; and this it did for a pretty while with so much impetuosity, that it threw up several parts of itself out of the wide-mouthed glass that contained it (and could have contained as much more) though there were not above two or three ounces of the liquor.

A YET greater disposition to intumescence, we thought, we observed in the gall, which was but suitable to the viscosity of the texture.

NOTE, that the two foregoing experiments were made with an eye cast upon the enquiry that I thought might be made; whether, and how far the destructive operation of our engine upon the included animal might be imputed to this, that upon the withdrawing of the air, besides the removal of what the air's presence contributes to life, the little bubbles generated upon the absence of the air in the blood, juices, and soft parts of the body, may by their vast number, and their conspiring distension, variously strengthen in some places, and stretch in others, the vessels, especially the smaller ones, that convey the blood and nourishment; and so by choaking up some passages, and vitiating the figure of others, disturb or hinder the due circulation of the blood; not to mention the pains that such distensions may cause in some nerves and membranous parts, which, by irritating some of them into convulsions, may hasten the death of animals and destroy them sooner, by occasion of that irritation, than they would be destroyed by the bare absence or loss of what the air is necessary to supply them with. And to shew, how this production of bubbles reaches, even to very minute parts of the body, I shall add on this occasion, hoping, that I have not prevented myself or any other, what may seem somewhat strange, what I once observed in a viper, furiously tortured in our exhausted receiver, namely, that it had manifestly a conspicuous bubble moving to and fro in the waterish humour of one of its eyes.

*Another digressive experiment belonging to the same title.*

To shew, that not only the blood and liquors, but also the other soft parts, even in cold animals, have aërial particles latent in them; we took the livers and heart of an eel, as also the head and body of another fish of the same kind, cut asunder cross ways somewhat beneath the heart; and putting them into a receiver, upon the withdrawing of the air we perceived that the liver did manifestly swell every way, and that both the upper and lower parts did so likewise; and at the place where the division had been made, there came out in each portion of the fish divers bubbles, several of which seemed to come from the *medulla spinalis*, or the cavity of the back-bone, or the adjoining parts; and the external air being let in both the portions of the eel presently shrank, some of the skins seeming to be grown empty or flaccid in each of them.

## T I T L E XIV.

*Of the power of assuefaction to enable animals to hold out in air, by rarefaction made unfit for respiration.*

“THE power of assuefaction in other cases, made me think it very well worth trying what it would do in respiration; and the rather, because I presumed it might prove an experiment of good use, if we should discover, that by a gradual accustomance an animal may be brought to live, either in a much thinner air, or much longer in the same air, than at first he could. But in regard, that to make such a trial perspicuously enough, the opacity of the bladder made use of in the former title was like to be an impediment, I devised another way to obviate that inconvenience, which may, I hope, be competently understood by the heedful perusal of the following trials.”

## E X P E R I M E N T I.

We included in a round phial with a wide neck (the whole glass being capable of containing about 8 ounces of water) a young and small mouse, and then tied strongly upon the upper part of the glass's neck a fine thin bladder, out of which the air had been carefully expressed, and then conveyed this phantastical vessel into a middle-sized receiver, in which we also placed a mercurial gage (adjusted by our elsewhere mentioned standard) this done, the air was by degrees pumped out, until it appeared by the gage, that there remained but a fourth part in the external receiver (as for distinction sake I call it) whereupon the air in the internal receiver expanding itself, appeared to have blown the bladder almost half full, and the mouse seeming very ill at ease by his leaping, and otherwise endeavouring to pass out at the neck of his uneasy prison; we did, for fear the over thin air would dispatch him, let the air flow into the external receiver, whereby the bladder being compressed, and the air in the phial reduced to its former density, the little animal quickly recovered.



## EXPERIMENT II.

A WHILE after, without removing the bladder, the experiment was repeated, and the air, by the help of the gage was reduced to its former degree of rarefaction, and the mouse, after some fruitless endeavours to get out of the glass, was kept in that thin air for full 4 minutes; at the end of which he appeared so sick, that, to prevent his dying immediately, we removed the external, and took out the internal receiver; whereupon, though he recovered, yet it was not without much difficulty, being unable to stand any longer upon his feet, and for a great while after continued manifestly trembling.

## EXPERIMENT III.

BUT having suffered him to rest a reasonable space of time, presuming, that assuefaction had accustomed him to greater hardships; we conveyed him again into the external receiver, and having brought the air to the former degree of expansion, we were able to keep him there for a full quarter of an hour; though the external receiver did not at all considerably leak; as appeared, both by the mercurial gage, and by the continuing distension of the bladder. And it is worth noting, that, till near the latter end of the quarter of an hour, not only the animal did scarce at all appear distressed, remaining still very quiet; but, which is more, whereas when he was put in, the trembling formerly mentioned were yet upon him, and continued so for some time; yet afterwards, in spite of the expansion of the air he was then in, they left him early enough. And when the internal receiver was taken out, he did not only recover from his fainting fit sooner than before; but escaped those subsequent tremblings we have mentioned.

## EXPERIMENT IV.

ENCOURAGED by this success, after we allowed him some time to recollect his strength, we reconveyed him and the odd vessel, wherein he was included, into the former receiver, and pumped out the air, till the mercury in the gage was not only drawn down as low as formerly, but near half an inch lower, that there the air might be yet further expanded than hitherto it had been. And though this did at first seem to discompose our little beast; yet after awhile he grew very quiet, and continued so for a full quarter of an hour; when being desirous to try what operation a farther rarefaction of the air would have upon him, we caused three exsuctions more to be made by the pump, before we discovered him to be in manifest danger (at which time the bladder appeared much fuller than before) but then we were obliged to let the air into the outward receiver; whereupon the mouse was more speedily revived than one would have suspected.

AND these trials of the power of assuefaction seemed the more considerable, because the air, in which the mouse had all this while lived, had been clogged and infected with the excrementitious effluvia of his body; for it was the same all along, we having purposely forbore to take off the bladder, whose regular intumescencies and shrinkings sufficiently manifested, that the vessel, whereof it was a part, did not leak.

## P O S T S C R I P T.

“THOUGH the success of the recited experiments is very promising; yet a subsequent trial or two, whose particularities are slipped out of my memory, oblige me in point of candour, to declare, that for further satisfaction, the trials of the power of accustomance, in reference to air unfit for respiration, ought to be both reiterated, and to be made in differing sorts of animals.”

## T I T L E XV.

*Some experiments shewing, that air, become unfit for respiration, may retain its wonted pressure.*

## E X P E R I M E N T I.

WE took a mouse of an ordinary size, having (not without some difficulty) conveyed him into an oval glass, fitted with a somewhat long and considerably broad neck, which we had provided, that it might be wide enough to admit a mouse in spite of his struggling. We conveyed in after him a mercurial gage, in which we had diligently observed, and marked the station of the mercury, and which was so fastened to a wire, reaching to the bottom of the oval glass, that the gage, remaining in the neck, was not in danger to be broken by the motions of the mouse in the oval part; the upper part of the long neck of the glass was, notwithstanding the wideness of it, hermetically sealed by the help of a lamp and a pair of bellows, that we might be sure that the imprisoned animal should breathe no other air than that which filled the receiver at the time when it was nipped up. This done, the mouse was watched from time to time; and though by reason of the largeness of the vessel, in comparison of so small an animal, he seemed to me rather drooping, than very near death at the end of the second hour; yet coming to look upon him about half an hour after, he was judged by the spectators quite dead, notwithstanding our shaking of the vessel to rouse him up. This made me cast my eyes upon the gage, wherein I could not perceive any sensible change of the mercury's station. But being unwilling to give over the mouse, without trying what fresh air would do to recover him, I caused the sealed part of the glass to be broken off, and, notwithstanding that his continuing to appear dead increased the confidence of those that thought him so, I obtained after a while some faint tokens of life; though I am not sure that they would have continued in a vessel, where the air was so clogged and infected, if it had not been that fresh air was frequently blown in by a pair of bellows, whose nose was inserted into the neck of the glass. This fresh air seemed evidently, though but slowly, to revive the gasping animal, whom I would not nor could not conveniently take out of the glass, till he had gained strength enough to make use of its legs; after which, without breaking of the glass (which I was loath to lose, having then no other of the kind) we took him out, and found him quickly able to go up and down. After which service, and another trial we had with him, which belongs not to this place, we set him at liberty to shift for himself.



EXPERIMENT II.

SUCH an experiment as the former we made with like success upon a small bird included with a gage in a receiver holding about a quart of water. The bird in about half an hour appeared to be sick and drooping, and the faintness and difficulty of breathing increased for about two hours and a half after that, at which time the animal died, the gage being not sensibly altered, unless perhaps the mercury appeared to be impelled up a little short higher than it was when put in; which yet might well enough proceed from some accidental cause.

EXPERIMENT III.

To satisfy some curious persons, that it is not want of coldness, but something else in the included air that makes it destroy the birds that are pent up in it, and by the hot exhalations that steam from their bodies, may be supposed to overwarm it, we made the following experiment.

In a glass-phial, capacious enough to hold about three-quarts of water, we not only included, but for greater accuracy, hermetically sealed up a small bird, and found, that in a few minutes he began to be sick and pant; which symptoms I suffered to continue and increase against the mind of a learned by-stander (who thought the animal would not hold out so long) till they had lasted just half an hour; at which time, having provided a vessel of water with sal armoniack, newly put into it, to refrigerate it (according to the way I elsewhere published); and the liquor thus made exceeding cold, somewhat to the wonder of those that felt it; the phial, with the sick bird, was immersed in it, and kept there in that condition for six minutes; and yet it did not appear, in the judgment of the by-standers, that the great refrigeration, that must be this way procured to the imprisoned air, did sensibly revive or refresh the drooping animal, who manifestly continued to pant exceedingly as before, and, as some affirmed, more; so that this remedy proving ineffectual, the phial was removed out of the water, and the bird some time after did, as I foretold, make many strains to vomit (though she brought up little) followed by evacuations downward, before she quite expired, which she did within a minute or two of a just hour, after the beginning of her imprisonment.

If I had been able (which I was not) to procure more birds, I would willingly have prosecuted this experiment by several other, not unhopeful trials, which for want of subjects I was fain to leave only designed.

T I T L E XVI.

*Of the use of the air to elevate the steams of bodies.*

In the digression about respiration annexed to the 41st of our Physico-mechanical Experiments formerly published, it is proposed as one of the considerable uses of the air in respiration, that, being drawn into the lungs, it serves to carry off with it, when it is breathed out again, the recrementitious steams that are separated from the mass of blood in its passage through the lungs; from which fuliginous excrements, if the blood were not continually freed by the help of the air, after nature had been accustomed to that way

way of discharging them, their stay in the body might have very great and destructive operations on it.

For the illustration of this use of the air, I shall now subjoin the following experiment.

WE made by distillation a blood-red liquor, which chiefly consisted of such saline and spirituous particles as may be obtained from the mass of blood in human bodies; this liquor is of such a nature, that if a glass phial, about half filled with it, be kept well stopped, the red liquor will rest as quietly as any ordinary one, without sending up any smoke or visible exhalation; but if the phial be unstopped, so that the external air be permitted to come in and touch the surface of the liquor, within a quarter of a minute or less, there will, upon this contact, be elevated a copious white smoke, which will not only fill the upper part of the glass, but plentifully pass out into the open air, till the phial be again stopped.

My purpose in this tract to forbear sidings in controversies keeps me from taking notice of the speculations suggested by some of the phenomena of this liquor; which yet I thought I might lawfully mention, as far as I have done it, because it but adventures upon giving one of the uses rather of the air, than immediately of respiration itself; and is brought but to illustrate what I have not found denied by any, though considered by very few; namely, the office of the air to carry off in expiration the fuliginous steams of the lungs. For in our experiment we manifestly see, that the very contact of the air may give the corpuscles of moist bodies a peculiar volatility or facility to emerge in the form of steams. I know, there are some corrosive spirits, as in nitre and salt, simple, or compounded of them, that, when they are very strong, emit for a while manifest fumes, but the difference of those liquors, and their inferiority to our red spirit, in the capacity of smoking liquors, might easily enough be manifested, if it were judged proper in this place, where it may suffice to take notice of these two things. The one is, that when the phial has lain stopped and quiet a competent time, the upper half of it will appear destitute of fumes, of which the air, it seems, will imbibe, and constantly retain but a certain moderate quantity; which may give some light towards the reason, why the same air, which will be quite clogged with steams, will not long serve for respiration, which requires frequent supplies of fresh air. The other is, that if the unstopped phial were placed in our vacuum, it would not emit any visible steams at all, nor so much as to appear in the upper part of the glass itself that held the liquor; whereas, when the air was by degrees restored at the stop-cock, without moving the receiver itself, to avoid injuring its closeness, the returning air would presently raise the fumes, first into the vacant part of the phial, whence they would ascend into the capacity of the receiver; and likewise, when the air that was requisite to support them was pumped out, they also accompanied it, as their unpleasant smell evinced, and the red spirit, though it remained unstopped, emitted no more fumes till the new air was let in.

ONE may compare with this liquor another smoking one, mentioned in the 29th of the first published pneumatical experiments, where an experiment is related of it, that has something in common with this, and may so far serve to confirm what is now delivered, as this also has some things additional to that; besides that, that liquor being made with ingredients corrosive, and of a bad name among chemists themselves, the fumes that proceed from it may fright many from daring to meddle with it, whereas this our red spirit has been found potently medicinal for some distempers of the lungs, by a doctor of physick, whom I desired to try it. The other phenomena of this liquor I shall not stay to describe, as not belonging to this place; and the liquor itself, with very little variation, I have, in the History of Colours, communicated.

T I T L E



## T I T L E XVII.

*Of the long continuance of a slow-worm and a leach alive, in the vacuum made by our engine.*

IN the often cited digression about respiration, there is mention made of the great vacuousness of house-snails, as they call them, and how little operation the withdrawing of the air had upon them in comparison of what it is wont to have on other animals. I shall now add by way of confirmation, that I made trial upon ordinary white snails, without shells, whereof two of differing sizes (the biggest about an inch and a half, and the other about an inch in length) were included in a small portable receiver, which being carefully exhausted, and secured against the return of the air, was attentively considered by me, presently after it was removed from the engine; whereby it was easy to discern, that both the snails thrust out and retracted their horns (as they are commonly called) at pleasure, though their bodies had in the softer places pretty store of newly generated bubbles sticking to them; but though they did not lose their motion near so soon, as other animals were in our vacuum wont to do; yet coming to look on them after some hours, they appeared moveless and very tumid, and at the end of twelve hours, the inward parts of their bodies seemed to be almost vanished, and they seemed to be but a couple of small full-blown bladders; and on the letting in of the air they immediately so shrunk, as if the bladders having been pricked, the receding air had left behind it nothing but skins; nor did either of the snails afterwards, though kept many hours, give any signs of life.

UPON a supposition, that the cold and clammy constitution of snails might be a main cause of their being able to endure the absence of the air so well, I thought it worth trial, whether eels and leaches might not yet be more able to continue in our vacuum than a snail; and accordingly some experiments were made pursuant to that curiosity; the most fully registered whereof are these that follow.

## E X P E R I M E N T I.

WE included in a receiver, whose globular part was about the bigness of a large orange, one of that sort of animals, that they vulgarly call eels: having withdrawn, but not solicitously, the air, and secured the vessel against the unpermitted return of it, we kept him there about eight-and-forty hours, during all which time he continued alive, but appeared somewhat swelled in his belly; his under-chap moving the very first night, but not the day and night following. By opening the receiver at length under water, we perceived, that about half the air had been drawn out. As soon as the water was impelled into the glass, the animal, that was before dull and torpid, seemed, by very nimble and extravagant motions, to be strangely revived.

## E X P E R I M E N T II.

WE took a leach that was of a moderate bigness, or somewhat short of it; and having included it, together with some water, in a portable receiver that was guessed to be capable of holding about 10 or 12 ounces of that liquor, the air was pumped out after the usual

manner, and the receiver being removed to a lightsome place, we observed, as we expected, that the leach keeping himself under water, there emerged, from divers parts of her body, store of bubbles, some of them in a dispersed way, but others in rows or files, if I may so speak, that seemed to come from determinate points. Though this production of bubbles lasted a pretty while, yet the leach did not seem to be very much discomposed by her present condition. This done, we disposed of the receiver, which was well secured from the ingress of the outward air, into a quiet place, where we daily visited it once at least, or oftener, as there was occasion; and found the leach somewhat fastened by her tail to that part of the glass that was under water, and sometimes wandering about that part which was quite above water; and still, when we endeavoured to excite her, she quickly manifested herself to be alive; and indeed (which will be thought strange) appeared so lively after the full expiration of five natural days, that expecting something might have happened to the receiver, and thereupon resolving to try how stanch it had continued, I opened it under water, by which means the outward air impelled in so much of that liquor, that I was satisfied, the receiver was immediately before as well exhausted, as others are wont to be in our pneumatical experiments.

## T I T L E XVIII.

*Of what happened to some creeping insects in our vacuum.*

NOTWITHSTANDING the great variety of reptiles, that nature does almost every where produce; yet the inconvenient time and place, wherein the following trials were made, supplied me with so few, that about those animals I find among my adversaria, no more than the ensuing notes.

## E X P E R I M E N T I.

WE took five or six caterpillars of the same sort; but I could not tell, to what ultimate species the writers about insects referred them. These being put into a separable receiver of a moderate size, had the air drawn away from them, and carefully kept from returning. But notwithstanding this deprivation of air, I found them about an hour after moving to and fro in the receiver; and even above two hours after that, I could, by shaking the vessel, excite in them some motions that I did not suspect to be convulsive: but looking upon them again some time before I was to go to bed (which may be was about 10 hours after they were first included) they seemed to be quite dead; and though the air were forthwith restored to them, they continued to appear so until I went to bed; yet, for reasons elsewhere expressed, I thought fit to try, whether time might not at length recover them, and leaving them all night in the receiver, I found the next day, that three, if not four of them, were perfectly alive.

## E X P E R I M E N T II.

WE took from an hedge a branch that had a large cobweb of caterpillars in it; and having divided it into two parts, we put them into like receivers, and in one of them  
shut



shut up the caterpillars together with the air, which from the other was exhausted. The event was, that in that, which had the air, the little and difficultly visible insects, after a small time, appeared to move up and down as before, and so continued to do for a day or two; after which, other occasions made the experiment to be neglected; whereas that glass, whence the air had been drawn out, and continued kept out, shewed after a very little while no motion, that we could perceive. But to try, whether caterpillars may continue so far alive in our vacuum all the winter, as the next spring or summer, to proceed in the transmigration to a butterfly, is a trial, that we have but begun, and therefore must not pretend to say any thing about its event.

## T I T L E XIX.

*Of the phenomena suggested by winged insects in our vacuum.*

WHEN our Physico-mechanical Experiments were dispatched to the press, the inconvenient season of the year, and the difficulty of making the receivers I then employed to keep out the air for any long time, hindered me from then publishing above a trial or two of what will happen to winged insects in our vacuum. But afterwards being provided of more commodious vessels, I thought fit at several times to repair that omission by various attempts, whereof the chief ensue.

## E X P E R I M E N T I.

THERE were taken four middle-sized flesh-flies, which having their heads cut off, were inclosed in a portable receiver, furnished with a pretty large pipe, and a bubble at the end. As soon as the receivers were exhausted, those flies lost their motion, which was not brisk before; an hour or two after, I approached them to the fire, which restored not their motion to them (but as to one of them, I suspect it had a languid motion for a while) wherefore I let in the air upon them, after which, in a very short time (though not immediately) they began one after another to move their legs, and one or two of them to walk; and having kept them all night in a warm place, when I sent one the next morning to try, if they would manifest any motion, he told me, that for a while they did, though when I afterwards rose myself, I could not perceive any motion in them.

Nov. 12.  
about 8 a  
clock at  
night,

## E X P E R I M E N T II.

ABOUT noon we closed up divers ordinary flies, and a bee or wasp; all which, when the air was fully withdrawn, lay as dead, save that for a few minutes some of them had convulsive motions in their legs. They continued in this state 48 hours, after which, the air was let in upon them, and that not producing any signs of life in them, they were laid in the meridian sun, but not any of them seemed in any degree to recover.

Sept. 11.

## E X P E R I M E N T III.

Dec. 11. WE put a great flesh-fly into a very small portable receiver, where, at first, it appeared to be very brisk and lively, but as soon as the air was drawn out, fell on his back, and seemed to have convulsive motions in her feet and proboscis; from whence she presently recovered upon the letting in of the air, which being drawn out again, she lay as dead; but a while after (within a quarter or half an hour) I perceived, that upon shaking the receiver, she stirred up and down, but faintly. This was done pretty late yesternight, since when I had not occasion to look on the glass, till this night after supper, when I found the fly not (whilst I staid to endeavour it) to be recovered either by warmth, or letting in the air. A while after this note was written, this fly recovered; and being next morning sealed up again in that glass, and kept 48 hours, though over the chimney, died for good and all.

## E X P E R I M E N T IV.

WE took a large grasshopper, whose body, besides the horns and limbs, was about an inch in length, and of a great thickness in proportion to that length; this we conveyed into a portable receiver of an oval form, and capable of holding, by our guess, about a pint of water and more; and having afterwards pumped out the air, till by the gage it appeared to have been pretty well drawn out, we took care, no air should re-enter to disturb the experiment. The success whereof was this; first, though, before the exhaustion of the air was begun, the grasshopper was stirring, and lively, and continued so for a while after the beginning of the operation; yet, when the air began to be considerably rarefied, he appeared to be very ill at ease, and seemed to sweat out of the abdomen many little drops of liquor, which being united, trickled down the glass like a little stream, which made at the bottom a small pool of clear liquor, amounting to near a quarter of a spoonful, and by that time the receiver was ready to be taken off, the grasshopper was fallen upon his back and lay as dead. Secondly, though having a little after laid the glass in a south window, on which the sun then shone, I perceived some slow motions in the thorax, as if he strained to fetch breath; yet I was not sure they were not convulsive motions, and whatever they were, they lasted but a while, and then the animal appeared to be quite dead, and to continue so for three hours from the removal of the receiver. Thirdly, that time being expired, the glass was opened, and the air let in upon him; notwithstanding which there appeared no sign at all of life; but imagining there might be some time requisite to recover him out of so deep a swoon, I let the glass rest in a convenient posture, that the water that came from him might not endanger him, for a quarter or half an hour; and though I then perceived no signs of life, yet being desirous to pursue the trial yet further, I caused him to be carried into a sun-shiny place, where the beams of a declining sun presently began to make him stir his limbs, and, in a short time, brought him perfectly to life again.

## E X P E R I M E N T V.

April 15. WE took one of those shining beetles they call rose-flies, and having included it in a very small round receiver, which we exhausted; and though he that attended the engine, affirmed,



affirmed, it struggled much whilst the air was withdrawing, yet presently after I could perceive but little motion, and part of that seemed almost convulsive; and afterward going abroad, and not returning to look on the glass till about six hours after, the fly seemed quite dead, and discovered not any motion upon that of the glass. And within about an hour after, though I let the air rush in, yet no sign of life ensued, neither immediately, nor for a pretty while after. So that suspecting the fly to be really dead, and yet not resolutely concluding it, though I would then wait no longer, yet three or four hours after (viz. about 10 of the clock at night) I returned to the receiver, and found the beetle lively enough. Whereupon I caused the glass to be again exhausted, and secured from the ingress of the air, during which time the animal seemed to be much disquieted by what was done to it, but did not lose its motion before I went to bed, which was soon after.

## EXPERIMENT VI.

ABOUT butterflies, I remember, I made several trials, most of which chanced to be lost; but thus much I very well remember, that having observed them not only to live, but to move longer than was expected, I chose to include divers of them in receivers somewhat large, especially that I might see, whether in so thin a medium some or other of them, by the help of their large wings, would be able to fly. But though, whilst the air continued in the glasses, they flew actively, as well as freely, up and down; and though after the exhaustion of the air, they continued to live, and were not moveless; nay, though at the bottom of the receiver they would even move their wings, and a little flutter, yet I could not perceive any of them to fly, by which I mean, perform any progressive motion, supported by the medium only. And by frequently inverting the receiver (which I took care should be pretty long, to let them fall from one extreme to the other) they would fall like dead animals without displaying their wings, though just as they came to touch the bottom, some of them would sometimes seem to make some use of them, but not enough to sustain themselves, or to keep their falls from being rude enough.

## TITLE XX.

*Of the necessity of air to the motion of such small creatures, as ants, and even mites themselves.*

“ IN the experiments hitherto mentioned, the animals, on which the trials have been made, were divers of them of a moderate bulk; and others of them, though small, yet not of the least sizes that nature afforded us. Wherefore I thought fit to annex the following experiments, wherein I designed to examine, whether even those minute sorts of animals, whose bulk is thought the most contemptible, have not, as well as the greater, need of the air, if not to make them live, yet at least to enable them to move.”

A PRETTY number of ants were included in a small portable receiver exhausted yesterday about noon: between six and seven in the afternoon, they seemed to be all quite dead, and the rather, because, though they were very lively just before they were sealed up, running briskly up and down the bubble they were in; yet they grew almost moveless as soon as the air was exhausted; and a little while after appeared more so; though

I then

I then suspected more than I since did, that they were much inconvenienced by some small glutinous substance that seemed to have got into the small receiver from the vapours of the cement. When I looked on them at the time lately mentioned, I opened the glass, whereupon the air rushed in; but no sign of life appeared for a great while in any of the ants; but looking upon them this morning about 9 a clock, I found many of them alive, and moving to and fro.

“It is said by naturalists upon the authority of *Aristotle*, that the animal, the Greeks call *ἀνὰπτι*, is the minuteſt of living creatures. But thoſe of this ſort being very hard, if at all to be met with here, I thought fit to make ſome experiments upon the leaſt of the terreſtrial animals I could procure, and try, whether or no mites themſelves, which are reputed but living points, and not to be taken notice of by the naked eye to be living, but by motions, which even an attentive one can ſcarce diſcover, ſtand in need of the air; eſpecially, becauſe, in caſe they do, it may ſuggeſt to us ſome odd reflections upon the ſtrange ſubtilty and minuteness of the aërial particles, which muſt be capable of flowing in, and paſſing out, at the inviſible and almoſt inimaginable ſmall pores, and other cavities of the parts of an animal, whoſe entire body is reputed but a phyſical point.”

We conveyed then a pretty number of mites, together with the mouldy cheeſe they were bred in to nourish them, into three or four portable receivers (which were all of them very ſmall) not much differing in ſize. From all of theſe, ſave one, we withdrew the air; and then, making uſe of our peculiar contrivance to hinder its return, we took them one after another from the engine, and laid them by, for further obſervation. That one, which I took notice that we had reſerved, and, in which, to obſerve the difference, we thought fit to leave the air, was ſealed at a lamp-furnace, after the uſual manner of nipping up glaſſes there. This done, there remained nothing but to obſerve the event of our trials, which afforded us the enſuing phænomena.

1. Thoſe mites that were incloſed in the ſmall glaſs, that never came near the engine, continued alive, and able to walk up and down for a full week after they had been put in; and poſſibly would have continued much longer, if the glaſs had not been accidentally broken.

2. As ſoon as ever one of the receivers was removed from the engine, I looked with great attention upon it, and though juſt before the withdrawing the air, the mites were ſeen to move up and down in it; yet, within a few minutes after, the receiver was applied to the engine, I could diſcern in them no life at all, nor was any perceived by ſome younger eyes than mine, whereunto I expoſed them. Nay, by the help of a double convex-glaſs (that was ſo ſet in a frame, as to ſerve me as a microſcope on ſuch occaſions) I was not able to ſee any of them ſtir up and down. Nor was any motion taken notice of in the other ſmall receiver of like bigneſs and ſhape with mine, by them that had exhauſted it of air. And my occaſions not permitting me to attend the obſervation any longer in the place where it was made, I took the receiver, I had ſo attentively conſidered myſelf, along with me in the coach; and having occaſion to make ſome ſtay, about an hour after I looked upon it attentively again, but could not perceive any of the mites to ſtir; and the like unſucceſſful obſervation I made, when I had a conveniency, two or three hours after that. And the place I did it in, being one, where I thought myſelf, as it were, at home, I firſt let in the air, to try if the mites were not quite dead; and though neither upon its ruſhing in, nor during my ſtay there, I could perceive any of them to ſtir; yet I left the receiver unſtopped, as it was in the window, upon a ſuſpicion, that the air might not be able to produce its operation upon them in a ſhort time.



3. AND therefore passing by the same place about two or three days after, I called in to look upon my receiver, and found a number of my little animals revived, as an attentive eye might easily perceive by the motion of certain little white specks, when it was helped to observe it by little marks I made on the outside of the glass (which was purposely chosen thin and clear) near this or that mite, with a diamond; by the approach to, or recels from which marks, the progressive motion became (perhaps within a minute) plainly discoverable; especially, if we used the following expedient (which I found the best of those I tried) namely, that when the eye perceived little white specks that looked like mites, the receiver should be so turned and returned, that the bellies and feet of those little creatures were uppermost; notwithstanding which, they would not easily drop down, but continue their motion; which specks being made upon the concave surface of the thin glass itself (to which you may approach your eye as much as you please) are thereby rendered much more easily visible. But, this being only intimated upon the by, I proceeded to take notice, that in the newly mentioned receiver, the mites did, by stirring up and down, continue to appear alive for two or three days after, if not longer. I should not, I confess, have thought it ridiculous to suspect, that the mites, which at first lost their motion, did at last really die; and that those I after saw stirring up and down, were others newly generated in the included mouldy cheese; but I was not apt to think this suspicion probable, not only because of the extreme difficulty of making any living creature to be generated in *Vacuo Boyleano*, but because it did not seem agreeable to what I elsewhere noted, about the way and time of the propagation of mites, whose eggs I have divers times observed with pleasure, that at a season of the year that was not favourable (for these things happened in a cold *March*) newly generated mites should in two or three days grow up to their just bigness, which several of those we observed, seemed to have attained.

4. BUT, because it doth not by the third phaenomena appear, whether or no, in case our mites had been kept in a moveless state for a much considerabler time than three or four hours, they would have been recoverable by the admission of the air; I shall add, to satisfy that doubt, that one of the portable receivers above-mentioned, being exhausted, and carefully secured from the regrefs of the air, was kept from Monday morning to Thursday morning: after all which time, our attentive eyes being unable to discover any signs of life among the included mites, the air was let in upon them, and after no long time, had such an operation upon them, that both I and others could plainly see them creep up and down in the glasses again.

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SOME  
CONSIDERATIONS  
TOUCHING THE  
USEFULNESSES  
OF EXPERIMENTAL  
NATURAL PHILOSOPHY.

Proposed in a familiar Discourse to a Friend, by way  
of Invitation to the Study of it.

The Second Tome, containing the latter Section of the Second  
Part.

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The PUBLISHER to the READER.

**W**HEREAS the preface of the noble author to this second tome of the usefulness of experimental philosophy, was written with design it should come forth a year or two before the last, it is fit, that something be now added about the present publication.

FIRST, if enquiry be made, why the essays, that now come abroad, are not accompanied with those others, that, according to the sorts of the titles, should precede some of them; he represents, that it was not thought fit, that those, that are now published, having no necessary dependence on the rest, and being sufficiently intelligible without them, should stay for discourses, that are not at present ready, and perhaps will not suddenly be so; partly, in regard they consist of no small number of loose papers, which  
by



by reason of some, yet insuperable, obstacles (of which want of health is none of the least) he cannot conveniently seek out, range, and complete; and partly, because he cannot, in the place where he is now detained, be master of divers uncommon minerals, and some chymical productions, whose descriptions through haste he omitted, because he had them at hand in the place, where those essays were written, and presumed, he could at leisure fill up those vacancies he left for such descriptions.

SECONDLY, as to the essays themselves, which, for the reasons just now mentioned, come not abroad with the rest, though the excellent author hath of late years constantly refused to promise any thing to the publick, yet, that the reader may the better judge of the scope and design of the whole treatise, he will not deny him an intimation of what subjects those essays relate unto, by telling him that one of them treateth of the usefulness of chymistry (not to physick, but) to the empire of man over the inferior works of nature: another, of the advantages, that a naturalist's country may derive from his curiosity: another, of the mutual assistance, that the speculative and practical part of physiology may afford each other: after which comes a discourse, containing inducements to hope for much greater things from experimental philosophy, than men have hitherto obtained.

LASTLY, as to what the author taketh notice of, about the coincidents of some experiments, that may be mentioned as well by others as by him; it is very possible, that the same things may, by the same, or other ways, come to the knowledge of different persons. Besides, that I have heard him mention with some complaint, that, when divers years since, he writ several discourses (whereof some belonged to the usefulness of experimental philosophy) for the use of a private friend, not for the press, he was not so shy, as had been requisite, of shewing divers experiments, and of imparting others in discourse, to inquisitive men, whether English or foreigners, that came to visit him; divers of which things he afterwards found in print, sometimes indeed with, but for the most part without, mention of his name. So, that sometimes his unwillingness to disoblige such writers, and to contend about such matters, made him either wholly omit some of the particulars he afterwards intended to publish, or even to cross out several passages that he had already written, where he would without much inconvenience (for, that did not always happen) either quite leave them out, or substitute others (though less proper) in their stead. He added also, that sometimes observing his notions and experiments to be ascribed to other writers, and somewhat wondering at it, he found indeed such writers to have mentioned such things, but in editions that came abroad after the publication of our author's writings; from whence such things might, with the greater likelihood be presumed to have been borrowed, both because some of the writers had conversed with him, and he could not find them in the first edition of such books. But these unfair proceedings being the faults but of a few, he said, he was far from imputing them to the generality of those, that have mentioned (which divers of those have very civilly done) his experiments, or writings in theirs.

THE particulars being thus taken notice of, the curious reader ought not to be any longer detained from conversing with the author himself in this instructive treatise. Farewell.

## The PREAMBLE.

**I** HAVE, in the preface, and body of the former, and already published part of this treatise, taken notice of so many of the things that concern the whole work in general, that I presume it will not here be necessary to detain the reader with any other particulars, than those, that will be offered by way of answers to some questions; that are like to be asked about the publication of this present tome.

AND in the first place, if it be demanded, why this latter part did not more closely follow the former, I have this to answer; that the papers it consisted of, chanced to be so unfortunately disposed of, during the late publick confusions, that for a great while I was not the master of them, and in the mean while was, sometimes upon one occasion, and sometimes upon another, engaged to venture abroad the History of Colours, the History of Cold (with the preliminary and additional tracts) Hydrostatical Paradoxes, and the Origin of Forms and Qualities; the publication of which treatises, besides that of some anonymous papers, as it took up much of the time I had to spare for the press; so it may, I suppose, keep it from being thought strange, that I did not trouble myself and others with this book also. And indeed, this having been (as the scope and divers passages of it sufficiently intimate) one of the first I wrote to the gentleman I call *Pyrophilus*, I had occasion, whilst it was out of the way, to make use of so many of the experiments and observations, that belonged to it, that fearing I had thereby too much robbed, and disfigured it, to leave it any way fit for publick view, I had the greater temptation to neglect the looking after it.

BUT if it be further demanded, why then, since it was not ready to come out more early, I did not condemn it not to come out at all? I have two things to return by way of answer.

THE first is, that some eminent virtuosi, to whom I owe a peculiar respect, were pleased to challenge the edition of this tome, as if I had made myself a debtor to the publick for the second part of this work, by having suffered what I wrote to a private friend to be divulged in the first. Especially since the publick had given that so very favourable an entertainment; as, besides other things, the early reprinting of it manifested.

THE other part of my answer, and that, which made the former consideration prevalent, is, that I was overcome, either by the reasons, or by the authority, of those ingenious persons, that were pleased to think, that this work would not prove unserviceable to mankind, to whose good, both as a man, and Christian, I have been long ambitious to contribute, as well upon the account of the great Author and divine Redeemer of men, as of that common nature, whereof all men partake. What the utilities of this work were conceived to be, the reader will find disclosed at the end of this preface. To which I will therefore refer him for an account of them; and now only take notice, that as to one of the scruples I had against the publication, namely, that I had plundered this present treatise of divers particulars, wherewith I had accommodated some of my other writings; I could not well reject this answer, that in so many years as had passed since the writings of this book, I had not been so negligent a commercer with the works of nature and art, as not to be able to make some amends for



for what I had taken away, and easily substitute other experiments and observations, to supply the vacancies left by those I had transferred to other discourses.

AND as to another of my scruples, about venturing abroad this tome, namely, that it must come forth so late, if it should come forth at all, it was answered, that it could scarce come forth more seasonably to recommend the whole design of the Royal Society, whose generous aims being to promote the knowledge of nature, and make it useful to human life. This treatise may procure them some number of assistants, in a work, whose vastness and difficulty will need very many, if men's curiosity and industry can by this treatise (or any to the like purpose) be well excited by a conviction of the real and wide disparity betwixt true natural philosophy, and that of the peripatetic schools; and that in cultivating the former, they will not meet with a field, that will afford them nothing, but (the wonted production of the latter) the thorns and thistles of acute indeed, but useless, and oftentimes troublesome, subtilties; but, that they may expect a soil, that may by a due culture be brought to afford them both curious flowers to gratify curiosity, and delight their senses, and excellent fruits, and other substantial productions, to answer the necessities, and furnish the accommodations of human life.

AND I will not deny, that I have had the fortune to be looked upon, as not the unfittest person in the world to offer something in this kind; for those, that are mere scholars, though never so learned and critical, are not wont to be acquainted enough with nature and trades, to be able to suggest those instances, that are the most proper to manifest that, which men are to be convinced of. The mere chymists, besides that their curiosity is wont to be too much confined to let them be fittest for such a work, have the ill fortune to be distrusted by the generality of men, not credulous; which is a great unhappiness in this case, because, that though their experiments were never so true (as divers of them are) yet skill in their art being requisite to make them, men's diffidence of the proposers, joined with the difficulty of examining the things, will not allow them, either to believe what is proposed, or to try it. And as for the new philosophers (as they call them) though, if they were to write but for philosophical readers, I know several of them, that would questionless do it rarely well; yet the generality of those readers, to whom we would give good impressions of the study of nature, being such as will probably be more wrought upon by the variety of examples, and easy experiments, than by the deepest notions, and the neatest hypotheses, such a treatise for the kind, as that which follows, containing many practices of artifices and other particulars, that are either of easy trial, or immediate use, may perhaps by that variety gratify, and persuade a greater number of differing sorts of readers, than a far more learned and elaborate piece, and might be welcomer to more intelligent and philosophical perusers.

If it be asked by some, that know me, whence it comes, that the second part of the usefulness of experimental philosophy being written (as very credible persons, that saw it can witness) about the year 1658, there may be met with in the following treatise some experiments of my own, that they know were since made, and some (though few) citations out of books published since that time? If, I say, this be asked, the answer is intimated a little above; for having transferred to other tracts many passages, that belonged to those I now publish, I was obliged to repair the injury I had done them, by supplying them with such particulars, as offered themselves to my memory, when I hastily reviewed this tome, without scrupulously minding the times when the particulars inserted did first occur. And if this advertisement be applied to some other of my writings, that either the importunity of friends, or some unwelcome accidents, engaged me to publish out of their due time, and not in their intended order; it may keep men from thinking, that when I first wrote them, I had read over, or at least seen

which indeed I neither did nor could) every book of a recenter date, of which upon occasion I mention a passage or two, and those perhaps, as they are cited by other authors, we being here in *England* but slenderly, and very slowly, furnished with modern foreign books.

ALL these inserted passages the reader should find included in parentheses, as the printers call these marks ( ), by which he will yet be able to distinguish several of them, though I now find, that some others, by the negligence of the transcribers, or of the prefs, or of both, have been omitted; which advertisement I fear may have need to be extended to some other printed tracts of mine, wherein parentheses are to be met with.

BATING these few additional passages, the ensuing book comes forth, without taking notice of what changes or discoveries have happened in the commonwealth of letters, since the time it was written in. On which account, if some few of those many particulars delivered there should chance to be co-incident with what some other man hath written, I would neither on the one side be thought a plagiarist myself, nor on the other side deny any man, to whom it may be due, the honour of the earliest publication; though, to shun needless controversies, I am somewhat shy of naming this or that person, as the first proposer or inventor of an experiment, which, especially if the persons or things be not considerable, is often difficult enough to discover: witness the contests, that have been, and yet continue, about the first inventors of common weather-glasses the ascension of water in slender pipes, the glass drops that fly in pieces, the measuring of time by a pendulum, and, which is more strange, the art of printing itself. If it be asked, why I did not forbear to make use of some practices of tradesmen, and other known, and perhaps seemingly trivial, experiments; these things may be replied.

I. THAT since on divers occasions it was requisite, that my discourse should tend rather to convince, than barely to inform my reader, it was proper, that I should employ at least some instances, whose truth was generally enough known, or easy to be known, by making enquiry among artificers, even by such as out of laziness, or want of skill, or accommodation, cannot conveniently make themselves the trials.

II. BUT yet I have taken care, that these should not be the only, nor yet the most numerous instances, I make use of; it being in this tome, as well as in my other philosophical writings, my main business, to take all just occasions, to contribute as much, as without indiscretion I can, to the history of nature and arts.

III. As to the practices and observations of tradesmen, the two considerations already alledged may both of them be extended to the giving of an account of the mention I make of them. Of the truth of divers of the experiments I alledge of theirs, one may be easily satisfied, by inquiring of artificers about it; and the particular, or more circumstantial accounts I give of some of their experiments, I was induced to set down by my desire to contribute toward an experimental history. For I have found by long and unwelcome experience, that very few tradesmen will, and can give a man a clear and full account of their own practices; partly out of envy, partly out of want of skill to deliver a relation intelligibly enough, and partly (to which I may add chiefly) because they omit generally, to express either at all, or at least clearly, some important circumstance, which because long use hath made very familiar to them, they presume also to be known to others: and yet the omission of such circumstances doth often render the accounts they give of such practices, so dark and so defective, that, if their experiments be any thing intricate or difficult (for if they be simple and easy, they are not so liable to produce mistakes) I seldom think myself sure of their truth, and that I sufficiently comprehend them;



them, till I have either tried them at home, or caused the artificers to make them in my presence.

THEY that have given themselves the trouble of endeavouring to make the experiments of tradesmen, to be met with in the writings of *Cardan*, *Weckar*, and *Baptista Porta*, for instance; and have thereby discovered (what is not usually obvious upon a transient reading) how lamely and darkly, not to add unintelligibly, several things are written, will probably afford me their assent, having found upon trial the instructions of such learned and ingenious men to be often obscure and insufficient for practice.

BUT here I must give the reader notice, that as mechanical arts for the most part advance from time to time towards perfection; so the practices of artificers may vary in differing times, as well as in differing places, as I have often had occasion to observe. And therefore I would neither have him condemn other writers or relators, for delivering accounts of the experiments of craftsmen differing from those I have given; nor condemn me, for having contented myself to set down such practices faithfully, as I learned them from the best artificers (especially those of *London*) I had opportunity to converse with.

BUT here perhaps it will be demanded by way of objection, whether I do not injure tradesmen, by discovering so plainly those things, which our laws call the mysteries of their arts? To a question, that may perhaps by some be clamorously pressed, not only upon me, but much more upon some ingenious men of our nation, whose pens have been more bold than mine in disclosing craftsmens secrets, it will be requisite to return several things by way of answer; but that such readers, as are not troubled with the scruple, may not be so with the apology, they will find this printed in another character, so that, if they please, they may pass it over unread.

[FIRST then, it may be represented, that I never divulge all the secrets and practices necessary to the exercise of any one trade, contenting myself to deliver here and there, upon occasion, some few particular experiments, that make for my present purpose: so that, for much more than I allow myself to do, I can plead the example, not only of other writers that have published books to teach the whole mystery of this, or that trade, as the priest *Antonio Neri* hath diligently done in his Italian *Arte Vetraria*, and some English, as well as foreign virtuosi, have done on other subjects; but also some of the artificers themselves, as the famous goldsmith and jeweller *Benvenuto Cellini* in his much esteemed Italian tracts of the lapidaries and goldsmiths trades. Thus also the famous mineralist *Georgius Agricola* published in Latin a whole volume of the more practical part of mineralogy, wherein he largely and particularly describes experiments, tools, and other things, that belong to the callings of mine men. To which I might add divers other treatises, some of them French, others Italian (which, though I could not procure them, I have seen among curious collections of books) that have been published about several arts by the artificers themselves. And it is notorious, that in English, as well as in divers foreign languages, we have books of the arts of gunnery, distillation, painting, gardening, &c. divulged by persons that professed those callings.

SECONDLY, it is not the custom of tradesmen to buy books, especially such as are not intended for such readers, and treat (for the most part) of things, either beyond their reach, or wherein they seem not likely to be concerned; and as for gentlemen and scholars, though some of them may, to satisfy their curiosity, make a few trials, yet their doing so will scarce in the least be prejudicial to tradesmen. Since, to omit other arguments, it will not be worth while for a virtuosi to be at the charge and trouble of buying

buying tools, and procuring other necessary accommodations to sell a few productions of his skill, though he should not scruple to descend to such a practice. For if he make but a small number of experiments, their effects will cost him more than the like may be bought for of those that make them in great quantities, and whom their trade obligeth to be solicitous to buy their instruments and materials at the best hand, and sell them to the best profit. Besides that most of the works of artificers are chiefly recommended to the more curious sort of buyers by a certain politeness, and other ornaments, comprized by many under the name of finishing; which require either an instructed and dexterous hand, or at least some little peculiar directions, which I did not always think myself obliged to mention, in a treatise designed to assist my friend to become a philosopher, not a tradesman, and published to help the reader to gain knowledge, not to get money.

THIRDLY, to publish an experiment or two, or in some cases, a much greater number belonging to a trade, is not sufficient to rob a tradesman of his profession. For besides that most trades consist of several parts, and are each of them made up of divers practices, (that commonly are more than a few) those numerous mechanical arts, that are called handicrafts, require (as their very name argueth) a manual dexterity, not to be learned from books, but to be obtained by imitation and use. And to these considerations I shall add this more important one, that mechanical professions are wont to be, as it were, made up of two parts, which, for distinction sake, I take leave to call the art and the craft; by the former whereof I mean the skill of making such or such things, which are the genuine productions of the art, (as when a taylor maketh a suit, or a cloak) and by the latter I mean the result of those informations and experiments, by which the artificer learns to make the utmost profit, that he can, of the productions of his art. And this oeconomical prudence is a thing very distinct from the art itself, and yet is often the most beneficial thing to the artificer, informing him how to choose his materials, and estimate their goodness and worth; in what places, and at what times, the best and cheapest are to be had; where, and when, and to what persons the things may be most profitably vended. In short, the craft is that, which teacheth him how both to buy his materials and tools, and to sell what he makes with them, to the most advantage.

FOURTHLY, it may often prove more advantageous than prejudicial to tradesmen themselves, that many of their practices should be known to experimental philosophers. This I suppose, that I have sufficiently proved in some, and especially in \* one of the following essays.

YET I shall now represent, that though some little inconvenience may happen to some tradesmen by the disclosing some of their experiments to practical naturalists, yet, that may be more than compensated, partly, by what may be contributed to the perfecting of such experiments themselves, and, partly by the diffused knowledge and sagacity of philosophers, and by those new inventions, which may probably be expected from such persons, especially if they be furnished with variety of hints from the practices already in use. For these inventions of ingenious heads do, when once grown into request, set many mechanical hands a-work, and supply tradesmen with new means of getting a livelihood, or even enriching themselves. As to the discipline subordinated to the pure mathematicks, this is very evident; for those speculative sciences have (though not immediately) produced their trades, that make quadrants, sectors, astrolabes, globes,

\* The essay here meant is that, which treats of the utility of the naturalists insight into trades.



maps, lutes, phials, organs, and other geometrical, astronomical, geographical, and musical instruments; and, not to instance those many trades that subsist by making such things as mechanicians, proceeding upon geometrical propositions, have been the authors of; we know, that whether the excellent *Galileo* was, or was not, the first finder out of telescopes, yet he improved them so much, and by his discoveries in the heavens did so recommend their usefulness to the curious, that many artificers in divers parts of *Europe* have thought fit to take up the trade of making prospective glasses. And since his death, several others have had profitable work laid out for them, by the newer directions of some English gentlemen, deeply skilled in dioptricks, and happy at mechanical contrivances; insomuch, that now we have several shops, that furnish not only our own virtuosi, but those of foreign countries, with excellent microscopes and telescopes, of which latter sort I lately bought one (but I confess the only one, that the maker of it, or any man, that I hear of, hath perfected of that bigness) which is of threecore foot in length, and which the ingenious artist that made it, *Mr. Reeves*, prized constantly at no less than an hundred pounds English money. I know not, whether or no I should add, that possibly some particular experiments of mine have not been hitherto unprofitable to several tradesmen. But this I may safely affirm, that a great deal of money hath been gained by tradesmen, both in *England* and elsewhere, upon the account of the scarlet dye, invented in our time by *Cornelius Drebel*, who was not bred a dyer, nor other tradesman. And, that we daily see the shops of clockmakers and watchmakers more and more furnished with those useful instruments, pendulum-clocks, as they are now called, which, but very few years ago, were brought into request by that most ingenious gentleman, who discovered the new planet about *Saturn*.]

I HAVE handled the subject of the foregoing arguments much more particularly, than I would have done, had not my pen been drawn on by a hope, that the things I have represented, may furnish apologies to many inquisitive men, who may be thereby emboldened to carry philosophical materials from the shops to the schools, and divulge the experiments of artificers, both to the improvement of trades themselves, and to the great enriching of the history of arts and nature.

IF it be further demanded, whether I have furnished these essays with the chiefest things I could have afforded them, I must confess, that I have not; for though I had, lying by me, several experiments and observations, less inconsiderable than many of those I have made use of, which would have been pertinent enough to the subjects here treated of; yet I purposely forebore to employ them in these tracts, because I would not defraud those others, to which they were more proper, and some of them necessary. For I freely declare, that my design in this present tome was not to furnish it as well as I could, but to preserve, as in a repository, several scattered experiments and remarks, which I could best spare from the other treatises I had designed, which might otherwise probably be lost: but yet I shall not deny, that I did not carelessly draw up some of the following tracts, but, that I endeavoured to write them in such methods, that they might contain several distinct heads, and those as comprehensive as I could easily make them, that both the young and hopeful gentleman, I call *Pyrophilus*, and I myself, might conveniently refer such other practices and experiments (especially those of tradesmen) as should hereafter occur to us, and appear to belong to those heads. And I did the less despair of his giving a kind reception to these discourses, because I could expect so little assistance in my undertaking, having never met with any book, great or small, written upon the subject I was to treat of.

IF

If hereupon it be objected, that by my own confession, divers of the particulars admitted into this book, are but slight, and some of them already known; I shall represent, that as some of the experiments spoken of are but slight, so there are others, that possibly discerning readers will not think to be altogether such; and that it was fit (for reasons already mentioned in this very preface) that I should not forbear to employ, as proofs to convince others, things either known, or easy to be made so, especially, since I commonly use them to some purpose, or other, whereto they have not been applied; and my design in the publication of these trifles being chiefly to invite the generality of readers, though of different inclinations, qualities, &c. to addict themselves to the study of experimental philosophy. The variety and easiness I have aimed at in the experiments I have set down, may, for aught I know, be more proper, than if I had confined myself to the mention of a few choice and elaborate experiments, which some readers would think impertinent to their studies, and others judge too difficult for them to put in practice. It appeared not unfit, that a book, whose title was like to procure it very different sorts of readers, should be for the most part written in a popular way; divers persons, especially those of a higher quality, by a trifle, that hath the luck to gratify their curiosity, may be more successfully invited to relish and esteem experimental learning, than by a deep notion, or a weighty experiment. And there are others, that will easier be brought to value and try experiments, by meeting with some few, though but slight ones, that happened to suit with their humour or calling, or to accommodate them on some particular occasions, than they would by many others, much more luciferous, or otherwise important. And though it were to be wished, that men's kindness to practical philosophy were grounded on the best motives; yet this treatise will not altogether miss the aim of its publication, if even upon the fore-mentioned slighter accounts, it engages readers to make, as well as relish, experiments; for the pleasantness, variety, usefulness, and other endearing qualities of such an employment will probably invite most of them to a further progress, whereby many useful phaenomena and observations are like to accrue to what is already known of the history of nature and arts. And if this shall come to pass, it will keep him from complaining of labour lost, who in venturing upon such a work, as now comes forth, was knowingly to postpone the appetite of fame to the desire of doing some service to mankind; to which end he takes one of the directest ways to be the contributing somewhat to the advancement of experimental philosophy.

It remains, that I add something more, which possibly may not a little befriend both these last mentioned answers, and several others contained in this preface; for, when all the former demands occurred to my thoughts, as likely to be made, some by one sort of readers, and some by another, those virtuosi, that were solicitous for the publication of these papers, were not backward to urge the utilities, which they fancied would thence accrue to the publick. And I cannot very well deny, that, as meanly as I think of a treatise, to whose tome I did not, till the second edition, (when I could conceal it no longer) let my name be prefixed; yet such a work as this for kind, well performed, may be a very useful one. And even of this following book, such as it is, it was suggested, that the uses would not prove despicable, in regard, that beside those, that are common to it with the formerly published tome, such as the improvement of the minds of men, and (especially) the assisting them to understand the works of God, and thereby engage them to admire, praise and thank him for them: besides these (I say) there may be other uses of the following tome, which, to avoid increasing a prolixity that I fear is already too great, I shall rather name than discourse of, contenting myself briefly



briefly to intimate, that it was conceived, the peculiar uses of this present tome might be such as these.

I. It may afford materials for the history of nature, which, that it may the more plentifully do, I have purposely, on several occasions, added a greater number of instances, than were absolutely necessary, for the making out of what I intended to declare or prove.

II. It may afford some instructions, advices, and hints to promote the practical or operative part of natural philosophy in divers particulars, wherein men have been either not able, or not solicitous to assist the curious.

III. It may enable gentlemen and scholars to converse with tradesmen, and benefit themselves (and perhaps the tradesmen too) by that conversation; or, or least, it will qualify them to ask questions of men that converse with things; and sometimes to exchange experiments with them.

IV. It may serve to beget a confederacy, and an union between parts of learning, whose possessors have hitherto kept their respective skills strangers to one another; and by that means may bring great variety of observations and experiments of differing kinds into the notice of one man, or of the same persons; which how advantageous it may prove towards the increase of knowledge, our illustrious Verulam has somewhere taught us.

V. It may contribute to the rescuing natural philosophy from that unhappy imputation of barrenness, which it has so long lain under, and which has been, and still is, so prejudicial to it. And to effect this rescue, it will in some measure enable those, that desire it, to employ those practical arguments, that are proper to convince many, that are not to be convinced by any other sort of proofs.

VI. AND which is the main of all, it may serve by positive considerations, and directions, to rouse up the generality of those, that are any thing inquisitive, and both loudly excite, and somewhat assist, the curiosity of mankind; from which alone may be expected a greater progress in useful learning, and consequently greater advantages to men, than in the present state of human affairs will be easily imagined.

OF THE  
 U S E F U L N E S S  
 O F  
 EXPERIMENTAL PHILOSOPHY.

The SECOND PART. The SECOND SECTION.

Of its USEFULNESS to the Empire of MAN over  
 inferior Creatures.

E S S A Y I.

*Containing some general considerations about the means, whereby Experimental Philosophy  
 may become useful to human life.*

**H**ITHERTO, my dear *Pyrophilus*, I have attempted to satisfy you of the usefulness of experimental natural philosophy to physick: it follows, that I proceed to endeavour to shew you, that it may be also very serviceable to husbandry, in all its subordinate parts, and to those other professions, that serve to provide men with food and rayment, or do otherwise minister to the necessities or accommodations of life; as the trades of brewing, baking, fishing, fowling, building, and the rest not needful here to be enumerated. For though the human body, in respect of the rational soul, (which is the inventress and seat of sciences) be one of the corporeal things, over which the empire of knowledge is to be established; yet, taking man as a creature made up of body and soul, the advancement of his empire seems to consist more properly in the enlargement of his power over the other creatures: physick seeming rather to defend him against revolts and insurrections at home, than to increase his power, and extend the limits of his empire abroad.

BUT, *Pyrophilus*, I hope, you do not expect, that I should now insist on each, or so much as on any of the above-mentioned trades, by whose intervention it is, that man exercises his dominion over external bodies. For such a work would require little less than an age, and much more than a volume; and besides (that it is vastly disproportionate,



tionate, both to my slender stock of mechanical skill, and to the little leisure I have to conclude this section in) I could not acquaint you with all that I could pertinently enough deliver about these matters, without too much defrauding some other treatises, that I design you: and therefore, I hope you will be content, if, in the remaining part of this tract, I do not only present you a not despicable number of considerations proper to manifest that, and to intimate, how experimental philosophy may be of great use to the promoting of mechanical arts and trades, but illustrate and confirm all, or most of those considerations by particular instances, derived from observations and experience.

This I shall, God assisting, endeavour to do in the following essays. But before I descend to particulars, it will be expedient in this place to premise some general considerations relating to the influence of experimental philosophy upon trades, and two or three advertisements, that concern the ensuing discourses.

## SECTION I.

FIRST then, to make it probable, that a true insight into natural philosophy may be capable of affording some reformation, or other kind of improvement to trades, I shall desire to consider, that being, for the generality of them, conversant about some few particular productions of nature, such men as are thoroughly skilled in her general laws, and acquainted with a vast number of her productions, and versed in the ways of applying nature and art jointly to several purposes, according to the several exigencies of things; such sagacious persons (I say) will, in all likelihood, be able, some way or other, to meliorate the inventions of illiterate tradesmen. As the husbandman's skill, for instance, consisting chiefly in the observations of the nature of a few plants and animals, their relation to such and such soils and kinds of culture, and the operations of stars and meteors upon them, which are subjects, that properly enough fall within the cognizance of the naturalist, it cannot seem improbable, that he, that has seriously and industriously inquired into the nature of generation, nutrition, and accretion, both in plants and animals, and knows how to vary an useful experiment, when once found out, so as to remedy the inconveniences, or supply the deficiencies, or improve the advantageousness, or translate and apply the use of it, and, in sum, he that can knowingly and dexterously manage, what his own or other men's observations have afforded him, will be able to cultivate the ordinary husbandman's skill with as much improvement, as that confused skill enables the husbandman to cultivate his ground.

## SECTION II.

To carry on the foregoing consideration a little farther, I will add, that it may as well conduce much to the manifesting, how much trades are subordinate to natural philosophy, as to the improvement of trades themselves, that it be attentively considered, what things each particular trade is, as it were, made up of. As for example, the chief things in the refiner's trade are, to know the ways of making, and the operations of aqua fortis upon silver, gold, and copper; to know how to purge that menstruum, that it may dissolve no gold, nor precipitate any of the silver it dissolves; to know what proportion there ought to be dissolved in it; to know with what quantity of water to weaken the solution, and how long copper-plates need lie in it, to precipitate all the silver out of it; to know how lead is to be colliquated with them, and what proportion of it is ne-

cessary and sufficient to carry off with it (when it is blown off upon the test) the baser metals; to know how to make cupples of several sorts and sizes, and upon them to draw off the lead or antimony from the silver or gold, and discern when the metal is sufficiently refined; to know what proportion of gold and silver is requisite for the making of water-gold as they call it, (because it is separated from silver by aqua fortis, which dissolves this metal, and leaves the other in a fine powder;) these things, to which many others are subservient, belong to the refiner's trade, which, though understood by few, seems to be a very narrow and simple trade, in comparison of a hundred others, whose operations are far more numerous and complicated. Now if all trades were judiciously resolved (if I may so speak) into the several parts they consist of, it would, I question not, manifestly appear, that the most, if not all of them, are in many particulars but corollaries deduced from some particular physical observations, or but applications of them to the uses of human life.

AND if this be so, you will not, I presume, think it unlikely, that by a farther discovery of the nature of those particular bodies wherewith the trade is conversant, and a solid knowledge of those laws of nature, and those operations of bodies upon one another, which it employs; some, if not most, of those parts, whereof the trade may be conceived to be made up, may be reformed or bettered; which is enough to make the philosopher an improver of the trade, which he may become upon such unobvious accounts, that perhaps it may not unreasonably be hoped, that even the chymist's charcoal may be made, by a good naturalist, equivalent to an excellent compost for land. For if it be true, as well as it is probable, not only that the food of those animals (as oxen, sheep, &c.) which the husbandman deals with, springs out of the ground; but that the plants, which affords them this food, are themselves nourished by a certain vegetative salt they find in the ground; and, that this salt being by frequent seminations exhausted, the soil grows barren, until either by the air, or the steams of the subterraneous parts, or the spontaneous maturation of the saline rudiments contained in the ground, or by adventitious manure, or by all or divers of these together, it be re-impregnated with a new vital saltiness: if these things be true, I say, then those chymical experiments, that conduce to discover to us, what kind of salt that is, and to what other salts it is allied or opposite, as it is to several acid ones, may probably afford very useful directions to the husbandman, towards the meliorating of his land, both for corn, trees, grass, and consequently cattle. And having had the curiosity \* to distil some earths, some dungs, and some seeds, and observe the salts abounding in the liquors yielding by them, (of which we have elsewhere occasion to speak) we found cause to wish, that experiments of that nature, in relation to the improvement of husbandry, might be industriously prosecuted by naturalists. He that has observed those many particulars in husbandry, which might invite that great naturalist Sir F. Bacon † (who yet mentions very few of them) to pronounce, that nitre is, as it were, the life of vegetables; he that observes how conducive that fertilizing dung of pigeons is, both to make earth fruitful to the husbandman, and to impregnate it with nitrous salt for the saltpetre-man; and he that knows, that moist fat earths, so defended from the rain and sun, that the one may not draw up, nor the other wash down the embrionated saltiness of them, will after a time abound in nitrous salt, if they are not permitted to spend any in producing of vegetables; such a

\* *Verulam* hist. v. & mort. p. 237. Certissimum est quamcunque terram, licet puram, neque nitrosis admixtam, ita accumulata & lectam, ut immunis sit soli, neque emitat aliquid vegetabile, colligere etiam satis copiose nitrum.

† *Nat. Hist.* cent. 5. exp. 444.



one, I say, will perchance be apt to think, that enquiries into the nature of saltpetre, may be of great concernment to husbandry. And to give you, *Pyrophilus*, some inducements to expect, that chymistry may be very useful in such kind of enquiries, I shall here mention to you a couple of my experiments relating to nitre.

THE first is that, whereby I endeavoured to give an inquisitive person hopes, that materials, which seemed unlikely, might, by due changes, and without much art, be turned into saltpetre. The experiment was this: I cauled some earth to be digged up just underneath the clay-floor of a pigeon-house; such earths being believed to abound the most with nitre, that needs only to have its particles brought together and united to compose saltpetre: a pretty quantity of this earth being put into a retort, and distilled with a good fire *ex arena*, afforded me, though little or no oil, yet a pretty quantity of a reddish liquor, which, instead of being, as others would have expected, of an acid nature like spirit of nitre, was fit for my purpose, by strongly participating of the nature of volatile salts; as appeared, not only in that I could, without rectifying it, turn syrup of violets with it immediately green, and precipitate a solution of sublimate into a milky substance; but because there came over, with the spirit, into the lower part of the receiver, a salt in a dry form, which not only was in taste not unlike the other volatile salts, but was so far from being of an acid nature, that with an acid menstruum it readily fell to his, and made an ebullition. So that it seems (which in an enquiry about nitre is very considerable,) that a salt, very repugnant to acids, may, by the operation of the earth and air, be so altered, as afterwards by a slight management to afford saltpetre, whose spirit is highly acid. But of this experiment I may hereafter make farther mention.

THE other, (which we elsewhere have occasion more particularly to take notice of with reflections on it) is briefly this. We took pot-ashes, which you know contain but the salt of burnt vegetables; and on those, first dissolved in a little fair water, we dropped aqua fortis (whose saline part consists indeed of little else than the spirits of nitre,) till all ebullition and hissing betwixt it and the resolved pot-ashes were perfectly ceased; and having filtrated this liquor, and set it in an open vessel in a gentle heat to evaporate, it did within two or three days after, (and sometimes, for we made it more than once, even in a few hours) being removed to a cold place, afford us very pure crystals of saltpetre, as both their shape, and flashing (on live coals) into a blue halituous flame, informed us. And since I have had occasion to mention the use of saltpetre in husbandry, I shall not forbear to add, that the knowledge, which the naturalist, as a discerning chymist, may give the husbandman of the natures and distinctions of saline bodies, may be of no mean use to him, by assisting him to discern and observe the considerable differences of the various saltinesses to be found in soils, and what sort of saltiness each particular seed or plant most affects. For by this means, not only many grounds might be made useful, which are thought barren, only by reason of our not knowing for what plants the saltiness predominant in them may be proper; but the same ground may yield much frequenter crops than commonly it doth, when it is successively sowed only with one sort of seed, by the due alteration of plants delighting in the several sorts of salts to be met with in that ground; which oftentimes, by being impoverished, or rather freed from one sort of salt, doth but the more plentifully feed those plants, that delight in another: which, in some places we have observed, that husbandmen seem to have taken notice of already, by sowing (in fields too remote from their dwellings to have compost brought to them) turnips, to fit the ground for wheat, and serve for a manure; though in this method some other circumstances may possibly concur with the nature of turnip-feed, to the preparation of the ground for wheat. And I am

prone.

prone to think, that there is scarce any ground or soil, except perhaps mere sand, that might not, even without much culture, be made fertile, or at least kept from being altogether barren, if we were on the one hand skilled in the ways of discerning the nature of the ground; and on the other hand acquainted with, and provided of, all the variety of seeds and plants that nature has, though not all in one country, afforded us. For there are divers soils, which here in *England*, or in other regions, are, as useless, left quite uncultivated; which seeds or plants, that abound in other countries, and would probably be made to grow in these, would make serviceable to the husbandman. Many steep and abrupt portions of ground (some of them very large) exposed to the southern sun are left altogether waste, not only in *England*, but in divers hot climates, where the planting of grapes for wine is not yet in use; though such pieces of land in *France* and *Italy*, and, as I have observed, even in the *Rhetian Alps*, nourish excellent vineyards.

I know an ancient and landed gentleman, who communicated to me upon his own knowledge an experienced way of making wheat grow and prosper well on mere clay, where there was no grain at all did thrive; which, though I have not hitherto had opportunity to try, yet upon the credit of a person so sober and qualified, I scruple not to mention it here, because the art consisting mainly in the imbibition of the feed for a determinate time in a certain expressed oil, that is not dear; it may make it probable, that without altering the whole soil by manures, a slight, but convenient change made in the feed itself may serve to make them fit for one another. And (to add, that upon the by) to shew, that the particular dispositions of some sorts of seeds may enable them to make the ground they are sowed in, much more productive, than it would otherwise be, I shall relate to you, that being not long since in the company of a learned and curious traveller, I saw, among some rarities of a quite other nature, an ear or two of corn, not much unlike our common wheat; at which being somewhat surprized, I asked him, what peculiarity had procured that grain admission among such rarities? to which he replied, that in the warmer region, where he begged it of a virtuoso, one of those grains would afford so vast a multitude, as he was almost ashamed to name, and I am more than almost afraid to repeat: but before I went out of the house, an English gentleman, that had a more than usual curiosity for such kind of trials, assured me, that having obtained some grains of that corn, and carefully sowed it in some land of his own, not far from the place we were in, he had out of a single grain several hundreds; though not near so many of them, as the other traveller, who yet was a very sober and judicious man, related to have been produced in a better climate and soil. Of this strangely prolific wheat, the gentleman readily granted me a promise of a sufficient quantity to make a trial; whereof, when I shall have received it from a servant of mine in the country, you may command the success. And this brought into my mind what I read in the learned Jesuit \* *Acosta*, who affirms, that in divers parts of *America*, where it is known, that our European wheat prospers not, the Indian (or, as many English have styled it, Virginian) wheat, they call *Maiz*, does so wonderfully thrive, that although the stalk bear often more than one cluster, and the grain be big; yet in some clusters he has reckoned seven hundred grains: to which he adds, that it is not strange in those countries to gather three hundred fanegues, or measures, for one sown. Which passages, especially the former, speak of an increase that seems so little credible, that I should on that account forbear to mention it, were it not, that in *Europe*, and even in *England*, I myself have reckoned such a multitude of grains upon one of the very numerous ears produced by the same single grain, that I found myself very inclinable to

\* Lib. IV. cap. 16. as he is published by *Purchas*.



absolve *Acofta*, and continue to look upon him as one of the best writers of the natural history of *America*.

We now proceed to take notice, that in some Eastern countries, a sort of rice (a grain that makes the chief and most usual food of the natives over almost all those parts) prospers very well upon land so drenched with waters, that seeds-men, to scatter the rice, do rather wade than walk. But this itself (which, for the main, was confirmed to me by eye-witnesses) is less strange, and does less illustriously confirm what I was proposing, than what the inquisitive Jesuit *Martinus* affirms to be the practice of some (as well great as small) countries in *China*, where, in divers places, that are all the year under water, and would by our European husbandmen be thought capable of no other use, than that of ponds or lakes, the Chineses cast a certain seed so well appropriated to the place, that is to receive it, that though it falls not immediately on the land but on the water, so that one would think they were not about to sow a field, but bait a pond for fishes, yet this seed, being adapted to the soil it meets with at the bottom of the water, does so well prosper and shoot up to the top, that in its proper season the surface of the water looks as fresh and verdant, as a fruitful meadow, and yields as rich a crop. But for fear of digressing, I shall, *Pyrophilus*, proceed to tell you, that perhaps also chymistry, especially in conjunction with hydrostaticks, may prove serviceable to the ingenious husbandman, by assisting him to discover the kinds and degrees of saltiness that are in several other bodies that he much deals with. I remember I have met with things surprizing enough, in examining some sorts of earths by distillation, and by several chymical instruments of discovery; but though I have likewise had the curiosity to distil dungs and grain, and fruits, and some other subjects, wherewith the husbandman is conversant, to observe what kinds of saline and other liquors, and in what proportion, and of what strength, they could afford me; yet not having any notes by me of the particular trials, I shall content myself to have given you this hint of a new sort of experiments in husbandry; and shall only add, as to salts, that since the fertilizing power of dungs seems to reside in the salino-sulphureous part of them, (and the like I have by chymical trials found in lime;) a practical insight into the differences and differing operations of salts (about which I elsewhere entertain you) may probably very much assist the husbandman to examine the several dungs, and other composts (the knowledge of which is of great moment in his art) and to multiply, compound, and apply them skilfully.

And as chymistry, that is conversant about fire, so even hydrostaticks and hydraulicks, that teach us to make engines and contrivances for the lifting up, and for the conveying of water, may in divers places be of no small use to the husbandman. For not to mention what is done in some more known parts of the East, of the like nature with what I am going to mention, *Martinus* informs us, that in one province of *China* (whose name I remember not) they are so curious to water their fields of rice, that they have upon the river excellent mills so made, as that great quantities of water are continually raised in buckets, or other convenient vessels, fastened to vast wheels driven by the stream; which watering-mills (to add that notable instance upon the by) are not, as our European mills are wont to be, fixed to one place, but built upon vessels, with which they may remove the mills, how great soever, from place to place, as occasion requires. Nor is this eastern way of raising water by wheels, so as that it may be conveyed by convenient channels to places many feet higher than the river, or other receptacle of the water, that is to be distributed, the only way, whereby the hydraulicist and mechanic may assist the husbandman; since he may considerably do it by the art of libellation, or conducting of water upon the ground. For the improvement, that

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may be made of land by water, in soils fit for that way of culture, may be far more considerable than is yet wont to be taken notice of; as indeed this husbandry itself is in many countries both elsewhere, and in *England*, as yet unpractised. I have had some lands of my own much bettered by being skilfully overflowed; so that when I observed the difference, the tenant, though shy of acknowledging the utmost advantage, confessed to me, that he thought it yielded him double the former income. And a gentleman of quality of my acquaintance, whose improvements I went lately to view, shewed me a scope of ground, which at his first coming to that wild place, four or five years ago, was boggyish, and which yet he had turned into a good dry soil, by only trenching it here and there with shallow trenches of not a foot deep, and overflowing it, by the means of those trenches, and conveniently placed dams, as evenly as he could, five, six, or seven times a year, betwixt the beginning of October, and about the middle of April, with the water of a neighbouring spring, which was no way enriched by land-floods, arising but in a very barren and uncultivated place, far from the neighbourhood of grounds capable of enriching it; and yet this spring drained away, if I may so speak, that ancient hydropical distemper of the land, and turned it, as I found by trial, into a good compact soil, on which store of mowers were (when I saw it) employed in making of hay, which this meadow yielded plentifully enough to be worth twenty times its former value. Nor is this the single considerable instance we have met with, of the improvement that may be made of divers kinds of land, only by skilfully overflowing them with common waters.

BUT, *Pyrophilus*, I may hereafter have so many occasions to mention particulars relating to agriculture, that I should presently dismilt them in this essay, were it not, that I am, by my having named husbandry to you, put in mind to employ it as an instance to confirm this observation, that the more comprehensive a trade is, the more likely it is, that it will be capable of being meliorated by natural philosophy. For such trades, as are of great extent, are obliged to deal with a considerable number of nature's productions, and to make use of divers of her operations; and consequently must comprehend the more particulars, wherein the manufacture or profession may be reformed, and otherwise advantaged by a knowing and dexterous naturalist. Thus the husbandman's corn makes it fit for him to have a competent skill in the whole art of tillage, the keeping of cattle great and small, the ordering of dairies, of wood, of flax and hemp, of hops, of the kitchen-garden, of an orchard, of bees, &c. besides that the particular productions of some of these, as honey, cyder, &c. require some skill, and are capable of much improvement; so that among so great a variety of things, wherewith the husbandman has to deal, it can scarce be otherwise, than that there will be several things, wherein the naturalist's higher and more reaching knowledge and experience will be serviceable to him. And whereas in the preservation both of cattle from diseases, and of the fruits of the earth from putrefaction, lieth one of the most beneficial and difficult parts of the husbandman's skill, he may therein be much assisted by an expert naturalist; who not only, by being able to accelerate putrefaction in divers bodies, may teach the husbandman to furnish himself with great variety of composts and manures, to relieve and enrich his ground with whatever peculiar sort of salt he observes to be deficient; but also may teach him how to preserve many of his seeds, and flowers, and fruits, beyond their wonted duration: as I know some persons, to whom I recommended methods of this kind, that use to preserve quinces, for instance, a great part of the year, by a strong liquor, or pickle, made of nothing but water, and what (for the most part refuse stuff) may be easily obtained from the quinces themselves. This way presented us fruit at almost the year's end; and a while since I could have shewn you (and, for aught I



know, can do so yet) cherries well shaped, and succulent enough, of above a year old, preserved without salt or sugar, by being kept in a spirit of wine fitted for that use and fully impregnated, before their immersion, with the tincture of the skins of other cherries of the same kind. The vast benefit that the Hollanders derive from the best way of salting or pickling of herrings, and the advantageous use that is made by others, of so powdering beef, and ordering other flesh, that it will last good to the *Indies*, and is sometimes brought uncorrupted into these parts again, may persuade us of the benefit that may accrue to the husbandman by the discovery of the ways of keeping the productions of the earth from corruption; especially if his skill be extended to weak wines, cyder, perry, and other liquors, which are wont to be made in great quantities, and yet apt to decay at home, and unfit to be transported far abroad. And the use of sugar to strengthen vinous liquors, and make them durable; and, without the help of salt or any sharp thing, to preserve great variety of fruits, and of the juices of herbs, may encourage us to think, that there may be very differing ways (and some of them seemingly opposite) to make many things outlast their natural periods of duration.

BUT my trials and observations (whether about the conserving of fruits, flowers, and flesh, or of other things of this sort) belonging more properly to another discourse (of the preservation of bodies) I shall now mention no more of them, but pass on to tell you, that very much prejudice, which often happens to the poor husbandman (and sometimes even to his utter ruin) by those, either stubborn, or contagious diseases (such as the rot in sheep, and the glanders in horses) that make havock of his cattle, may in great measure be prevented by the instructions of a knowing naturalist, especially if he be an expert physician too. For as many diseases, so many cures are analogous in men and beasts; and the remedies prove frequently more successful in these than in them, as well for divers other reasons, as because the bodies of many brutes are more able to bear the operation of strong remedies; and yet the unaccustomedness of almost all of them to physick makes them more relievable, than men by any (not improper) remedies. I will not now relate, that I have in some countries found medicines, that have been usefully tried against diseases in men, cried up for their efficacy against their analogous ones in horses; nor with what difference in the dose these may be purged by several of the same catharticks, especially aloes, that are employed for the purgation of human bodies. I shall rather inform you, that as in these salt is, you know, reputed a great resister of corruption, and an enemy to worms (with a sort of which the livers and neighbouring vessels of sheep have been observed to be infested); so by the bare use of Spanish salt, which each sheep, being first made to bleed a little under the eye, was made to take down a small handful, two or three times, with some days of interval, without being suffered for some hours to drink any thing after it: by this remedy, I say, given at the time of the year when there is danger that the sheep will begin to be blotched, many flocks have for divers years been preserved by a rich intelligent gentleman of my acquaintance, that is a great sheep-master, and has thereby (and that also lately) preserved his flocks in a moist country, when most of his neighbours lost theirs. I might here mention to you, *Pyrophilus*, the virtues of crude antimony, to cure the foulness of blood, and even the leprosy in swine; of quicksilver, to cure the worms in horses; of *Palmaris*'s famous remedy, which he solemnly affirms to be a constant one against the bitings of a mad dog in cattle, and of a more parable one for men also, whose success I almost admired in a near relation of yours and mine; of the use of the antimonial cup for several sicknesses in horses and sheep, which (if I misremember not) was successfully tried by one to whom I recommended it; and of another antimonial medicine, which (though much commended to me by a virtuoso that took it himself) a gentleman of my ac-

quaintance, resident in the country, who prepares it, assures me, that he uses it with strange success to fatten his horses (made lean by occasion of sickness) with whom yet it works not, either as an emetic, or a purge. And I could here present you divers other receipts much prized for their having (as well as the newly mentioned remedies) frequently been found effectual against the same diseases both in human bodies and in brutes, if I did not think it less proper to make in this place a veterinarian excursion, than to tell you, that, if you have any curiosity for them, you may command them.

I MIGHT add, if I had leisure, some reasons, why I despair not, that in time the husbandman may, by the assistance of the naturalist, be able to advance his profession by a therapeutical part, which may extend not only to the animal productions of the ground, and to the vegetable ones; but (in a large acception of the term) to the distempers of the ground itself. For if the causes of the barrenness of soils in general, and of their indisposition to cherish particular plants or animals, were by the philosopher's sagacity discovered, I see not why many of those defects may not be removed by rational applications and proper ways of cure; as well as we see inconveniences remedied in many other inanimate bodies, without excepting the close and stubborn metalline ones themselves.

AND perhaps also, that by a way of management suggested by the knowledge of causes the barrenness of a soil may be cured, or its fertility much promoted by methods, that do nothing near so much require cost as skill. Some ingenious husbandmen have of late proclaimed themselves much satisfied with a way of correcting two of the barenest sorts of land, not by rich manures or other costly cultures, but by skilfully mixing the sand and clay themselves in a due proportion, according to the use the husbandman designs to make of it. And whereas one of the best modern writers of agriculture reports, as he may, for a strange thing, that he had seen seven or eight and thirty ears of barley, that sprung from one grain; I remember, that an ingenious gentleman, to satisfy some curious persons what might be done in that kind, sowed corn upon a piece of land, very near the place of my abode, which prospered so strangely, that one root that I took particular notice of, though perhaps not the fruitfulest in the field, produced sixty and odd ears of corn; and yet, which was the strangest, this wonderful increase depended upon a philosophical observation; nothing extraordinary having been done, either to the land, or so much as to the seed; as I had opportunity to know, both by the informations of observing men, and by the confession of the gentleman himself, who was pleased to make choice of me to intrust his secret with, that, in case he died before me, the publick might not lose it. Upon which account he also confided to me another specimen of his skill. He once presented your excellent mother a company of several sorts of choice apples, among which there was one sort excellently tasted, but very small; the following year he presented her another basket of the like fruit, but finding no small ones among them, she took occasion to ask him, what was become of the tree that produced those delicious little apples, that made part of his former present? to which he replied, that he had brought several of its productions among the other fruits she was looking on; and thereupon shewed her some that came from the same tree, and appeared by the peculiar relish to be of the same sort, though exceedingly differing in bulk, that neither your mother, nor I, had any suspicion that the same tree bore them. Upon which occasion he readily gratified my curiosity by acquainting me with his way, which depended almost only upon a physical observation; all that he added being not any rich compost, but some despised leaves of a very cheap and common vegetable. But husbandry is too large a subject for me to prosecute in this place, and therefore I shall here dismiss it.



SECTION III.

THE next thing I shall observe to you, *Pyrophilus*, is, that it is not only to the trades that minister to the necessities of mankind, but to those also that serve for man's accommodation or delight, that experimental philosophy may bring improvements; for these arts also do, for the most part, consist in the knowledge and application of some of nature's productions and courses, whose being referred to the accommodation or delight of men, rather than to any other purpose, does produce nothing that is truly physical in the things so referred, which thereby acquire only such a kind of respect to man, as that which the metaphysicians call an extrinsecal denomination; and we see that the same things, without varying their nature, are serviceable to men in very differing capacities: as wine serves one that is dry to quench his thirst, serves a fainting person to revive his spirits, and the drunkard to inebriate him; the same spirit of wine, that serves the physician to make tinctures and extracts for the recovery of health, may serve the ladies to dissolve benjamin into a tincted liquor, that diluted with fair water may be used as a cosmetic, which I have received many thanks for; and the same spirit skilfully employed upon ingredients, to be named to you ere long, is of excellent use for making divers fine varnishes made with rectified spirit of wine; nay, the newly mentioned solution of benjamin may itself be applied to all those differing uses; for of itself it is a pretty and odoriferous varnish, and I have used it (though not often, for want of opportunity) with very good success against a sort of tetters, which I caused frequently to be bathed with it. What happy applications knowledge and skill may make even of unpromising things, to the furnishing men with delights, is methinks very evident in musical instruments, as lutes, viols, &c. For who would think (if experience did not assure us of it) that with a few pieces of wood joined together, and the guts of cats or lambs wreathed or twisted into strings, the skilful musician, by the help of mathematicks and exercise, should be able to charm the ear with the greatest, as well as most innocent delights, the sense belonging to the organ is capable of, and which sometimes does not only please, but ravish the transported hearers. But though, *Pyrophilus*, as I was lately saying, physicks may not only be very improving to those arts and professions that serve to provide man with the necessities or accommodations of life, but also to those that serve chiefly to furnish him with pleasures and delights; as might be instanced in experiments of colouring, perfuming, making sweet-meats of all sorts, embellishing the face with cosmetics, and divers others of the like voluptuous nature; and though I may elsewhere have occasion, when I come to treat of colours, odours, tastes, and other qualities, to acquaint you with some receipts and experiments of this kind; yet now I do not only want leisure to mention them, but am desirous, that natural philosophy should engage you to court her, rather by her gratifying and enamouring your reason, than by her bribing and inveigling your senses.

SECTION IV.

THOUGH what has been represented about the usefulness of experimental philosophy to trades does chiefly belong to those, wherein nature's productions are employed to human uses, by those operations, wherein nature herself, rather than the artificer, seems to have the chief hand, as the trades of brewing, baking, gardening, tanning, &c. yet I would not exclude those very trades, wherein the artificer seems to be the main agent,

and in whose ultimate productions the chief thing that is wont to be considered, is the adventitious shape or form, which the artificer, as an intelligent and voluntary agent, does, by the help of his tools, give the matter he works on, as in the trades of the smith, the mason, the cutler (when distinct from that of the sword-maker) the watch-maker, and other handicrafts. For though these consist rather in the manual dexterity of men, than the skilful ordering of the productions of nature, by their material operations upon one another; yet to many, if not all, even of these, the naturalist may some way or other be a benefactor.

For there are divers of these manual trades, that, especially as they are exercised in cities and greater towns, consist of several parts, and have need of several other trades to prepare materials for them, and dispose them to receive the last form, which the artificer is to give them, to fit them for sale. And we may, in many cases, observe, that though this artificer, that gives the matter this last form, does it chiefly with his hands and his tools; yet those other tradesmen, to whom he is beholden for his materials, do some or other of them, to prepare and qualify them for his use, need some observations of the conditions of the body they deal with, or must employ some physical operations, wherein they may be much assisted by the knowing naturalist, who may also teach the manual operator himself how to make choice of his materials, and examine the goodness of those that subordinate workmen shall bring him. Thus, though stone-cutting be a trade that seems to consist almost wholly in giving, with proper tools, to marble, freestone, and other materials, the shape, which the artificer designs; yet, if I had leisure, I could easily shew you, that even in this trade, there are many particulars, wherein experimental philosophy might be helpful to the artificer. For ways, hitherto unused, may be found out (as I have partly tried) to examine the nature and goodness of the marble, alabaster, and other stones, which the mechanicks deal with. A competent knowledge of the sap, that is to be found in stones employed for building, is of so much importance, that the experienced master workmen have confessed to me, that the same sort of stone, and taken out of the same quarry, if digged at one season, will moulder away in a very few winters; whereas digged at another season, it will brave the weather for very many years, not to say, ages (but of my observations of this kind, more elsewhere). The cements also, and stoppings (as they call them) which are of good use in this trade, may be easily bettered by the naturalist that is versed in such mixtures. And I remember, I had occasion to teach a fine cement for the rejoining of the broken limbs of statues to their bodies, to an inquisitive artificer, who, by such like helps, did in other cases so well counterfeit marble with a cement, that even where there was occasion to fill up great cavities with it, the work would pass for entire; the additaments being not distinguished from the natural marble. Want of curiosity also keeps our stone-cutters here in *England* unacquainted with the ways of working upon porphyry, which they will not undertake either to polish or to cut. Nor is *England* the only country, where the art of working upon porphyry (which appears to have been in great use amongst the *Romans*) is unknown, though at *Rome* there are some few, that do with great gain exercise it. And though I know not precisely, what it is they employ, yet I presume, it may be powder of emery; for with that, and water, and steel-saws, I have here in *England* caused a porphyry-stone to be cut. And the mention of porphyry puts me in mind of telling you, that by an art I have, white marble may be so stained, and that durably, with spots great or small, and red or brown, as it pleases the artificer, as I may hereafter have occasion more fully to relate. It would be too long to discourse to you here of artificial marble, and divers other things, that stone-cutters affirm to belong to their trade, wherein you will scarce doubt, but that it may be capable of improvement.

Where-



Wherefore I shall only add, that whereas this profession does much require very good steel-tools, and they must have these from smiths, and others that deal in iron, if these men's trade were bettered by the naturalist, they might be able to afford the stone-cutter the better tempered tools; and that even the smith's craft, though it seem to be merely a manual art, is yet capable of much melioration by the knowledge of nature, were not difficult to manifest, if it were proper here to insist on the proofs of it; yet thus much I shall here take notice of, to confirm this fourth observation, that not only the philosopher may, as a mineralist, and a mechanician, improve the ways of making iron and steel, before they come to the smith's hand, but likewise may devise better expedients, than are among us in use, for the ordering of iron and steel, when it comes to be formed into weapons and tools. The sword-blades, and other arms, that are made at *Damasco*, are very famous every where, and (as far as some trials have informed us) justly for their excellency in cutting even iron. And yet it seems to be only the skill of the artificers in ordering it, that gives the swords and other instruments made at *Damasco* so great a preheminance above others. For though the goodness of them have been presumed to proceed from that of the iron-mines, and steel, peculiar to the region of that city; yet the judicious *Bellonius* \*, having made particular inquiry at his being there, informs us otherwise, and tells us, that iron and steel, being brought thither from other parts (the country having no mines of it) receives there from the skill of the workmen its temper and perfection. And I see not, why I may not reasonably suppose, that in the tempering of steel, it is not only the goodness of the metal, and the determinate degree of heat, though these be the only things artificers are wont to look after, that give the best temper; but that much may depend upon the nature of the liquors, or other bodies, wherein the hot steel is plunged, and upon other ways of ordering it, if those be skilfully chosen and employed. I have had a graver so well tempered (but by whom I know not) that all the known ways used by me and others (who wondered, as well as I, at the unsuccessfulness of our endeavours) could not deprive it of its temper, as they would have done any gravers that we make here; and it was afterwards affirmed to me, that it was made of steel tempered at *Damasco*.

I MAY elsewhere tell you, *Pyrophilus*, both of a way I have tried of hardening gravers, without quenching them in any liquor or tallow, or any other unctuous body; and that having persuaded an ingenious artificer to try an unpractised way of tempering gravers, he soon after brought me one to see the goodness of it, which, by being plunged in a certain cheap mixture (wherewith I may hereafter acquaint you) had been hardened and tempered at once; which though most artificers would think scarce possible, yet, upon the authority of trial, I shall venture to deliver, what some may think as strange, namely, that though ignition and extinction in cold water be the common and known way to harden steel-gravers, yet by that way, only observing precisely a nick of time, steel may be made strangely soft. But of this more elsewhere. I shall now add, that having enquired of one of the curiousest and most observing makers of steel tools, whether he did not find a difference in the employing of pump-water, or river-water, in giving them their temper, he satisfied me, that he did so; and observed the former to be fitter for some sorts of tools, and the latter for others. There may be divers other particulars, wherein iron and steel may be improved by the naturalist. The first may be this; that the metal be rendered so soft, as to be, by the help of strong moulds, put into shapes. This an eminent and credible artificer assured me, he had often seen his master do to iron, with considerable profit. Or else it may be made fusible like an-

\* P. Bellonius observat. Lib. II. cap. 93.

other metal, as I remember I have (sometimes, with a certain flux-powder, which I composed, if I much forget not, of tartar, sulphur, and arsenick) made it run, even with a charcoal fire, into a mass exceeding hard, and very polishable. A third way may be this; that it be so ordered, as to be preserved very long from rust, which an ancient virtuoso, who had purchased the secret of a rare artist for a great price, and used to shew his friend's steel so prepared, assured me, was done chiefly by tempering it in water well impregnated with the bark of a certain tree. In a word, there may be divers other ways, whereby iron or steel themselves, or their trades that employ them, may be meliorated; and to add, that on this occasion, there are many and very differing accounts, upon which a trade or profession may be benefited by the Experimental Philosopher; for he may either find out variety of materials, wherewith to perform the things desired by the tradesman, or he may render those materials that are already in use better conditioned; or he may discover and reform the unheeded errors and mistakes to be met with in the trade; or he may devise more easy and compendious ways of producing the effect that is required; or he may improve some of the auxiliary trades, of which the trade spoken of has need or use; or may instruct the artificer to choose, and examine, and preserve his materials and tools better than is usual, or can make the ultimate productions of his trade sooner, or cheaper, or easier, or better conditioned, or applicable to more uses, or more durable, than they are commonly made. Nor are these all the particulars that might here be enumerated to the same purpose, if this fourth consideration had not detained us too long already.

## S E C T I O N V.

THE naturalist may increase the power and goods of mankind upon the account of trades, not only by meliorating those that are already found out, but by introducing new ones, partly such as are in an absolute sense newly invented, and partly such as are unknown in those places, into which he brings them into request. For it were injurious both to nature and to man, to imagine, that the riches of the one, and the industry of the other, are so exhausted, but, that they be brought to afford new kinds of employments to the hands of tradesmen, if philosophical heads were studiously employed to make discoveries of them. And here I consider, that in many cases, a trade differs from an experiment, not so much in the nature of the thing, as in its having had the luck to be applied to human uses, or by a company of artificers made their business, in order to their profit; which are things extrinsical, and accidental to the experiment itself. To illustrate this by an example; the flashing explosion made by a mixture of nitre, brimstone, and charcoal, whilst it past not farther than the laboratory of the monk, to whom the invention is imputed, was but an experiment; but when once the great (though unhappy) use, that might be made of it, was taken notice of, and mechanical people resolved to make it their profession and business, to make improvements and applications of it; this single experiment gave birth to more than one trade; as namely, those of powder-makers, founders of ordnance, gunners (both for artillery and mortar-pieces) gunsmiths; under which name are comprized several sorts of artificers, as the makers of muskets, small pistols, common barrels, screwed barrels, and other varieties not here to be insisted on.

THE discovery of the magnetical needle's property to respect the poles has given occasion to the art of making sea-compasses, as they call them, which in *London* is grown to be a particular and distinct trade. And divers other examples may be given to the same



same purpose; especially where mechanical tools and contrivances co-operate with the discovery of nature's production. So that oftentimes a very few mathematical speculations, or as few physical observations, being promoted by the contrivance of instruments, and the practice of handicrafts men, are turned into trades; as we see, that a few dioptrical theories lighting into mechanical hands, have introduced into the world the manufactures of spectacle-makers, and of the makers of those excellent engines, telescopes, and microscopes.

THE observing, that though quicksilver will amalgame with gold (and thereby seem to be destroyed, which made *Pliny* think it an enemy to metals) yet it may be separated from the gold again, without diminution of that noble metal, has brought forth the trade of gilders, whose art consists chiefly in mixing, by the help of a competent heat, good gold with five, six, or seven times its weight of quicksilver, until the mixture come of such a consistence, that they may spread it as they please upon the silver or copper to be gilt. For having by this means overlaid it evenly with gold, they can easily with fire force away the mercury; and, with a liquor impregnated with nitre, verdigris, sal armoniack, and other saline bodies, which they call a colourish, restore its lustre to the remaining gold, which they after make bright by polishing.

THE almost obvious and trivial observation made by some sagacious person (whoever it was) that a spring was a physical, continual, and durable power or force, and the corollary he thence deduced, "that this force, skilfully applied, might be equivalent to the weights that were thought necessary to move the wheels of clocks:" these reflections, I say, joined with a mechanical contrivance, produced those useful little engines, watches, that now afford a plentiful livelihood to so many dexterous artificers; which, though custom has made familiar to us, yet were unknown to the ancients, and highly prized and admired in *Cbina* itself, when first (in the last century) brought thither. The discovery of the virtue of aqua fortis, to dissolve silver and copper without working upon gold, added to the observation, that lead melted with either of the two noble metals, and then forced from them by fire, will carry away with it any of the baser metals that may have been mixed with them; these two particulars, I say, have begot in latter ages the art of the refiners we now have.

MEN's having observed the operations of some lixiviums, clays, and a few other familiar things upon the juice of the sugar-cane, has not only occasioned the adding of the culture of those reeds to the other parts of husbandry left us by the antients; but has produced the several trades of sugar-boilers, or makers of sugar, refiners of sugar, and confectioners; not to mention the great addition the concreted juice of the sugar-cane brings to the apothecaries profession, upon the score of syrups, conserves, electuaries, and other saccharine medicines. Nay, a very slight manual contrivance or operation, if it light fortunately, may supply men with a trade, as in the art of printing. To which I shall only add, that in *Cbina*, and some other eastern parts, the lucky trial that some made to bore very small holes through *Porcellane* or *Cbina* cups, and employ very slender wire instead of thread or silk, has given being to the vulgar trade of those people, that go up and down in those countries, as tinkers do with us, getting their livelihood by sewing together the pieces of cracked or broken *Porcellane* vessels; as I have been informed by more than one credible person that lived in the East, and had experience of the use of cups so mended, though filled with liquors as hot, as they are wont in the East to drink their coffee and tea.

THE mention freshly made of *Cbina* brings into my mind, that whereas the knowledge of some gums and liquors in that country afforded those useful as well as most beautiful varnishes, which we call by the name of the kingdom that supplies us with them, and

which

which do both there, and in *Japan*, employ multitudes of tradesmen; I am credibly informed, that the art of making the like varnished wares is now begun to be a trade at *Paris*, and I doubt not but it will before long be so in *London* too. For though some accounts, that were given me by virtuosi, of that varnish, were such, that the trials of them did very ill answer expectation; yet having read in *Linfboten's* voyages, that in *China* and *Japan* they make this excellent varnish of gum lacca, I found by some trials, that I was able to imitate one of the best sorts of it, by dissolving the gum in high rectified spirit of wine \*, and then giving it a colour, and laying it on in such a manner, as I may have before long a fitter occasion to inform you.

AND without much impropriety, I might alledge the art of cultivating and gathering sugar-canes, and of ordering their juice, as a recent instance of the transplanting of arts and manufactures. For, as I am informed by very credible relations, there are not very many years effluxed, since, in our memory, a foreigner accidentally bringing some sugar-canes, as rarities, from *Brazil* into *Europe*, and happening to touch at the *Barbadoes*, an English planter, that was curious, obtained from him a few of them, together with some hints of the way of cultivating and using them. Which, by the curiosity and industry of the English colony there, were in a short time so well improved, that that small island became, and is still, the chief storehouse, that furnishes not only *England*, but *Europe*, with sugars. And this instance I the rather mention, because it is also a very notable one, to shew how many hands the introduction of one physico-mechanical art may set on-work; since I have had particular opportunity to learn by enquiry, that the negroes, or, as they call them, blacks, living as slaves upon that spot of ground, and employed almost totally about the planting of sugar-canes, and making of sugar, amount at least to between five and twenty and thirty thousand persons. And, that you may see how luciferous in that place this recent art of making sugar is, not only to private men, but to the publick; I shall add, that by divers intelligent and sober persons interested in the *Barbadoes* (and partly by other ways) I have been informed, that there is, one year with another, from that little island, which is reckoned to be short of thirty miles in length (and so I found it, by measuring it on one of the fairest and recentest maps) shipped off for *England*, especially, ten thousand ton of sugar, each ton being estimated at two thousand pounds weight, which amounts to twenty millions of pounds of that commodity; which, though it may seem scarce credible, yet one of the ancient magistrates of that island lately assured me, that some years it affords a much greater quantity.

I SHALL not fortify what I have hitherto discoursed with particulars that will elsewhere more properly fall in; it being sufficient for my present purpose, that the instances already mentioned may render it probable, that the experimental philosopher may not only improve trades, but multiply them, till I have occasion in the last essay of this book, to make it out more fully. Nor do I despair, that among other ways, whereby trades will be increased, one may be the retrieving some of those, that were anciently practised, and since lost; of which we have a catalogue in the learned *Pancirollus*. For as it is the skilful diver's work, not only to gather pearls and coral, that grew at the bottom of the sea, and still lay concealed there; but also to recover shipwrecked goods, that lay buried in the seas, that swallowed them up: so it is the work of the experimental philosopher, not only to dive into the deep recesses of nature, and thence fetch up her hidden riches; but to recover to the use of man those lost inventions, that have been swallowed up by the injuries of time, and lain buried in oblivion. This I do not say altogether ground-

\* See the Appendix to Essay V.



lessly, though for some reasons I here decline mentioning the things, that induced me to say it.

SECTION VI.

To what has been hitherto said I shall venture to add, not only, that the sagacious philosopher may better most of the trades that are already in use, and add to the number of mechanical employments; but that I am apt to think it might, without much hyperbole, be affirmed, that there is not any one profession or condition of men (perhaps scarce any single person of mankind) that may not be some way or other advantaged or accommodated, if all the truths discoverable by natural philosophy, and the applications that might be made of them, were known to the persons concerned in them. So that besides those discoveries that are compiled or formed into trades, there are, and may be found a multitude of loose particulars, whereby the naturalist may much gratify and assist men, according to the exigency of particular occasions. The nature of the thing will scarce permit me to illustrate so unlikely an assertion, without employing instances in themselves trifling, if not despicable; of which I will therefore give you but a few, because, if they were not pertinent to my present purpose, they would be fitter to divert than inform you.

I HAD, not long since, the honour to be known to a very great court-lady, who was much troubled, that having frequent occasion to write letters, she could scarce handle a pen without blacking her fingers with ink. I smilingly undertook to make her write without ink, which I myself was formerly wont to do, by first preparing my paper with a powder made of copperas, slightly calcined upon a fire-shovel, till it grow friable, and galls, and gum-arabick finely pulverized, and exquisitely incorporated with the vitriol in a certain proportion; which, though a few trials will better teach than rules (because, according to the goodness and calcination of the vitriol, the proportion of the other ingredients must sometimes be varied) yet to assist you in your first guesses, I shall tell you, that, for the most part, I used myself three parts of calcined vitriol, two parts of galls, and one part of gum-arabick, and mixed them not before I was ready to employ them; for this powder being with a hare's foot, or any other convenient thing, carefully rubbed into the paper, and the looser dust struck off, doth, without discolouring it, so fill its pores with an inky mixture, that, as soon as it is written upon with a clean pen, dipped in water, beer, or such other liquors, the aqueous part of the liquor dissolving the vitriolate salt, and the adhering particles of the galls, makes a legible blackness immediately discover itself on the paper. This mention of writing brings into my mind, that several times having had occasion to make a word or two, that was but lately written, look as if it had been written long before, I performed it, by lightly moistening the words I would have to look old with oil of tartar per deliquium, allayed with more or less fair water, according as I desired the ink should appear less or more decayed; which experiments may be often useful in manuscripts, to keep the recent interlineations or other additions from betraying themselves by their freshness not to have been written at the same time with the rest of the manuscript.

And the design I had in making use of the lately mentioned powder of galls and copperas puts me in mind of another way of writing without ink (and too without danger of blacking one's fingers or linen) which I remember I have practised sometimes with one powder, and sometimes with another. For considering, that common silver being rubbed upon bodies, whose surfaces are a little rough, and even upon coloured cloth,

the metal would leave a blackness on it, it was easy to conclude, that if the surface of the white paper were operated by a multitude of irregular grains of a powder as white as it, would retain a blackness, wherever a blunt silver bodkin should be drawn over the grating particles: and accordingly I found, that either exquisitely calcined hartshorn, or clean tobacco-pipes, or (which is better than that) mutton-bones (taken between the knuckles, and) burnt to a perfect whiteness, being finely powdered and searfed, and well rubbed upon paper, would make it fit to be written upon with the point of a silver table-book pin, or bodkin of silver (which metal is not absolutely necessary in this case,) as well as that, which is called mathematical paper, (if the being prepared with one, or other of these powders, do not make it the same.)

AND now I am upon the mention of such preparations of paper, I remember, that I was once in a place, where I could get no white leaves, to supply a fine table-book that I had much use for; nor could I hear of any tradesman in the whole country, that knew the way of making so much as ordinary table-books: wherefore I bethought myself of trying to make something by way of succedaneum, which succeeded at the first attempt. And though there may be better ways to make white table-books, yet perhaps you will find none more simple and easy; the two only ingredients we had in it, being to be had at every apothecary's shop. I only take cerus, rubbed to very fine powder, (which is done in a trice) and temper it up with fair water glutted with clear gum-araback. With this mixture (being brought to the consistence a somewhat thick falve) I rub over the paper I prepare, putting on more or less, according as I would have it last; and having suffered it to dry (which it will quickly do) it may, if there be occasion, be presently used with the point of a silver-pin, which will make the letters appear very conspicuous upon a mixture, that does not at all impair the whiteness of the paper; and what was thus written I could, with spittle or water, blot out three or four times successively without spoiling the paper. Which questionless had been much better prepared, if divers couches of the mixture had been laid on, and suffered each to dry, and if afterwards the paper had been smoothed by being scraped with a knife, and polished.

A VERY ingenious artificer, who had contrived an instrument useful to others, and profitable to himself, whereof an absolute necessary part was a glass filled with fair water, and exactly stopped, complained to me, that though his instrument did exceeding well in all but frosty weather, yet then it was apt to be spoiled by the freezing of the included liquor, which too often broke the glass. Whereupon I taught him to remedy it, by substituting, instead of water, good spirit of wine, which has not in our climate been observed to freeze; or rather, (because in his bigger glasses, that liquor would be chargeable) either sea-water strengthened with a little salt, or else common spring-water with a twentieth, or at most a tenth part of salt dissolved in it. For though this brine look, if well made, as clear as common water, yet I have not observed, that the sharpest of our English winters would make it freeze.

To a person of quality, that was very curious of the way of writing secretly, I undertook to teach an easy way (which after I knew it, I found also in an old printed book) of sending a written message, without putting it into the power of the bearer to betray it; which I could easily have performed myself, if the message were to be delivered in a short time, and not too far off, by writing on his back, or other convenient part of his body, with a clean pen dipped in my own urine, (there being some urines, with which I have found, to my wonder, that the experiment would not succeed.) For if he, that receives the message, rubs but a little of the black substance remaining of paper after it is burnt, those false parts adhering to those other of the liquor that lurk yet in the pores of the



the skin (whence, if the messenger went fast, and very fast, the sweat would probably dislodge them) do denigrate all that was written, and make it legible enough, sometimes, as I have tried, after many hours.

I REMEMBER too, that intending one summer to make some abode at a house I had in the country, I sent for from *London*, among other things, a quantity of damask table-linen, with which he that sent it me, inconsiderately packed up a great pot of a certain confection, which, for some purposes, I had caused to be made of the pulp of sloes, which, by agitation of the horse it was carried on, being brought to ferment, and run out of the broken pot, stained all the new damask from the top to the bottom. At which an old domestick of mine (whom you remember very well) seeming much troubled, because he had sent for it; to convince him, that experimental philosophy was not altogether useless, I steeped the stained linen, for some convenient hours, in new milk; and afterwards causing it to be thoroughly and diligently washed in the like liquor, the damask came forth unstained, and almost as white as it. What urine, if duly and long enough employed, may do to take stains, even of ink, out of linen, is but to be hinted in this place; where I might add, that with strong spirit of salt, wherewith I moistened, as often as was needful, the spotted places (first wetted with fair water) I have out of new linen taken spots of ink (especially fresh ones) of very differing sizes, without leaving (after the linen was well washed out in fair water) any of those yellow stains which many call iron-moles.

SOME ingenious persons, that deal much in lixiviums and brines, complaining the other day, that besides that they could not sometimes easily come at an egg, to try, by its sinking or floating, the strength of the saline liquors they would examine, there needed a good quantity of liquor to make such a trial in, I allowed their complaint to be just, and the rather, because I observe, for nicer estimates of the strength of liquors, the trial by eggs is uncertain enough, in regard, that even the same egg will, as I have found, by being kept, grow lighter, whence stale eggs have usually a great cavity (that seems filled only with air) at the bigger end: and I told them, to omit the more artificial, but more difficult, ways of examining such liquors, I sometimes used a way, whereby I could try the strength of the lixiviums made with chymical salts, though I had not above a thimbleful of the liquor, and this with a body, that will not easily waste like an egg, and therefore may be kept. For I substituted, instead of the egg, a small piece of amber, about the bigness of a pea, which in a very strong solution of lixivate salt will, as I let them see, swim on the top, but sink in a weak one. And as you may take a piece of amber, less or bigger than a pea, as best fits your occasions, and need not be at all scrupulous about the figure, (provided the amber be once well ducked in the liquor; so it is some convenience, that two pieces of amber, whereof the one is far more reddish, and the other paler, will be, as far as I have tried, of somewhat differing specifick gravities, so that the one will float in some liquors, wherein the other will sink.

I REMEMBER I was once in a country, where I had a great mind to try some things with Dantzick vitriol, or some other blue copperas, but, by reason of the wars, could not possibly procure any, though there were in that country a place, where green vitriol was made by the help of iron: wherefore getting some of that liquor, which the rain had washed from the copperas stones, I did, by putting into it a convenient quantity of copper, reduced into small parts, make the newly mentioned liquor, serve for a menstruum to work upon the metal, and by exhaling the solution to a due consistence, I obtained the blue venereal vitriol I desired. And the like, I doubt not, may be done with such



of those common green vitriols made of iron, wherein the saline part is not too much satiated with the martial.

AN ingenious and well known person, that is a great dealer in cyder, coming to visit me, and expressing a great desire to be able to make some that would be stronger, and thereby likelier to keep longer than the ordinary way, I extempore directed him to an unusual course, for which he afterwards came to give me solemn thanks. The way was to take the strained juice of apples, and in ten or twelve gallons thereof to steep for 24 hours (more or less) about two bushels of the same kind of apples grossly bruised: the apples being lightly expressed, the infusion was (with fresh) repeated once more, (care being to be taken, that the infusion be not made too strong and thick, which may hinder the seasonable clarification of the liquor.)

It was not perhaps difficult to mend this prescription; but I give you the account of it, as I received it from him, because he assured me, that none of his many trials had furnished him with cyder so well bodied, and so much applauded. The cautions that belong to this practice, and the various applications that may be made of this way of making vinous liquors of fruits, without additions (so much as of water,) by infusion, and the varyings of the experiment according to particular cases, I must not here stay to mention.

It was not long since, that accidentally rummaging in a dark place, where I had not of a long time been, and where unknown to me some chymical glasses, negligently stopped, and not written on, had been put; one of them falling down made two or three great stains in the conspicuousest part of a new suit I had then on; and would have obliged me to leave it off, but that judging by the nature of the stain, that it was made with some acid spirit, I tried, by smelling to them, whether among the other bottles, one or other had not some urinous, or otherlike spirit; and lighting on a liquor, which, though I know not what it was, I guessed by the stink, to abound with volatile salt, I bathed the stained parts well with it, and in a trice restored them to their former colour. And, by a like way, I have presently remedied the discolorations made by some sharper and fretting liquors, of died garments of other sorts and materials, which those blemishes would else have rendered altogether unfit for wearing.

ANOTHER time, discoursing with a statesman of the ways whereby well-meaning persons may be injured and defamed, I undertook, that out of a parchment-writing, with his hand annexed, I would take out all that was written above his name, without spoiling or disfiguring the parchment, on which I would afterward write what I pleased, and whereby I make people believe, that he had acknowledged under his hand such things, as never came into his thoughts. And to satisfy him of the possibility of this, I did in a few minutes take off from the parchment all that was written on it, without defacing the parchment. Some attempt to free paper from what is written upon it with aqua fortis, but, that by discolouring the paper, make men apt to suspect some intended deceit. And for the true way of performing such an effect, and divers others of the like nature, which I have sometimes for curiosity prosperously experimented, I think it much fitter to be concealed than communicated; because if such secrets should fall into the hands of persons inclined to mis-apply them, they might very much disturb human society. And therefore it is better men should want the light afforded them by such experiments, than be brought into the danger of such mischiefs, as they may be made to suffer by the misemployment of such discoveries.

I REMEMBER, that not long since, a virtuoso happening to have made a solution of gold, wherewith he thought to make aurum fulminans, thought he had cause to suspect, that it had been enbaled with copper, and therefore would not be so fit for his work;  
where-



whereupon I considered with myself, that a good urinous spirit being employed instead of the usual menstruum (oil of tartar,) as it would precipitate gold out of aqua regis, so it would readily dissolve copper, I conjectured, that by the affusion of such a liquor I might both discover, whether the solution (whose colour did not at all accuse it) contained any copper, and if it did free the gold in great part from the baser metal: and indeed I found, that after the urinous spirit had precipitated the gold into a fine calx, the supernatant liquor was highly tinged with blue, that betrayed the alloy of copper, that did not before appear.

I HOPE you think, *Pyrophilus*, that it is because these instances are more pertinent to my design, than many others (that might have been substituted) in themselves more valuable, that I have mentioned such inconsiderable ones; and I shall not repent the naming of such instances, if they have let you see, that even mean experiments are not to be despised, but, that the meanest may be sometimes, not only useful, but more proper to convince strangers to natural philosophy of the manifold uses of it, than experiments of a higher and abstruser nature. For as in a shipwreck, it may more advantage the distressed pilot to know the supporting nature of a bladder filled with wind, though otherwise but a despicable and airy thing, than to know the abstrusest properties of the magnetick needle; so, in some cases, the more obvious and slight experiments may be much more welcome and serviceable to us, than others at other times much more considerable. So true is that of the wife man, That every thing is beautiful in its season.

For my part, I am very apt to hope, that natural philosophy will prove more and more serviceable, both to single persons in their particular occasions, and to trades themselves in general; as by other ways, so especially by making a further search into, and thereby detecting new qualities, or discovering unheeded uses, of the productions of nature, and of art, that are already known.

I WILL not here take notice of what may be further hoped for in the detection of medical virtues of things, because I treat of that subject in a more proper place: and as for the mechanical uses (if I may so call them) and applications of the works and laws of nature, though he that gazes upon the seemingly great variety of productions to be met with among tradesmen, and in the shops of artificers, may be tempted to think, that art has curiously pryed into, and employed, almost all the materials that nature could afford it; yet he that shall more narrowly and severely consider them, may easily discern, that tradesmen have really dealt with but very few of nature's productions, in comparison of those they have left unemployed; and, that for the most part, they have, in the things they daily converse with, scarce made use of any other, than the more obvious qualities of them; besides some few more lurking properties, which either chance, or a lucky sagacity, rather than inquisitiveness or skill, discovered to them. And indeed this great variety of productions we have mentioned, proceeds more from a manual dexterity of diversifying a small number of known things into differing shapes, than either from the plenty of natural or artificial productions they work upon, or any diligent or accurate search made into the qualities of those productions. But because, to a considering man, it cannot but be obvious enough, that the uses of the things they deal in, and much more those of other concretes, which they are not engaged to observe, have not been hitherto sufficiently enquired into; I shall content myself to add, that if men were but sensible enough of their own interest, and in order thereunto would keep their eyes heedfully open, partly upon the properties of things, and partly upon the applications that may be made of those properties, to this or that use in human life, they might not only discover new qualities in things, (some of which might occasion new trades,) but make such uses of them, as the discoveries themselves would never before-  
hand

hand have suspected or imagined: whereof I may, God permitting, give you elsewhere divers instances.

## S E C T I O N VII.

AFTER the foregoing general considerations (about the usefulness of natural philosophy to the empire of man over things corporeal,) which I thought fit to take notice of in this first essay, it remains, *Pyrophilus*, that I also add a word or two about those that are to follow.

AND first you must not expect, that I should methodically enumerate, and particularly discourse to you of all the grounds, and motives I may have of looking for great advantages to accrue to mankind by men's future progresses in the discovery of nature. To entertain you with considerations, which perchance you would judge but speculative and remote conceits, would exceed my leisure, and perhaps be unwelcome to you; and therefore I choose to confine myself to the insisting on those grounds of expectation, which I can render probable by examples and instances of what is already actually attained to, or at least very likely (in no long time) to be so. And this advertisement I thought necessary to premise, partly indeed, that you may not think, that I have overlooked all the particulars pertinent to my subject, that I shall leave unmentioned, but much more, that you might not suspect, that there are no other inducements to hope much from experimental philosophy, than those you will find treated of in the following essays. And this one thing in particular I dare not forbear to give you notice of, that for the freshly intimated reason, you will there find omitted one of the principal grounds of hoping great matters from improved physiology; namely, that by the sagacity and freedom of the lord *Verulam*, and other lights of this age, considering men are pretty well enabled both to make discoveries, and discern a possibility of removing all the impediments, and other causes of barrenness, that have hitherto kept physicks from being considerably useful to mankind; such as many false and fruitless doctrines of the schools; the prejudices by which men have been hitherto imposed on about substantial forms, the unpassable bounds of nature, the essential difference betwixt natural and artificial things, &c. a too plausible despondency; a want of belief that physicks much concerned their interests; want of encouragement; want of natural history; want of curiosity; want of a method of enquiring; want of a method of experimenting; want of physical logick; want of mathematicks and mechanicks; want of associated endeavours; to all which but too many other particulars might be added.

2. You will not think it strange, that in the following tracts much of the usefulness, for which I would recommend physicks, supposes future proficiency in them, if you consider the nature of my design; which is not to make an eulogium of natural philosophy, imperfect as it yet is, but to shew, that as it may be, and probably will be, improved, it may afford considerable advantages to mankind. And since, as I long ago intimated to you, my purpose in this book is to invite you, and assist you to invite other ingenious men, to a farther study of nature, it is very agreeable to my design, to represent the greatest benefits I make it promise you, as effects and recompenses of your future attainments: and I should allowably enough discharge my part in this treatise, if I should not do any more (which yet I hope I shall do) than give you reasonable inducements to entertain high expectations of the fruits, that may be gathered from natural philosophy, if it be industriously and skilfully cultivated: and the very rendering such an expecta-



expectation probable, I take to be a good step towards the attainment of the things expected; many of which would questionless be obtained, if men were thoroughly persuaded, that they are most worthy to be endeavoured, and very possible to be compassed. And therefore I wonder not, that so judicious a friend to philosophy and mankind, as Sir Francis Bacon, should in several places represent men's opinions of the impossibility of doing great matters of the nature of those things we are speaking of, as one of the chief obstacles to the advancement of real and useful learning; and I the rather insist on the things, that may heighten your expectations, not only because many prudent and learned men, who have been bred in the philosophy of the schools, are apt to judge of all philosophy by that, which for so many ages has been barren, as to useful productions, (though fruitful enough in controversies,) but because I have met with some morose authors, and others as despondent persons, who, because they have unsuccessfully attempted to perform things according to the prescriptions of some unfaithful writers of natural philosophy, fall presently to believe themselves, and to persuade others, that nothing considerable is now (at least without almost insuperable difficulties) to be performed by natural philosophy itself, especially, whilst men amuse themselves about speculations and trials, that seem not to tend directly to practice; our ancestors having had the luck to light upon all the profitable inventions, which skill in physiology is able to supply mankind with. But (to take notice first of what was last suggested) I make no doubt, but that many experiments, whereby men are not presently enabled to do what they could not before, may yet be very useful to men's interest, by discovering or illustrating the nature or causes of things. For though that famous distinction, introduced by the lord Verulam, whereby experiments are sorted into luciferous and fructiferous, may be (if rightly understood) of commendable use; yet it would much mislead those, that should so understand it, as if fructiferous experiments did so merely advantage our interests, as not to promote our knowledge; or, the experiments called luciferous, did so barely enrich our understandings, as to be no otherways useful. For though some experiments may be fitly enough called luciferous, and others fructiferous, because the more obvious and immediate effect of the one is to discover to us physiological truths, and of the other, to enable us to perform something of use to the professor; yet certainly there are few fructiferous experiments, which may not readily become luciferous to the attentive considerer of them. For by being able to produce unusual effects, they either hint to us the causes of them, or at least acquaint us with some of the properties or qualities of the things concurring to the production of such effects. And on the other side those experiments, whose more obvious use is to detect to us the nature or causes of things, may be, though less directly, and in somewhat a remoter way, exceeding fructiferous. For since, as I have formerly observed, man's power over the creatures consists in his knowledge of them; whatever does increase his knowledge, does proportionately increase his power. And perhaps I should not much hyperbolize, if I should venture to say, that there is scarce any considerable physical truth, which is not, as it were, teeming with profitable inventions, and may not by human skill and industry, be made the fruitful mother of divers things useful, either to mankind in general, or at least to the particular discoverer and dexterous applier of that truth. To countenance this opinion of mine, I have already given you some instances, and reserve more for the last essays of this treatise; especially having observed it to have been a fault, which, though prejudicial enough to the interest of mankind, is very incident to the more sober and severe sort of philosophers, and perhaps more to them, than to others, to conclude every thing to be impossible, or, at least, unfit to be attempted, that cannot

cannot be performed by the already known qualities of things and ways of applying them; without considering, that as many simples of excellent virtues grow in wildernesses, and not by the highway's side, so divers admirable properties of things may be found, out of the customary progress, or beaten roads (if I may so speak) of nature; and that philosophers are oftentimes deceived, when they think they may have made a true and perfect analysis of the possible ways, whereby such and such effects may be produced. For nature by her subtlety oftentimes transcends and illudes the greatest subtlety of human ratiocinations. And as she may have quite other ways of working than we are aware of, so the knowledge of some peculiar and concealed property of a thing may enable them, that are acquainted with it, to perform that with ease, which, by the known qualities of things, is either not at all to be performed, or not without great difficulty.

Nomb.  
xiii. 14.

THIS seeming paradox you may find in due place confirmed; and in the mean while, to return to those learned men, who having attempted some things, and possibly performed a few in natural philosophy, would keep the world from expecting any great matters from it, I shall venture to say of them, that as the Jewish spies, though they brought their countrymen out of the land of *Canaan*, some few of the goodly fruits of that soil, yet bringing them withal a discouraging account of the difficulties they were like to meet with in conquering it, did the *Israelites* more harm by their despondency, than good by their fruits; so divers of the authors we are speaking of, though they may have presented us with some acceptable fruits of their enquiry into experimental learning, yet by bringing up an ill report concerning the study of it, and thereby deterring irresolute persons from addicting themselves seriously to it, they have more prejudiced them by their despondency, than advantaged them by their experiments. And though I dare not, a chymist would not, scruple to pursue the simile, and tell you, that as only those two of the spies, *Caleb* and *Joshua*, who made no doubt but that they should conquer the fertile (though never so well fortified) land of *Canaan*, did really possess it, all their disanimated brethren wandering and dying in the wilderness; so none but those generous attempters, that dare boldly venture upon the difficulties that surround the knowledge of nature, are like prosperously to overcome them, and possess what they contend for.

Nomb.  
xiv. 28,  
29, 30.

BUT I must leave this digression to proceed to the last advertisement I am to give you, which is, that I know you may possibly expect, that I should say something to you distinctly of the chief means, by which the naturalist may probably advance trades, and assist man, by the blessing of the author of nature, to recover part of his lost empire over the works of nature. And I confess, I have more than once had thoughts of a kind of project (if I may so call it) for the advance of experimental philosophy, consisting of such heads as these: a prospect of what probably may be attained to in physics (both as to theory and practice.) A summary account of what is attained already. The imperfectness of our present attainments. What helps men now enjoy. The incompetency of our present helps. The hindrances and the causes of them. And the means and helps that may be employed. To which other heads might in case of need be added. But notwithstanding the expectations you may have, that I should handle such subjects, and the thoughts I have had about them; I purposely waved the treating of them by themselves in the ensuing essays, partly, because these unelaborate discourses are not designed for a just treatise on the subjects handled in them, containing but such loose experiments and observations, as could, without too much impoverishing other papers, be put together on this occasion; and partly, because I have in effect



effect been careful to mention several of those things, that you might expect to find separately treated of; but knowing, that a far less discerning eye than your's may easily, if there be occasion, distinguish them, I thought it more convenient to interweave them with the other parts of the following discourse, since every proposition of a probable way to improve philosophy is also a ground of expecting those advantages, that may be hoped for from philosophy improved.

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OF THE  
USEFULNESS  
OF  
MATHEMATICKS  
TO  
NATURAL PHILOSOPHY.

OR,

That the Empire of MAN may be promoted by the Naturalist's Skill in MATHEMATICKS, (as well pure, as mixed.)

IF it were not allowable for any but those, that are thoroughly skilled in the abstruser mysteries of the mathematicks, to discourse of those disciplines, the title of this essay, would, I fear (*Pyrophilus*) make you think me guilty of presumption, since you may perchance remember, that when you were conversant about those studies, I confessed to you, that the great authority of some famous modern naturalists had, for a while, diverted me from making any great progress in those sciences, by their resolute denying them to be useful to physiology. But, as I do not pretend to have taken that pains, which else I might have done, to become a speculative geometrician; so I consider, that without understanding as much of the abstruser part of geometry, as *Archimedes*, or *Apollonius*,  
Vol. III. 3 H

*Apollonius*, one may understand enough to be assisted by it in the contemplation of nature; and that one needs not know the profoundest mysteries of it, to be able to discern its usefulness. And therefore I shall venture to propound something to you concerning this last-named subject, especially, since otherwise you may be influenced, as I once was, by the great authority of those modern philosophers, who would have the use of mathematicks, as disciplines, that consider only abstracted quantity and figure, to be rather hurtful than advantageous to a naturalist, the object of whose studies ought to be matter. But though these endeavour to keep men from thinking the mathematicks to be of any great use toward making a man a good naturalist, by alledging the extravagant opinions that *Kepler* himself, who was mathematician to three emperors, and some other modern astronomers, have broached or maintained concerning matters physiological; yet I confess, that after I began, by reflecting upon divers of my experiments, especially mechanical, to discern how useful mathematicks may be made to physicks; I have often wished, that I had employed about the speculative part of geometry, and the cultivating of the specious Algebra I had been taught very young, a good part of that time and industry, that I spent about surveying and fortification (of which I remember I once wrote an entire treatise) and other practice parts of mathematicks. And indeed, I think, that a competent knowledge in mathematicks (for a profound one is not always necessary) may be so serviceable to those that would become philosophers, that I shall not scruple to mention it as another thing, which may increase your expectation from physiology, that those, who pass for naturalists, have, for the most part, been very little, or not at all, versed in the mathematicks, if not also jealous of them. And I the less scruple to write to you on this subject, because I do not know that others have prevented me; for though the learned *Clavius*, and some other expositors of *Euclid*, have said much of the usefulness of geometry to other mathematical disciplines; and though not a little has been said in the praise of mathematicks in general; yet it is left free for me to discourse to you of (what is the subject of this essay) the utility of mathematicks, in reference to modern physicks, and therein not only to the notions of the corpuscular philosophy, but even to practical and experimental knowledge.

Now there are several scores, upon which skill in mathematicks may be useful to the experimental philosopher. For there are some general advantages, which mathematicks may bring to the minds of men, to whatever study they apply themselves, and consequently to the students of natural philosophy; namely, that these disciplines are wont to make men accurate, and very attentive to the employment they are about, keeping their thoughts from wandering, and inuring them to patience of going through with tedious and intricate demonstrations; besides, that they much improve reason, by accustoming the mind to deduce successive consequences, and judge of them without easily acquiescing in any thing but demonstration.

AND indeed the operations of symbolical arithmetick (or the modern Algebra) seem to me to afford men one of the clearest exercises of reason that I ever yet met with, nothing being there to be performed without strict and watchful ratiocination, and the whole method and progress of that appearing at once upon the paper, when the operation is finished, and affording the analyst a lasting, and, as it were, visible ratiocination.

BUT, *Pyrophilus*, I may not insist on these, or the like general uses of pure mathematicks, since there are divers others, which more immediately respect natural philosophy.

AND to shew this the better, give me leave to premise to the following particulars, a couple of observations.



THE first is, that the phænomena which the mathematician concurs to exhibit, do really belong to the cognizance of the naturalist. For when matter comes once to be endowed with qualities, the consideration how it came by them, is a question rather about the agent or efficient, than the nature of the body itself. So the image or picture that a man sees of his face in a looking-glass, though that be an artificial body, falls as well under the speculation of the naturalist, as when the like picture is presented him by calm and clear water. And the rain-bows, that are often artificially made in grottos, by dispersing the water of fountains into drops and showers, have a just title to his contemplation, as well as the rain-bow that is formed in the clouds. And the echoes, that are admired in some of those grottos, purposely and artificially contrived to afford rare ones, do as well belong to his cognizance, as those that nature makes in ruder dens, and other cavities of hills and mountains. And indeed most of those phænomena require (for the main) the same solutions, whether the skill of man do or do not intervene to exhibit them.

THE second consideration, which I am often obliged to repeat, is this; that since man's power over the creatures depends chiefly upon his knowledge of them, whatever serves to increase considerably his knowledge, is likely, either directly, or in its consequences, to add to his power; which two advertisements being thus given you, *Pyrophilus*, I now advance to the particulars, whose mention they made me suspend.

I. AND first, these disciplines teach men the nature and properties of figures, both upon surfaces and solids, and the relations (for they can scarce be properly called proportions) betwixt the surface and solidity of the same body. It is true, that matter, or body, is the subject of the naturalist's speculations; but if it be also true, that most, if not all the operations of the parcels of that matter (that is, of natural bodies) one upon another, depend upon those modifications, which their local motion receives from their magnitude and their figure, as the chief mechanical affections of the parts of matter; it can scarce be denied, that the knowledge of what figures are, for instance, more or less capacious, and advantaged or disadvantaged, for motion or for rest, or for penetrating or resisting penetration, or for the being fastened to another, &c. must be of considerable use in explicating many of the phænomena of nature; and it is sufficiently known, how much of the doctrine of figures may be learned from geometricians, who treating expressly and copiously of triangles, circles, surfaces, elliptical, parabolical, hyperbolical, and other plain figures; as also of spheres, cones, cylinders, and especially prisms, pyramids, cubes, and regular bodies, intimate also the methods of judging of the figures of other bodies, that are either composed of them, or may, by reason of some analogy, be referred to them.

THERE are divers properties, as well of planes and solid figures, and their habitudes to each other, as of such lines as are described by motions, or wherein motions may be made; the knowledge whereof may be of good use not only to the speculative naturalist, but the practical.

To know the proportion that *Archimedes* has demonstrated to be between a sphere and a cylinder, and either of those to a cone so and so qualified; or to know that a triangular pyramid is the third part of a prism, having the same base and height; and, in a word, to know the proportions between geometrical bodies, may sometimes be of good use, in cases where we can procure the one and not the other, or at least not so well as the other. Of this an instance is given us by the ingenious *Morinus Ghetaldus* (as I find him cited by a late mathematician) who tells us, that *Ghetaldus*, finding it very difficult to procure an exact metalline sphere, wherewith to examine the proportion, in point of weight, between heavy bodies of the same bulk, found, that yet he could get



a cylinder of tin to be turned true; and having therewith made his experiments or observations, it was easy for him, knowing out of his *Archimedes*, that the proportion of a cylinder, whose basis is equal to one of the great circles of a sphere, and whose height is equal to the diameter of that sphere, is to that sphere *in ratione sesquialterâ*, as they speak, i. e. has the same proportion that three has to two; it was, I say, easy for him, who had often had occasion to weigh his cylinder exactly, by subtracting a third part of the whole weight, to find in the remainder the desired weight of a sphere of tin, whose diameter was equal to that of the basis, or to the height of the cylinder\*: which weight of a sphere of a known diameter being once obtained, he deduced from them the weights of the other spheres he had occasion to employ about the construction of those tables, which have been much made use of by divers succeeding mathematicians. And what applications I have made of the same Archimedean theorem, I may elsewhere inform you.

It being also taken for granted by divers modern geometricians and engineers, that the excellent *Galileo*, and his not degenerate disciple *Torricellius*, had demonstrated the line, which a heavy body, projected, and even the bullet, shot out of a cannon, describes, to be parabolical; it may be of moment in the practice of gunnery, and in reference to divers experiments to be made with other projected bodies, to be well versed in the nature of the parabola and parabolical lines, which are also thought to be capable of doing wonders in burning-glasses, in case these metalline specula can be brought to a parabolical figure; one of whose remarkable properties is, that all the beams that, being parallel to the axis, fall upon the internal superficies, are reflected to one point or focus; where consequently, if the burning-glass be any thing large, the heat must be very intense, especially in comparison of a spherical burning-glass of the same bigness.

AND as for delightful and recreative experiments, you will easily allow me, that there are abundance of catoptrical ones of that sort, which depend upon the figure of spherical, cylindrical, and other sorts of reflecting glasses.

2. I MIGHT here tell you, *Pyrophilus*, that pure mathematicks themselves, setting aside the assistance they are wont to give to mixed mathematicks, may be of use to human life, and to the experimental naturalist; of which I shall give you, as a specimen, this notable example.

THE properties of arithmetical and geometrical progressions in numbers seem to have very little to do with the practice of weighing out things in shops and warehouses. And yet by the knowledge of the double progression, beginning from an unit (as arithmeticians call that, wherein the consequent is still double to the antecedent) as 1, 2, 4, 8, a great deal of cumber, and sometimes of charge, may be saved. For with three weights you may weigh all the pounds that are from one to seven inclusively; with four weights, all those that exceed not fifteen pounds; upon which observation is grounded the division of some boxes or sets of weights used by our goldsmiths. And if you would, as is very usual, put weights (when there is occasion) in both scales, to help the thing to be weighed to bring the balance to an æquilibrium, then the triple progression (i. e. where the numbers increase in a triple proportion, as 1, 3, 9,) has a much more notable property for our purpose; by considering which, the industrious *Stifelius* concluded, that by three weights you may weigh any number of pounds from one to thirteen inclusively; with four weights, any number of pounds from one to forty inclusively; with five weights, any number of pounds not exceeding sixscore and one; and with but six weights, any number of pounds from one to three hundred and sixty.

\* Archimed. proposit. 32. lib. 10. de sphaera & cylindro.



four. But the method of ordering so few weights to serve so many purposes is best found out by symbolical arithmetick or algebra, by which I have taken pleasure to work so fine a problem; which, because it is applicable, not only to pounds, but to the parts of pounds, and those of differing denominations, it may be of so great use to you, if ever you busy yourself about statical experiments, that I shall to the end of this essay annex a table, to shew what weights are to be taken in every possible case, which I found ready calculated to my hand by the ingenious *Franciscus a Schooten*, professor of mathematicks at *Leyden*.

To the former instance, of the use that an experimenter may make of pure mathematicks, I might, if it could be sufficiently delivered in a few words, add the method of computing the combinations that may be made of any number of things proposed, which some mathematicians call *Regula combinatoria*. For though I remember not to have found this method fully handled in any one author, even among the modern algebraicians; yet, as it is delivered by some arithmeticians, it is by no means to be despised, but, as it may be managed by symbolical arithmetick, it will, if I mistake not, want nothing, but the being skilfully applied by the naturalist, to be on certain occasions very serviceable to him.

3. WE may take notice in the next place, that mathematicks may much help the naturalists, both to frame hypotheses, and judge of those that are proposed to him, especially such as relate to mathematical subjects in conjunction with others.

WHAT wretched theories the ignorance of mathematicks has made naturalists, otherwise very considerable in their way, frame and propose, may be evidently shewn in the accounts that *Epicurus*, and his paraphrast *Lucretius*, give of the sun, and other celestial bodies. And indeed what satisfactory account can be given of the varying lengths and vicissitudes of days and nights, and the eclipses of the sun and moon, the stations and retrogradations observed in planets, and other familiar celestial phenomena, without supposing these great mundane bodies to have such situations in respect to one another, and to move in such lines, or at least to be made to appear to move in them by the motion of the earth in such a position, and in such lines? Nay, how without the knowledge of the doctrine of the sphere will the naturalist be able to make any sober and well grounded judgment in that grand and noble problem, which is the true system of the world? which is endeavoured to be solved after such differing manners by the Ptolemæans and Peripateticks, by the Tychonians and by the Copernicans, both less and more modern.

THAT then the knowledge of celestial bodies is not well to be attained, nor consequently the theories proposed of them, to be intelligently judged of, without arithmetick and geometry (those wings on which the astronomer soars as high as heaven) he must be very little acquainted with astronomy, and particularly with the various and too often intricate theories of planets, that can doubt. And truly, when I consider the astonishing distance and immensity of the celestial bodies, and those almost numberless fixed stars (each of them perhaps much vaster than the whole earth) which in a clear night I take pleasure to gaze at through the better sort of telescopes, both in the milky way, and in other parts of the sky, that seem not so much as whitish to our eyes; I cannot but highly prize a science that acquaints us, that what we know of so much of the universe as the globe we inhabit and call the world, is but a point to it, taking up a little more room in it, than a physical center in the sphere.

THE usefulness also of pure mathematicks to geography is likewise evident: and sure inquisitive men ought not to despise this and the former part of learning, without which,



as I was lately saying, they cannot know so much, as whether the earth we live upon moves or stands still.

THERE are also divers phenomena of nature, that are neither astronomical nor geographical, where the usefulness of mathematicks is manifest enough. For as to the phenomena of that sense, to which the naturalist is most beholding, sight, what a pitiful account is given of them by those Aristotelians, physicians, and other writers, without excepting many good anatomists, that have been strangers to mathematicks, in comparison of what has been done (not to mention *Euclid*, *Albarez*, and *Vitellius*) by *Kepler*, *Scheiner*, *Herrigon*, and some other modern mathematicians.

AND it is evident to those that are acquainted with dioptricks, that without some knowledge, not only of the properties of convex bodies, and of the laws of refraction from and towards the perpendicular (as the masters of opticks speak) but also of the properties of lines, as circular, parabolical, hyperbolical, &c. and figures, as ellipses, circles, parabolas, hyperbolas, &c. it is almost impossible, either well to explicate most of the phenomena of that noblest of our senses, sight itself, or to make a well grounded judgment of others explications of them. He, that is altogether a stranger to this part of mathematicks, will scarce be able to conceive the reason of the admirable fabrick of the eye, and how the crystalline humour does by its convex figure (like a lenticular glass) refract and converge the beams (or at least the pencils) that proceed from the visible object, that they may paint the more lively picture of it upon the retina at the bottom of the eye; nor will he understand why, by reason of the decussation of the beams within the eye, this picture must be made inverted, though we apprehend the objects themselves in a right posture; nor why small objects, placed near the eye, where they are seen under a wide angle, appear as big, as very much greater, that are seen at a greater distance from it. And much less will he be able to understand the reason of those many delusive apparitions exhibited by concave, convex, conical, and cylindrical glasses, the catoptricks, or doctrine of reflex vision, belonging yet more to the mathematicks than dioptricks do.

4. AND since that from the magnitudes of divers bodies, or of several parts of the same body, and so likewise from their degrees of celerity in their motion, there will arise a certain respect, which if they be but two, geometricians call a ratio, and if more than two, a proportion (though these terms are oftentimes confounded, and promiscuously employed by authors; and since proportion is so frequently to be met with in the works of him, who by an eminent, though apocryphal writer, is truly said to have made all things in number, weight, and measure; and since the doctrine of proportion, as such, belongs to the mathematician, as the noblest parts of those sciences he treats of; I think it may safely enough be affirmed, that he, that is not so much as indifferently skilled in mathematicks, can hardly be more than indifferently skilled in the fundamental principles of physiology. Nor perhaps would it be rash to say, that the fifth book of *Euclid's* elements, where the doctrine of proportions is chiefly delivered, may prove more instructive to the naturalist than the fifth book of *Aristotle's* physics. And therefore I do not so much wonder, that *Plato* should, over the gate of his school place an inscription (*ἰδέσθαι ἀγεωμέτρωντος ἰσχύειν*) forbidding the entrance to persons unacquainted with geometry, as unfit to judge of what was there taught.

NAY this, though you may think it strange, is very true, that there are some considerable phenomena of nature, which are so far from being explicable by their causes, that men cannot so much as understand what is meant by them, without some knowledge of the doctrine of proportions. As, for instance, when the teacher of opticks tells us, that the



the increments of light are in *duplicitatâ ratione distantiarum, secundum quas à corporibus recedunt, à quibus primum efficiuntur*; he, that knows nothing of proportions, cannot tell so much as what they mean by this theorem, much less whether or no it be true. And so, when the same proposition is by the diligent *Mersennus* \* applied also to sounds, a common reader would not at all understand him, if he did not add by way of explanation, that if, for instance, the noise of a piece of ordnance be heard a league off, that noise will be four times stronger, if it be heard but at the distance of half a league. Nor will this example itself give such a reader, as we speak of, a clear understanding of the proposed theorem. But a considerable instance in this kind may be afforded us by the noble discovery of the moderns, especially *Galileo*, who observe, that when a heavy body descends through the air, the spaces past through, from the beginning to the end of the motion, are among themselves in a (not double, but) duplicate ratio of the moments or equal divisions of time spent in the fall; which requires the knowledge of what a duplicate proportion is, to be well understood; but it may in some sort be explained, and so noble a phenomenon must not be here omitted, by saying, that *Galileo* affirms himself to have observed, that a brass bullet of 100 pounds will, in the space of one minute of an hour, descend an hundred Florentine cubits (which some reckon to be 180 feet of ours, and consequently, saith *Mersennus*, four cubits in one second, or sixtieth part of a minute; and by adding, that the bullet falls in such a ratio, that the acceleration of the motion is made according to the progression of odd numbers, beginning from an unit, or one; so that if in the first moment of time the weight fall down one fathom, in the second moment it must descend three fathoms; in the third, five fathoms; in the fourth, seven; in the fifth, nine; in the sixth, eleven; and so onward. Whence *Mersennus* gives this rule, to know how far the weight will descend in a determinate time assigned; and by knowing how far it has descended, to calculate how long it was in falling. † *Regula generalis*, says he, *hæc est. Si dentur tempora, & querantur spatia, quadrentur tempora, & habebuntur rationes spatiorum. Si dentur spatia, & querantur tempora, investigetur latus spatiorum, & dabitur ratio temporum.*

DIVERS other instances might be produced, to manifest the requisite need and advantage of some knowledge in mathematicks to a speculative naturalist; but I shall content myself to name one more, viz. that the grand theorem or rule of the staticks, that in the balance, or resembling instruments, the proportion betwixt the equivalent weights, and their distances from the fulcrimentum or prop, is reciprocal (so that it is usual with butchers and other tradesmen, to weigh in the statera, commonly called the stillards, 10 or 20 pounds weight, for instance, hung near the fulcriment, with one pound weight placed on the other side of the beam, at 10 or 20 times distance from it) and many other theorems that serve to explicate the properties of the grand instrument of nature, motion (especially as produced or modified by weight, or equivalent force variously adapted, and applied) cannot well be understood without an insight into geometry, and especially the doctrine of proportions; and how much the knowledge of the principles and theorems of the mechanicks may assist the naturalist, both to explicate many of nature's phænomena, and to try experiments, and work great changes on her productions, men will then more readily confess, when they shall better discern how many of her works are but engines, and do operate accordingly.

5. AND give me leave, *Pyrophilus*, to add in this place, that the doctrine of proportions, as it is the soul of the mathematicks themselves, so it may be of vast, though

\* Harmonic. lib. I. prop. 12.

† Mersen. Harmon. lib. I. Propof. 24. Corollar. 1.



perhaps yet unheeded, use in physiology too; not only as it helps the naturalist (as we have newly seen it does) to understand divers phaenomena of nature, but as it may enable him to perform divers things, which he could not perform without it; of which though I may have occasion to give you hereafter in other papers several examples, yet I shall now mention two or three for illustration sake.

THAT the pendulum is the accuratest instrument that we yet have of measuring short spaces of time, I presume, you do not doubt; and I need not tell you, that he, who would know what length a pendulum must be of, to measure by its swing some determinate space of time, as, for instance, a half second (or half the sixtieth part of a minute) must find it out by trial and observation, if he be not acquainted with the doctrine of proportions; but in case he is versed in that, as well as in the phaenomena of pendulums, he may from the length of one pendulum, that exactly measures a known part of time, without making particular trials and observations, deduce the length of pendulums that will serve to measure other divisions of time. For instance, that diligent observer *Mersennus* assures us, that he found by frequent trials, that a slender string with a pistol or musket bullet at the end of it, whose length comprehending the bullet, was three feet and a half (elsewhere he mentions three feet and a 27th) vibrates second (minutes); this now being taken for granted, and it being a received theorem concerning pendulums alike in all things but length, that the lengths are in duplicate proportion to the times in which their vibrations are respectively performed, or are as the squares of the vibrations they perform in the same time, and consequently, the times are in subduplicate proportion to the lengths of the pendulums; if a man would, as I was saying, have a pendulum that shall vibrate half-seconds, he must not take, as one unacquainted with these things would be apt to do, a pendulum of a foot and three quarters, which is one half the length of that which vibrates a whole second, for such a pendulum would prove much too long for his purpose; nor need he by multiplied observations laboriously find out how much it is too long (which oftentimes for want of a standard he cannot do) but since the proportion between a second and half a second is double, and the proportion betwixt the length of the strings, that are to vibrate these two differing spaces of time, must be duplicate of the proportion of the times themselves, it follows, that the length of the strings must be as four to one, which is the duplicate of the proportion of two to one, and so the length of the shorter string must be but a fourth of that of the longer.

THIS, if it were needful, might be confirmed by a problem of the learned *Ricciolo's*, whereof I shall here give you an example, because I may hereafter have occasion to shew you the farther use of it. Let us then suppose, to avoid fractions, that a pendulum that vibrates seconds, is three entire feet long (as indeed some modern mathematicians tell us it is, and as it may well be according to the measures used in some places). If then you multiply 3600, the square of the vibrations, which are 60, that your three feet pendulum makes in a second, by the length of the pendulum, which is 36 inches, and divide the product, viz. 129600, by 9 inches, the fourth part of the length of the former pendulum; and if lastly, of the quotient (14400) you extract the square root, you shall find it to be 120, that gives you the number of vibrations that will be made in a second by a pendulum of nine inches long, and this root being twenty, which is the double of sixty, you may see, that to make a pendulum that shall vibrate half-seconds, it must be but one quarter so long as that, which vibrates whole seconds. And if I thought you were like to think these rules as strange, as a person wholly unacquainted with the nature of pendulums, and the doctrine of proportions may do; I would invite you to consult experience, as I have purposely done in differing pendulums, that divide a minute into seconds, half-seconds, and quarter-seconds; since though your trials should



not be very nicely made, they may suffice to persuade you, that the above-mentioned rules are either accurately true, or at least true for the main, and therefore true enough to be very useful in many occurrences.

To the above-mentioned instances afforded by pendulums I shall here add but one more, that comprehends many thousands; for the art of composing of that great variety of harmonious tunes, that makes musick so delightful to us, depends upon the doctrine of proportions. And he, that being well skilled in that, knows how to apply it to the notes or words proposed, according to the observations which experience has afforded, of the gratefulness of such and such consonancies, &c. may out of his own head compose a strange variety of new and pleasing tunes, which are so many exercises, that man makes of the power his skill gives him over the bodies, of which his musical instruments consist, and over those which they affect.

6. I KNOW not, *Pyrophilus*, whether I may not reckon amongst the advantages that mathematicks may afford the naturalist, that they will in many cases suggest to him divers new experiments, whereby to vary those, wherein the figures of bodies, the lines of motion, as also numbers, proportions, and the like affections, which the mathematician is wont to treat of, may come into consideration. For it is very likely that those suggested experiments, which either would not be thought on, or could not be skilfully proposed, by a person not versed in mathematicks, may, either immediately, or upon the score of the applications that may be made of them, prove serviceable to men; of which I hope in one of the following essays to give you some instances.

See Essay IX.

I CARE not to mention to you, how great a variety of trials and observations, about the best way of levelling great guns, and the differing distances to which they will carry at such and such elevations, and the lines described by the motion of the bullet, and other particulars belonging to the art of gunnery, have been proposed and tried, upon the hints suggested by geometry's mathematical disciples (especially) and others, because many good men with these fatal arts had been less understood. And therefore I shall rather put you in mind of the great variety of phenomena, which pure mathematicks have helped men to discover and derive from these familiar observations; that a beam of light, passing through differing mediums, is not continued in a straight line, but broken or refracted; and, that in such and such conjunctures of circumstances, the sun or moon will suffer an eclipse that will obscure such a part of the body, and last from such a time to such a time; from which observations of eclipses divers very considerable things have been deduced by mathematicians, not only as to astronomy, but also geography, navigation, and chronology. And he that considers what the doctrine of proportions, and of concords (or, as our musicians call them, cords) and discords, has contributed to the great number of musical instruments that have been actually made, and delightfully practised, and that it may afford the naturalist divers hints applicable to other purposes (which I shall hereafter have occasion to intimate) he, I say, that considers these things, especially if he be also acquainted with ingenious, pleasant, and some of them useful experiments, that have been or may be derived from the observations, that when a beam of light falls upon a body, and rebounds from it, the angle of incidence is equal to that of reflection; that if the superficies of the body be curve, the angle is to be estimated as if it fell upon a tangent to that superficies; that if the beam penetrate the body, and come to it through a thinner medium, it is refracted towards the perpendicular, if through a thicker medium, from the perpendicular; he, as I was saying, that shall consider these things, and wishal, what a great variety of propositions, as well problems as theorems, have been deduced by mathematicians by the help of these few observations, and of as few propositions touching the place of the object seen by the help of specular

and dioptrical glasses, will easily grant, what by so many instances I have been endeavouring to prove.

7. I COME now to the consideration, wherewith I shall conclude this essay, viz. that divers disciplines that are reckoned amongst the mixed mathematicks, are chiefly practical, and may assist the naturalist in making experiments and observations, which he either could not make, or could not make so accurately without them; as may appear, partly by the art of dialling, which teaches how to measure time, and tends chiefly to practice; partly by the art of perspective, which is of great use to represent solids and distances upon a small and plain superficies, and is very serviceable to the limner's art; wherein if scholars and travellers were more generally conversant, the history of nature would be far better adorned with lively representations of plants, animals, meteors, &c. and also by several parts of the art of navigation, and particularly that which they call histriodromia, or the doctrine of the lines, by which pilots make their ships to sail. Now if in these and divers other instances that may be given, it must be acknowledged, that mixed mathematicks may be serviceable to the naturalist, and assist him to promote the empire of man; it ought not to be denied that pure mathematicks themselves, as vulgar arithmetic, geometry, and algebra, may be of use to the naturalist, since it is from those speculative parts of the mathematicks, that not only these other more practical disciplines are derived, but a greater number of those disciplines that are called mixed mathematicks, may, according to what I elsewhere observe, be hoped for. For as sounds and pure mathematicks make up musick, and water with the same sciences make hydrostaticks; so, as I elsewhere note, by a further application of the same parts of knowledge to other subjects (and in some cases even to the same) those disciplines that are called mixed mathematicks, may be advanced probably as to number, as well as certainly as to usefulnesses and variety of experiments. Nor is it only in those parts of learning that I have now particularly named, that useful applications may be made of the theorems and problems of pure mathematicks, since upon these sublime sciences do also in great part depend those other mathematical disciplines, which are wont (by a synecdoche) to be called mechanical, and which it is now time that I pass on to consider.



OF THE  
 U S E F U L N E S S  
 O F  
 MECHANICAL DISCIPLINES  
 T O  
 N A T U R A L P H I L O S O P H Y.  
 S H E W I N G

That the Power of Man may be much promoted by the Naturalist's Skill in MECHANICKS.

**T**O prevent the danger of stumbling (as they speak) at the threshold, I shall begin this discourse with advertizing you, that I do not here take the term mechanicks in that stricter and more proper sense wherein it is wont to be taken, when it is used only to signify the doctrine about the moving powers (as the beam, the lever, the screws, and the wedge) and of framing engines to multiply force; but I here understand the word mechanicks in a larger sense, for those disciplines that consist of the applications of pure mathematicks to produce or modify motion in inferior bodies; so that in this sense they comprize not only the vulgar staticks, but divers other disciplines, such as the centrobarricks, hydraulicks, pneumaticks, hydrostaticks, balisticks, &c. the etymology of whose names may inform you about what subjects they are conversant.

Now that these arts (if you will allow them that name) may be of great use to the experimental philosopher, and assist him to enlarge the empire of man, may be made probable by this general consideration, that divers of those things, which in the former essay have been evinced to make the mathematicks useful to the naturalist, may be applied *mutatis mutandis* to the mechanicks also. Besides, that these disciplines have some advantages peculiar to themselves. But the truth of what is thus represented in general terms will possibly be better discerned, and more persuasive, if we descend to some particulars.

I. *FIRST* then, the phenomena afforded us by these arts ought to be looked upon as really belonging to the history of nature in its full and due extent. And, therefore, as they fall under the cognizance of the naturalist, and challenge his speculation; so it may well be supposed, that being thoroughly understood, they cannot but much contribute to the advancement of his knowledge, and consequently of his power, which we have often observed to be grounded upon his knowledge, and proportionate to it. When, for instance, we see a piece of wood, ducked under water, emerge again and float, even vulgar naturalists think that it belongs to them to consider the reason of this emersion and floating, which they endeavour to render from the positive levity, which they fancy to be (upon the account of the air and fire) inherent in the wood, though some woods that will swim in water, being put into oil, or high rectified spirit of wine, may sink.

BUT I see not, why it should not belong to philosophers to consider and investigate the reason, why one part of floating wood appears above the water, whilst the other keeps beneath it; and why the extant part is equal to the immersed, or either greater or lesser than it, in such a determinate proportion; and why the same wood will sink deeper in some waters than in others (as in a river than in the sea) as on the other side some woods will sink lower than others in the same water. For if these things be duly examined, as they may by the help of hydrostatics, not only the cause of these and the like phenomena will be discovered; but by the applications of that discovery an easy way may be devised to measure and estimate the differing strength of several salt springs, and also of divers kinds of lixiviums, and brines; to which may be added divers other practical corollaries from the same discoveries, which I shall hereafter have occasion to particularize.

II. *THE* mechanical disciplines help me to devise and judge of such hypotheses as relate to those subjects, wherein the notions and theorems of mechanicks either ought necessarily to be considered, or may usefully be so.

OF this we have instances, not only in those engines that are artificial, and are looked upon as purely mechanical, as the screw, the crane, the balance, &c. but in many familiar phenomena, in which the theorems of mechanicks are not wont to be taken notice of to have an interest; as in the carrying a pike or musket on one's shoulder, in the force of strokes with a longer or shorter sword or other instrument, the taking up and the holding a pike or sword at arm's length, and the power that a rudder has to steer a ship; in rowing with boats, in breaking of sticks against one's knee, and in a multitude of other familiar instances, of which the naturalist's skill in mechanicks will enable him to give a far more clear and solid account than the ancient schoolmen, or the learnedest physicians that are unacquainted with the nature and properties of the centre of gravity, and the several kinds of levers, the wedge, &c.

III. *NAV*, there are several doctrines about physical things, that cannot be well explicated, and some of them not perhaps so much as understood, without mechanicks.

THAT which emboldens me to propose a thing that seems so paradoxical, is, that there are many phenomena of nature, whereof though the physical causes belong to the consideration of the naturalist, and may be rendered by him; yet he cannot rightly and skilfully give them without taking in the causes statical, hydrostatical, &c. (if I may so name them) of those phenomena, i. e. such instances as depend upon the knowledge of mechanical principles and disciplines.

OF this we have an obvious example in that familiar observation, that we partly touched upon just now, about the swimming and sinking of wood in water. For if it be demanded, why wood does rather swim upon water than sink to the bottom of it, a school-philosopher would answer, that wood abounds with air, which being an element very  
much



much lighter than water, keeps it aloft upon the surface of that liquor. But this answer will scarce satisfy a naturalist versed in hydrostaticks. For not now to question what is taken for granted, that there is a positive levity, and that the air is endowed with that quality, experience shews us, that though when wood is not heavier than so much water, as is equal to it in bulk, it will swim; yet in case it be heavier than so much water, it will sink. As we see in divers woods, and particularly in guaiacum, which I therefore the rather name, because chymists observe, that if it be burnt, it leaves far less ashes (and such are supposed to contain the terrestrial and heavy parts) behind it, than many woods that we know will float in water. And though stones and iron be, upon the score of their weight, believed to be bodies that have little air in them, yet if the liquor into which they are put, be heavier, bulk for bulk, than they, they will not sink, but float, and if forcibly depressed, they will emerge; as you may try, when you please, by putting stones or iron, or the like ponderous body, upon quicksilver or melted lead; so that we need not here consider, whether air be, or be not predominant in a proposed body, when we would know whether it will or will not sink in an assigned liquor.

AND though we should admit the air, whether included in the pores, or looked upon as an elementary principle, to be the cause of its being lighter than an equal bulk of liquor, yet the air would be but the remote cause of its swimming, its immediate cause being, that the floating body is lighter than an equal bulk of the liquor, and therefore the same body, without acquiring or losing air, may swim in one kind of water, and sink in another. As in the case of heavy bodies, as loaden ships, that having prosperously sailed over the sea, are recorded to have sunk as soon as they come into harbour, i. e. into a more fresh water; and an egg, that will sink in common water, will swim in a strong brine. Nay a body may (as I, and others have tried) be so poised in water, that if the liquor be a little warmer, than when the body was poised in it, the body will sink; as it will emerge again upon the refrigeration of it.

AND if this general answer of the lightness of the air will not give so good an account as hydrostatical principles, why a piece of wood will float or sink; it will much less give so satisfactory an account, why differing woods in the same water, or the same piece of wood in differing waters, will sink just so far, and no farther; whereas, by hydrostatical principles, the phenomenon is easy to be accounted for, according to that theorem of *Archimedes* \* *περὶ τῶν ὀγκύμενων*, that solids lighter than the liquor they are put into, will sink in it so far, as that as much of the liquor as is equal in bulk to the demersed part, be equal in weight to the whole floating body; whence these corollaries are derived, that a floating body has the same proportion in weight to as much liquor as is equal to it in bulk, as the immersed part of the body has to the whole body. And likewise, that as much liquor, as is equal in bulk to the whole body, has the same proportion in weight to the said body, as the whole body has to that part of itself which is beneath the surface of the liquor. And as these corollaries determine the proportion between the immersed and extant part of the floating body; so (to shew you that these theories lead to practice) they suggest the way of making a small and light instrument, elsewhere described, to measure by a floating body the differing gravities of several liquors in reference to one another, as well as to the body itself. And upon the same grounds, the learned *Stevinus* shews, that if you know what part of a floating body is immersed in a liquor, whose specific gravity is also known, as it easily may be, you may presently find the weight of the whole solid body, let it be never so much too great to be weighed.

\* Lib. I. Prop. 5.

in balances or stateras, yea, though it were a vast ship itself; as supposing that that part of such a vessel that lies under water, should be 100,000 cubick feet, and that a cubick foot of water weighs 70 lb. (which though it be not the weight we have observed a foot of water English measure to amount to, yet that alters not the general rule) by multiplying 100,000 by 70, the product will be 7,000,000 lb. for the weight of the whole ship, with all that is contained in it, as ballast, ordnance, &c. or rests or leans upon it. If I should ask a mere school-philosopher, why sucking-pumps will not raise water higher than 40 feet (though it be commonly presumed they will raise it to any height) or why in an inverted siphon of glass, if you pour water and quicksilver in a sufficient quantity, the surface of the water in one leg of the siphon will not be in a level with the surface of the quicksilver in the other, but 13 or 14 times as high above the bottom of the siphon; or why, if a piece of iron, and a piece of marble or a flint, &c. be equiponderant in the air, if the scales be let down into the water, the metal will appear far heavier than the stone; if, I say, I should ask a mere naturalist both these or the like questions, I doubt I should much more perplex him, than he would satisfy me. And it were easy to add a multitude of examples, whereof a good account will scarce be given by a naturalist that is unacquainted with mechanicks, and may easily be assigned by one that is skilled in them. But referring the schoolmen to *Aristotle's* mechanical questions, to shew them the necessity and usefulness of mechanical knowledge, to give the solution of sundry phenomena that frequently occur, I will only add an example or two to make good the most paradoxical part of what I was saying; namely, that there are divers physico-mechanical phenomena, which are not to be, I say not explicated, but so much as well understood, without the knowledge of mechanical disciplines.

THERE is a considerable theorem in hydrostatics, which is thought to have been first taken notice of by *Mersennus*, and in a late writer, is thus expressed: *Velocitates motus aque descendantis & effluentis per tubos aequalium foraminum, sed inequalium altitudinum, habent subduplicatam rationem altitudinum*. Of which the corollary is, that the tubes are in a duplicate ratio to that of the velocities of the water that subsides in, and runs out of them; so that to make one tube at a circular hole of the same diameter run out in the same time twice as much water as another, the greater ought to be not only twice, but four times as long as the shorter. And of the same proportion (my trials about which I may elsewhere acquaint you with) divers other practical applications may be made, which must not be here insisted on.

IV. As I formerly said of the mathematics, so I now say of the mechanicks, that they may assist the naturalist to multiply experiments by those enquiries, that they will suggest, and those inferences and applications, whereto they may lead us.

Of this we have a noble instance in the great variety of trials, which enquiries, versed in hydrostatics, and other mechanical disciplines, have, upon the score of their being so qualified, been either prompted, or at least assisted to make, about the famous quicksilver experiment devised by *Torricellius*; about which, though so much has been done already, yet almost every year brings forth new phenomena.

ANOTHER example to our present purpose we may take from the great number of new propositions, that the diligent *Mersennus* has given us in his ballistics, about the force and effects of bows, and the like springy bodies. But a yet more noble instance is given us by the most ingenious *Galileo*, who, as we may learn from the already mentioned French writer, that has given us an account of *Galileo's* new thoughts in that language, has published so many propositions (of which he sets down 19 or 20, with the demonstrations) about the resistance of bodies to be broken, and the weights requisite



to break them, and the lengths at which they may be broken by their own weight, that he has reduced them into the form, and given them the title of a new art.

To all which I shall need to add no more, than that he, who knows and considers what a variety of useful propositions have been, or may be mechanically deduced from the observation of *Archimedes*, that a solid body weighs less in water than in the air, by the weight of water equal in bulk to that body, will easily dispense with me for not adding any farther instances on this occasion.

AND the mention of this hydrostatical proposition of *Archimedes* falls in the more properly in this place, because it will warrant me to tell you, that divers mechanical theorems are not only fertile in other theorems, but in useful applications too, of which I may hereafter have occasion to give you some examples, by acquainting you with the uses I have made of the lately mentioned proposition of *Archimedes*, and some corollaries, that partly by others, and partly by us, have been inferred from it.

V. BESIDES the utilities that may be ascribed to the mechanicks in common, with the more speculative mathematical disciplines, they have some, as I formerly intimated, that are more peculiarly their own, since they may be of great use to the naturalist in making of such instruments and tools, as for many of his observations, trials, and other purposes, he may either absolutely need, or advantageously employ.

OF this we have an example in the mariner's compass, as it is called; which is so necessary to those remote navigations, whereto natural philosophy and mankind owes so much. For though *Baptista Porta* \* does, as well as other authors, ascribe the invention of the directive faculty of the magnetick needle to one of his countrymen (*Amalphi*, in the kingdom of *Naples*) yet he confesses, that for want of the knowledge of making such sea-compasses as we now use, this lucky inventor was fain to make use of a piece of wood or straw, to keep the needle a-float, and then imbue it with a magnetick virtue; which was a shift subject to great and manifest inconveniencies. And indeed, notwithstanding the knowledge of the verticity of magnetical needles, if by that of the properties of the center of gravity, or some practices derived thence, some men, versed in mechanicks, had not devised a way so to poise the needle, that notwithstanding the rolling and tossing of the ship, it will continue horizontal enough to direct the pilot; what would become of him in those storms, when he has most need of a faithful guide?

By the help of the centrobairical doctrine, mechanicks have been enabled to make those dipping needles, whose phenomena are very odd; and though, as far as I have tried, they yet seem uncertain enough; yet it may very possibly happen, that farther observations may reduce them to some theory, whence practical inferences may be deduced.

AND you will the more easily believe, that the mechanical applications of centrobairical notions may be of immediate use, if we consider, that by virtue of them, divers writers, and others of unsuspected credit, assure us, that they have made a kind of lamp so poised, that one may roll it up and down like a bowl, without overturning the vessel that contains the oil, or extinguishing the flame.

FROM the knowledge that compressed air has a spring, whereby it resists farther compression, and a slight contrivance to make use of this pneumatical principle, an acquaintance of mine made a slight engine, which afterwards I found mentioned in a printed book, by which he was a great gainer, going, when he was well satisfied for his pains and hazard, to the bottom of the sea, and by the help of this engine staying there some-

\* Mag. Nat. lib. VII. cap. 7.

times for divers hours, till he had fetched up valuable things out of sunk ships, and tied cables about their guns, that they might afterwards be buoyed up.

BUT there might be given so many examples of instruments and tools that are useful to the naturalist, and for which, yet, he ought to thank the mechanicks, that it were tedious to enumerate them, especially since the shops of mathematical instrument-makers, and other tradesmen, may supply you with enough of them, to verify what this paragraph would persuade.

VI. I SHALL conclude the considerations I designed for this essay by this, that as the knowledge of the theorems of mechanicks, and the practices which have been thence derived, may very much assist the naturalist to make good mechanical contrivances, according to the exigencies of his several purposes; so one good mechanical contrivance may be equivalent to, and may perhaps actually produce many good experiments.

THE former part of this proposition will not, I think, require much proof. For a man must be but a dull naturalist, that shall know the properties of the center of gravity, of levers, balances, screws, wedges, and other instruments for increasing force, and by frequenting the shops and work-houses of mechanicians, shall have seen variety of engines and instruments to compass different things, if he do not, from the survey and consideration of all these, grow more able, by compounding, varying, and otherwise improving them, to devise such means and expedients, as he would not else have thought on, to make some trials, that he could not make before, and to make others more accurately, or more easily, or some way or other better.

AND as to the second part of our proposition, namely, that one good mechanical contrivance may be as considerable as many particular experiments, by enabling the naturalist to produce either numerous, or noble ones, or both, it may be manifested by several examples.

AND I shall begin with so familiar a one, as that afforded by valves, or trap-doors. For as slight and obvious as the invention of them seems, yet not only we owe to them a great variety of pumps and bellows for oeconomic uses, but they make very considerable parts of several other engines, and may, as some trials have informed us, be applied about several new experiments, especially if they be made of brass, and yet so small, that like some of those I have had made by skilful workmen (who, when I first directed them, told me, that they could not be made) they may be used, not only in small glass-pipes, but in syringes themselves.

By the help of small valves, and the knowledge of the spring of compressed air, have been made those wind-guns, which may be employed, not only to weigh the air (whose weight we found them to evince, but not determine) but to kill deer, and other game, without making a great noise that would fright away the rest.

If I did not, *Pyrophilus*, foresee, that in the following essays of this treatise, I shall have occasion to mention some other instances of the service, that mathematical and mechanical disciplines may do the naturalist, I should here add divers particulars, which I had rather you should, when you meet with them, refer hither; and therefore I shall conclude what I intended now to say about these disciplines, by two or three short instances that relate to what I have already said concerning them.

THE first is, that it was not my design to treat of the utility of the mathematics and mechanicks in an absolute way; for then I must have said much to their advantage, which I have omitted, because it would have too much swelled these essays, and not have been pertinent enough to them. And therefore I thought it sufficient for me to touch upon those things, on whose account these disciplines may be made useful to the naturalist, by assisting him either to frame theories, or to make observations and experi-

ments,



ments, some (at least) of which, directly, or in their applications, either are already, or are like to prove, practical and useful. And it seems to me very probable, that the notions and practices of these disciplines that have been too much hitherto restrained by mere mathematicians and mechanicians to the stars, the earth, the water, and some few other conspicuous parts of nature, may be very well extended by a philosopher to sundry other productions, as well of nature, as of art. As *Archimedes* deduced hydrostatics from the application he made of vulgar statics to bodies weighed in air and water, or in water only; and the ingenious *Torricellius*, and others, have of late applied the principles of hydrostatics to that ponderous body (which the chymists reckon among metals) mercury.

My next advertisement is, that mentioning mechanical instances, not so much to acquaint you fully with the things themselves, as to make the mediums to infer what I would prove, I have taken the mechanical propositions, that I employed, as they are delivered by the artists themselves, without warranting that their proportions will hold true in mathematical strictness. For though I have made trials myself of several things of this nature, yet having often observed, how difficult it is to find a mathematical preciseness in physical and mechanical things, I think it not amiss to intimate thus much to you, though I may elsewhere have a fitter opportunity to rake it out, that so great an exactness is in many cases not necessary to make the rules, that want it, useful in practice.

The concluding intimation I mean to give you, is, that I have not hitherto mentioned a service, that mathematicks and mechanicks may often do the naturalist, which is not fit to be silently pretermitted; and it is, that by lineal schemes, pictures, and instruments, they may much assist the imagination to conceive many things, and thereby the understanding to judge of them, and deduce new contrivances from them.

THAT I do not groundlessly say this, you will grant, if you consider, how difficult (not to say impossible) it were to go through with a long geometrical demonstration, without the help of a visible scheme, to assist both the fancy and the memory; and how difficult it is to give beginners an idea of the grounds of cosmography and geography, without material schemes and globes, your own very recent experience, as well as that of others, will, I presume, inform you. As it also may, how useful, not to say how necessary, pictures, and in some cases, models, are wont to be, when engines, houses, ships, and other structures are to be judged of, that they may be approved or improved; but I shall rather take notice, that not only mechanical, mathematical, and anatomical things, need schemes and pictures to represent them clearly to our conceptions; but many things that are looked upon as more purely physical, may, in my opinion, be much illustrated the same way. Of which, if *Des Cartes* has, as some say, been the intruder, I think he deserves our thanks for it. For, as *Plato* said, God does always geometrize; so in many cases it may be as truly said, that nature does play the mechanician, not only in animals, but in plants and their parts, and divers other bodies; in the explication of which, curious, and oftentimes invisible contrivances of her's, pictures, that represent them well to the eye, and, if it were needful, in dimensions much greater than natural, may very much further the framing of right ideas of them in the mind.

THAT THE  
GOODS of MANKIND

May be much increafed by the

NATURALIST's INSIGHT  
INTO  
TRADES.

**T**O make out what is propofed in the title of this difcourfe, I fhall endeavour to fhew two things. The one, that an infight into trades may improve the naturalift's knowledge. And the other, that the naturalift, as well by the fkill thus obtained, as by the other parts of his knowledge, may be enabled to improve trades.

SECTION I.

AND firft, it feems to me to be none of the leaft prejudices, that either the haughtinefs and negligence, which moft men naturally are prone to, or, that wherewith they have been infected by the fuperciliousnefs and lazinefs, too frequent in fchools, have done to the progrefs of natural philofophy, and the true intereft of mankind, that learned and ingenious men have been kept fuch ftrangers to the fhops and practices of tradefmen. For there are divers confiderations that perfuade me, that an infpection into thefe may not a little conduce, both to the increafe of the naturalift's knowledge, and to the melioration of thofe mechanical arts.

I. AND I confider, in the firft place, that the phænomena afforded by trades, are (moft of them) a part of the hiftory of nature, and therefore may both challenge the naturalift's curiofity, and add to his knowledge. Nor will it fuffice to juftify learned men in the neglect and contempt of this part of natural hiftory, that the men, from whom it muft be learned, are illiterate mechanicks, and the things that are exhibited are works of art, and not of nature. For the firft part of the apology is indeed childifh, and too unworthy of a philofopher, to be worthy of a folemn anfwer. And as for the latter part, I defire that you would confider, what we elfewhere exprefly difcourfe againft, the unreafonable difference that the generality of learned men have feemed to fancy betwixt



betwixt all natural things and factitious ones. For besides, that many of those productions that are called artificial, do differ from those that are confessedly natural, not in essence, but in efficiencies; there are very many things made by tradesmen, wherein nature appears manifestly to do the main parts of the work: as in malting, brewing, baking, making of raisins, currans, and other dried fruits; as also hydromel, vinegar, lime, &c. and the tradesman does but bring visible bodies together after a gross manner, and then leaves them to act one upon another, according to their respective natures; as in making of green or coarse glass, the artificer puts together sand and ashes, and the coagulation and union is performed by the action of the fire upon each body, and by as natural a way, as the same fire, when it resolves wood into ashes, and smoak unites volatile salt, oil, earth, and phlegm into soot; and scarce any man will think, that when a pear is grafted upon a white thorn, the fruit it bears is not a natural one, though it be produced by a coalition of two bodies of distant natures put together by the industry of man, and would not have been produced without the manual and artificial operation of the gardener.

II. But many of the phenomena of trades are not only parts of the history of nature, but some of them may be reckoned among its more noble and useful parts. For they shew us nature in motion, and that too, when she is (as it were) put out of her course, by the strength or skill of man, which I have formerly noted to be the most instructive condition, wherein we can behold her. And as it is manifest that these observations tend directly to practice, so, if I mistake not, they may afford a great deal of light to divers theories, especially by affording instances, wherein we see by what means things may be effected by art, and consequently by nature, that work mechanically.

III. The phenomena afforded by trades are therefore the fitter to be translated into the history of nature by philosophers, because they, whose profession it is to manage those things, being generally but shopkeepers, and their servants being for the most part but apprentices and boys, they neither of them know themselves how to describe in writing their own practices, and record the accidents they meet with; so that either learned men must observe and register these things, or we must, to the no small prejudice of philosophy, suffer the history of nature to want so considerable an accession, as the shops and workhouses of craftsmen might afford it; which accession would be much the more copious, if the experiment of trades were made by a naturalist, who would doubtless so manage them, as to make them far more instructive, and better fitted for the design of a natural history, than the same experiment would be, if they were related but by an illiterate tradesman, though never so honest.

AND, *Pyrophilus*, to invite you, as you design a further progress in natural philosophy, to disdian as little as I do, to converse with tradesmen in their workhouses and shops; give me leave to tell you, that as he deserves not the knowledge of nature, that scorns to converse even with mean persons, that have the opportunity to be very conversant with her; so oftentimes from those that have neither fine language nor fine cloaths to amuse him with, the naturalist may obtain informations, that may be very useful to his design, and that upon several scores.

For first, tradesmen are usually more diligent about the particular things they handle, than other experimenters are wont to be; because these, if they want diligence, lose nothing but what that very want of it keeps them from taking notice of, or at most, the satisfaction of an unnecessary curiosity; whereas tradesmen have another guise concern in the management of what they employ themselves about, for their livelihood depends upon it. And as, if they be careless, others more diligent will get away their custom, so, if they do any thing extraordinary well, the chiefest, and, for some time, the

whole benefit will accrue to themselves, and by improving their profession they better their income.

SECONDLY, As it is proverbially said, that necessity is the mother of inventions, so experience daily shews, that the want of subsistence, or of tools and accommodations, makes craftsmen very industrious and inventive, and puts them upon employing such things to serve their present turns, as nothing but necessity would have made even a knowing man to have thought on. By which means they discover new uses and applications of things, and consequently new attributes of them; which are not wont to be taken notice of by others, and some of which, I confess, I have not looked upon without wonder.

THIRDLY, I have several times observed trades deal with things unknown to classical writers, and unused, save in their shops. And these are not only factitious, but divers of them natural; as manganese (by some called magnesica) and zafra (if at least it be what many repute it) emery, tripoli, &c. and of both sorts there are some that are exceeding useful; as of those formerly mentioned, the two first are to glass-men and porters; and the two latter to a number of other tradesmen; and as among artificial concretes, soaders are of necessary use to gold-smiths, lock-smiths, copper-smiths, brasiers, pewterers, tin-men, glassiers, &c. amels to gold-smiths, glass-men, &c. lakes of several sorts to painters, heralds, &c. and putty to amel-founders, potters, stone-cutters, gold-smiths, glass-grinders, and divers other professions. I shall add, that even of those natural things, of which some mention is made in famous books, one may learn many things in shops, not to be met with there, both as to the differing kinds of things, and as to the marks of their goodness, and as to other particulars conducive to the knowledge of those subjects. And I freely confess to you, *Pyrophilus*, that I learned more of the kinds, distinctions, properties, and consequently of the nature of stones, by conversing with two or three masons, and stone-cutters, than ever I did from *Pliny*, or *Aristotle* and his commentators.

FOURTHLY, You shall often find, that tradesmen, being unacquainted with books, and with the theories and opinions of the schools, examine the goodness and other qualities of the things they deal with, by mechanical ways, which their own sagacity or casual experiments made them light upon. And though these, having little or no affinity with those, that a bookman would have taught them, will appear to him extravagant; yet being such, as, if they really serve the craftsman's turn, must be true and useful, their being extravagant will but make them the more new and instructive, and consequently the more fit to be admitted into the history of nature.

FIFTHLY, The observations that tradesmen can supply us with, though they are not probably at any one time so accurately made by them, as they would be by a learned man; yet that defect is recompensed by their being more frequently repeated, and more assiduously made, than most of the experiments, wherein men of letters have furnished natural history; so that those circumstances, which are not heeded by the artificer at one time, may obtrude upon his observation at another, and, by reiterating the same processes so often, it can scarce be doubted but that divers phenomena will offer themselves, even to an unattentive eye, that would not have been all of them taken notice of by a more heedful experimenter that had performed the operation but once or twice. But this will be further confirmed in the next paragraph.

SIXTHLY, There are tradesmen that do often observe in the things they deal about, divers circumstances unobserved by others, both relating to the nature of the things they manage, and to the operations performable upon them.

Of the particulars, wherein the observations of tradesmen (for the utility of many of their practices is not questioned) may help us to investigate the nature of bodies, I could name



name more than my present haste allows me to mention; and I shall, as a specimen, take a little notice, first, of some of the remarks they have to distinguish and estimate what they call the goodness and badness of the things they deal with; and then of some few of their observations that depend upon the influence that time and season have on the things they handle, and upon the artificers operations on them. For, to begin with the first, although they commonly mean by such terms (of goodness and badness) no more than the fitness or unfitness of such things to yield a good price, and in order thereunto for the purposes they are to be employed about in their particular trades; yet this fitness or unfitness is wont to consist in, or to suppose qualities that may relate to divers other things, and be applied to many other purposes. For some of the tradesmen's criteria discover to us a variety and a difference of kinds in bodies of the same denomination; as from the potters, the tobacco-pipe-makers, and the glass-men, we may learn a considerable variety of clays; and from stone-cutters and masons no less variety of stones untaken notice of by classic authors. So from carpenters, joiners, and turners we may learn, that some woods, as oak, are fit to endure both wet and dry weather; others will endure well within doors, but not exposed to the weather; others will hold out well above ground, but not under water; and others on the contrary will last better under water, than in the air.

AND as the distinguishing marks we were speaking of may inform us of the differences and kinds of bodies; so they may likewise on other accounts give us notice of divers of their qualities. Thus we find by the glass-men and soap-boilers, that some ashes, as those of kaly, bean-stalks, &c. do much more abound in salt, than other some; and yet some of those sorts of ashes make clearer, or otherwise better glass, than the rest do. We may likewise learn of the malsters the differing impressions that the barley receives according to the fewel, whether straw, wood, furze, &c. that makes the fire wherewith it is dried. And I remember, I have known an ingenious malster much advantaged by a way he had of so preparing malt, as if it had not been dried with wood (usually the cheapest, but not the best fewel for that purpose) whereas indeed it was a secret consisting only in the choice and seasoning of such a kind of wood, that even the solid parts of it cleft burnt almost like straw with a clear flame, so strangely free from smoke, that I could not behold it without some wonder.

THE other sort of instructive observations to be learned of tradesmen consists of those that are made about the operation, that continuance of time, or change of season and weather may have upon certain bodies, and ways of handling them. For naturalists, usually contenting themselves to make their experiments but once or twice, when their leisure best serves, or their occasions most require, have not the same opportunity to discern what influence the temper, which the air then is put into, either by the season or the weather, or both, may have on the event of the trial; whereas tradesmen, by long and sometimes unwelcome experience, are taught such and such things will be best done at such seasons of the year, or in such kind of weather; which if they be not in some cases observed, either the thing will not succeed, or the tradesman will be damaged by his trial.

THUS we see, that tanners make choice of that part of the spring, when the bark abounds with the rising sap, to take it off from the trees; because at all seasons it will not be so good nor come off so easily. Thus joiners think not wainscot sufficiently seasoned, till it be so many years old. And in several countries, butchers observe, that though a young bullock may be very good meat, if spent soon after it is killed; yet if powdered, to be long kept, before the beast be four or five years old, the salt will too much fret it, and make it little worth. And I look upon it as one of the advantages  
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the naturalist may derive from tradesmen's observations, that the same things being successively dealt with by the father and the son, the master and the apprentice, they sometimes make far more long winded observations than the philosopher has opportunity to do. As for instance, those that make mortars of lignum vitæ, and will make them good, will keep it in the house twenty years, or perhaps more, to season, as they call it, before they will employ it. And experienced masons tell us, and as far as I have observed truly enough, that as there are some sorts of lime and stone, that will decay in few years; so there are others, that will not attain their full hardness in thirty or forty, or a much longer time. Of which I may elsewhere give you some instances.

To the six foregoing particulars one more may be added to the same purpose with the rest, and it is, that by frequenting the workhouses and shops of craftsmen, a naturalist may often learn other things, besides the truth and falsity of what they relate, concerning the history of the arts they make profession of. For though a tradesman, being for the most unlearned, and aiming only at making or performing those particular things, which, when done, are to bring profit, usually overlook those phenomena that make not to his purpose; yet nature, (who minds as little his design, as he does those works of her's, that conduce not to it) is by some agents and operations, that he employs to compass his ends, engaged to do several things, that have a connection with those the artificer persecutes, or else do depend upon them: so that the naturalist may oftentimes observe in shops divers considerable phenomena, that the tradesman regards not; because they neither further, nor hinder him in his work, and will be looked upon by him as impertinent to the history of his profession, in case he should be put upon delivering it. And yet some of these occurring phenomena being produced by nature, when she is as it were vexed by art, and roughly handled by ways unusual, and sometimes extravagant enough, may discover to a heedful and rational man divers luciferous things not to be met with in books, or probably not so much as dreamed of by the authors of them. Sundry examples of this I shall have occasion to disperse in the following Essay, and other tracts that are designed you in this second volume of our present treatise.

## S E C T I O N II.

I WILL now therefore proceed to shew, that as the naturalist may, as we have seen, derive much knowledge from an inspection into trades; so by virtue of the knowledge thus acquired, as well as by that which he has upon other accounts, he may be as able to contribute to the improvement of trades.

THIS he may do by several ways, and especially by these three. The first, by increasing the number of trades, by the addition of new ones. The second by uniting the observations and practices of differing trades into one body of collections. And the third, by suggesting improvements in some kind or other of the particular trades.

THE first of these I shall here lightly pass over, having elsewhere occasion to discourse of it more fully; only I shall here take notice, that, for the experimental philosopher to increase the number of trades now in use among us, it will not be absolutely necessary, that he should invent new ones, since he may do it by reviving the trades formerly known to the ancients, but lost to us; such as the making incombustible cloth of *lapis amiantus*, the Tyrian purple, the making of Mosaic work, and those many other inventions, which you may find mentioned in *Pancirollus*, and his learned commentator *Salmuth*. Of which it were not amiss, that a catalogue were made publick; for such things, having been once actually done by men, are not impossible to be done again;

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and therefore I see no reason to despair, that in so ingenious an age as this, some, if not most, of them may be retrieved.

THE second advantage that trades may derive from an inquisitive naturalist, is, that by this means the several observations and different practices of trades, whose managers want the curiosity, the skill, or the opportunity, to make a general inspection into trades, which they would find the more difficult to do, because craftsmen will often be more shy of one another, and more backward to disclose the mysteries of their art to one that may make a gain of it, and thereby lessen theirs, than to a philosopher, that inquires to satisfy his curiosity, or enable himself to be helpful to them. And certainly, if so much as the known hints, that may be given by the experiments already dispersed among men of several professions, were known to any one man, though otherways but of common abilities; as my own experience has in some measure informed me; those united beams, which scattered are scarce considerable, would afford him light enough to better most of the particular trades, that are retainers to philosophy. And perhaps, it were not amiss, if there were some knowing and experimental persons appointed by the publick to take an exact survey of the trades in use amongst us, and inform themselves particularly of all the secrets and practices belonging to them, that thus discerning the errors and deficiencies of each, they may rectify the one, and supply the other, partly by the hints afforded by the analogous experiments of some other trades, and partly by their own notions and trials.

Thus a few of the more ingenious French gardeners have of late usefully applied to the watering of young and tender plants that way of filtration, which is used by apothecaries with moistened cotton wicks or rolls, or else with lifts of either linen or woollen cloth, so ordered, that one end being immersed in the liquor to be strained, the other may hang over the brim, and out of the vessel somewhat lower than the bottom, or at least the surface, of the liquor. For if this lower end of the lift be placed over the root of any seed or tender plant, it will, by constantly and leisurely dropping on it, water it much more temperately and uniformly, than can be done by common watering-pots. And even this way of irrigation may by a cheap and easy mechanical contrivance be very much improved. There is another practice among stone-cutters, that cast or mold things with plaister of Paris, to obtain finer powders than searces are wont to give them, by stirring the powder well in water, and after it has rested a little while, pouring off the upper part of the troubled liquor into another clean vessel; at the bottom of which there will in time settle an impalpable powder. I will not here tell you what use I make of this in chymistry, to obtain much finer powders than are usually to be met with of the same denomination. And I shall but intimate to you, that by letting the first water stand but so much the longer before you pour off the upper part of it, till not only the grosser and heavier, but the less fine particles be subsided, you may get a powder, yet much more subtle, than those artificers that employ the former way, without this circumstance, are wont to obtain. This, I say, it shall suffice me to have pointed at, because it is more proper to take notice, that the way of obtaining subtile powders by the help of water is useful, not only to the above-mentioned craftsmen, but likewise to glass-men, potters, makers of telescopes, and microscopes, those that cast metals in spaud, and other tradesmen too. Besides, that I may hereafter have occasion to tell you, that it is of great use in *China* for the makers of porcelain.

BUT it is not only by acquainting artificers of different professions with one another's practices, that the naturalist may further trades, but by making materials employed by one sort of craftsmen serviceable to another. That philosopher, who has surveyed a great number of trades, and compared them together, may do this with advantage, you will

will easily grant, when I shall have advertised you, that without any such assistance as that of a philosopher, in whom their distinct knowledge may center, and who has skill to enlarge the applications of them, we may observe, that sometimes tradesmen themselves can make use of one another's productions. Of which I shall give you a couple of examples, the one furnished me by litharge, the other by aquafortis.

THE former of these, which is but lead powdered and almost vitrified, by being blown off (or melted into) the refiner's test, as it serves the chymist to take his sugar of lead (which it has been observed to do better, than minium) and other saturnine medicines; so it serves divers comb-makers to die horns (as we have tried by the mixture of litharge, quick-lime, and sharp vinegar. It serves also some painters and others to accelerate the preparations of their fat oils, as they call them. And some varnishers to make their varnishes dry quickly. It likewise serves some artists to make counterfeit gems; and we have tried, that by melting it with about a third part of pure white sand, or calcined crystals, and then putting in a small quantity of mineral concretes, according to the colour intended to be introduced, one may make sapphires, emeralds, &c. coloured like the natural ones; though this way makes these productions too ponderous, soft, and dim, and is far inferior to another we may elsewhere have occasion to disclose.

OTHER mechanical uses of litharge I omit, to come to the second instance I was mentioning, which is taken from aqua fortis. For not only refiners use it to part silver from gold and copper (whence the French call it *Eau de depart*) but divers makers of curious wooden works use it for the discolouring and staining of their woods. Dyers make great use of it about their colours, and even about scarlet itself. Other artificers employ it to colour bone or ivory, steeped for a convenient space of time therein, having first made it of the colour they desire, by dissolving in it copper (instead of which I have sometimes used verdigris) or other bodies, fit for their present turn; and some too by dissolving in it the fourth part of its weight of sal armoniac, turn it into aqua regia, and in that make a solution of gold, wherewith may be stained (as we have tried and taught some artificers) the ivory hafts of knives, and boxes of the same matter, with a fine kind of purple colour, which yet will not suddenly disclose itself on them. Some book-binders also employ aspersions of aqua fortis to stain the leather, that makes those fine covers of books that, for their resemblance to speckled marble, are wont to be called marbled. It is also employed (as themselves have acknowledged to me) by some of the diamond cutters, to free the dust of diamonds from metalline powders, as I shall hereafter declare. It is likewise of great (and as they imagine of necessary) use to those that etch plates of copper or brass. To which may be added, that we have caused canes to be stained into the likeness almost of tortoiseshell by a mixture of aqua fortis, not too well rectified, which is unexpedient in this work, and oil of vitriol laid on at several times and places, upon canes held over a large chafing-dish of coals, that by the heat the staining liquor may be the better sucked in by the canes, which must afterwards have a glo's given them, by being diligently rubbed with a little soft wax and a dry cloth. Nor are these all the uses made of aqua fortis, as you will find hereafter by instances, that I reserve for other places. But I thought fit to mention this liquor in this place, rather than any of those many factitious bodies I might have taken notice of, for these two particular reasons. The one, that the uses hitherto enumerated of this menstruum may serve to confirm what I told you in the second essay, of the great utility of menstruums; and the other, that though aqua fortis be a liquor of exceeding common use, and wont to be distilled by men of several professions, as chymists, refiners, gold-smiths, &c. yet they have had hitherto so little curiosity to enquire into the nature of it, or vary the ways of making it, that not only the ways, that a



A skilful naturalist might direct for improving it, have not been taken notice of, but no small oversights may be observed to be generally and daily made about it. And an ingenious gentleman of my acquaintance, by making some trials to improve it, has been so far successful in his attempts, that he makes it by great odds better than that which the refiners are wont to employ, or, as far as my trials have informed me, than any I have used; and affords it for not much above half the price that is commonly given for it. Nor have his experiments this way alone promoted the refiner's trade, but have also disclosed to him a way of clearly recovering most of his aqua fortis, after he has used it in the separation of metals, not only in its former strength, but somewhat increased in virtue; which you will the more easily think possible, if I tell you, that aqua fortis may be made and received in other vessels, than those that are usual. As also, that without dreaming of this chymist's way, I have re-obtained that menstruum exceeding strong, after having employed it upon certain minerals, for from others I know not whether it may be so regained. And lastly, that there are some bodies, besides glass and earth, that are not brittle like these, and yet serve for the second distillation of aqua fortis, though made very strong at the first.

AND since I am mentioning of this liquor, I shall intimate (and only intimate here) that, by adding to saltpetre, instead of the usual additament of three times its weight of brick, or clay, or the like, about an eighth or tenth part only of its weight of another substance, we have, even in ordinary sand furnaces, obtained, though slowly, a nitrous spirit, or aqua fortis much stronger at the first distillation, than that which is wont to be sold by our refiners, for double or rectified aqua fortis.

You, *Pyrophilus*, and divers other virtuosi, have much more opportunity to make an inspection into particular trades, than my other studies and occasions will allow me, and yet I have been more than once able to suggest to eminent artificers such things, concerning their own profession, as they tried and thanked me for. And therefore I have often wished, that some ingenious friends to experimental philosophy would take the pains to enquire into the mysteries, and other practices of trades, and give us an account, some of one trade, and some of another, though the more are handled by the same person it will be, *ceteris paribus*, the better, not only delivering historically what is practised, but also adding their own reflections, and any other thing they think fit to propose, towards the melioration of the professions they write of.

AND to give you, for a specimen of this (not perhaps the best that I could, but) such an one, as will be sure not to make you despair of out-doing it, I will add at the close of this essay, what came into my mind, and cost me about an hour to set down, about the trade of those that sell varnished wares.

SOME Italian writers (who indeed are to be commended for it) have given us accounts of some particular professions, as besides others, that I have heard of, but could not procure: *Antonio Neri* has written *Dell' Arte Vetraria*, and *Benvenuto Cellini* of sculpture, and the statuary's art, and of some other professions, worthy, with the art of glass-making, to be made English.

AND indeed, I would willingly invite both you and other virtuosi of our own country, as well as of others, not to disdain to contribute their observations to the history of trades. And if you pitch upon any, you may command my thoughts of the method, wherein an account of it may be the most conveniently given. For I look upon a good history of trades, as one of the best means to give experimental learning both growth and fertility, and like to prove to natural philosophy what a rich compost is to trees, which it mightily helps, both to grow fair and strong, and to bear much fruit.

And this I was so persuaded of, that I once designed, if the publick calamities of my country had not hindered, to bind several ingenious lads apprentices to several trades, that I might the better, by their means, both have such observations made as I should direct, and receive the better historical accounts of their professions, when they should be masters of them.

III. But it is not only by making the practices and productions of some trades serviceable to others, that the experimental philosophy may be a benefactor to those professions. For he may do it by the third of the formerly mentioned ways (which in some cases is coincident with the second) namely, first by surveying the rules and observations already received, and the practices already in use of each particular trade he would improve, and then by taking notice of two things concerning it, viz. the deficiencies and inconveniencies, that blemish it, and the optatives that may be made about it; that he may also in the last place propose rational (if not certain) methods or expedients to supply or remedy the first; and either accomplish the second, or make approximations to it, as far as it is feasible, or as his skill reaches.

By deficiencies and inconveniencies I do not here mean those things, which are wanting to the absolute perfection, which a philosopher might wish to find in the trade he considers; (for these belong to the optatives) but those, which are wont to be complained of, and not irremediable, or that are wanting to a more easily obtainable degree of perfection. I shall not pretend to enumerate these in particular trades, but only observe in general, that the chiefest of them seem to be such as these.

FIRST, that the artificer may be too much confined to certain materials, some of which may be scarce, or dear, or ill conditioned, in comparison of others that the naturalist might propose. As I remember, that being in a place, where we could not procure good vitriol to make aqua fortis with, after the manner of our English refiners, by a substitution of burnt alum for vitriol, but in a far less proportion, we made solvents for silver, as good as theirs, if not much better.

And especially in such cases as these it is, that the naturalist may be very much assistant to tradesmen. For there are many things, which he who is acquainted with variety of bodies, and the accounts on which they work on one another, will either quickly discern to be performable by other materials, than those that tradesmen confine themselves to, or probably guessed to be performable by other agents more in the tradesmens power; and by making trials of his conjectures, it is like he will within a few trials discover what he seeks. I know an ingenious person, that upon the general complaint made by tanners, of the scarcity and dearth of the bark of oak, found a way to prepare leather without that or any other bark, as well, if not much better, than it is wont to be done the ordinary way, at least, as far as I, and divers others more skilful than I, could guess by some variety of it, which he shewed me. And this variety of materials, which may be suggested by the naturalist, is therefore the more considerable, because, that though the suggested materials be dearer than that in common use, yet it may be so much better conditioned in other regards, as to be preferable to it. And though diamond dust be very many times dearer than the powder of emery, yet I sometimes cause work to be done for me in a shop, where, to cut some gems, and even loadstones themselves, the craftsmen I made use of did, by my encouragement, employ the precious powder of diamonds, instead of that of emery, because the former makes so great a dispatch, and obliges them so much the seldomer to change their tools they apply it with, as makes an advantageous amend for the dearth. And so, though common spelter-foder be much cheaper, than that which is made with silver instead of spelter, yet in divers cases, this last is preferable, even by artificers themselves. For trials informs us,  
that



that this will run with so moderate a heat, as often needs not endanger the melting of thin and delicate pieces of work, that are to be soldered; and if this silver-solder be so well made, as some I can shew, you may with it, solder even upon solder itself, made the ordinary way with brass and spelter, and so fill up those little holes or crannies that may have been left or made in the first soldering, and are not safely to be mended, but by a solder more easily fusible than the first.

SECONDLY, that the tradesman may be confined to certain ways of working, when perhaps it would be much more advantageous to him, if he had others proposed him by the experimental philosopher, who may perhaps discern, that what is mechanically done by the artificer, may be better done physically; and on the contrary. Whereas goldsmiths, first directed probably by some chymist, by boiling silver-spurs, hilts, &c. of curious workmanship in salt, alum, and argol, give it that whiteness and clearness, which it would scarcely be securely brought to by brushing, or pumice stone, or putty. And the like clearness, experience has informed us, that old sullied pieces of good gold may be brought to in a trice, by the help of warm aqua fortis. And as there are divers other things (some of which you will find mentioned in a following essay) that, though wont to be done mechanically, may be done better by physical means; so of those things that ought to be done mechanically, many things, that are wont to be done by the labour of the hand, may with far more ease and expedition (the quantity considered) be performed by engines; by which, if they be skilfully devised, our observations make us bold to think, that many more of those, that are wont to require a laborious or skilful application of the hands, may be effected, than either shop-men or book-men seem to have imagined. For not to mention those several instruments, on which I have *extempore* played divers tunes that I had never learned, when we see, that timber is sawed by wind-mills, and files cut by slight instruments, and even silk-stockings woven by an engine, besides divers other artificial inventions left not named, because they cannot intelligibly be so in few words, we may be tempted to ask, what handy work it is, that mechanical contrivances may not enable men to perform by engines?

THIRDLY, there may be deficiencies also in this, that what the artificer undertakes is either long in doing (as in the ordinary way of tanning, brick-making, seasoning of wood, &c.) or takes up more pains, or requires a greater apparatus of instruments, or else in some other way more chargeable, or troublesome, or laborious to be effected, than it needs be. And these kinds of deficiencies may in very many cases be supplied by the experimental philosopher. As I know an inquisitive person, that has upon a solemn trial, tanned as well as the masters of the profession, in far less time, (and if I much forget not, in less by above half) than they; so in some places they have a quick way of seasoning some kinds of wood, for the use of sea-timber, by baking it in ovens, (which way I have also known used here in *England*, to season some sorts of wood for other uses in a few hours;) so, whereas our grinders of dioptrical glasses have hitherto believed, that they must make use of Venice glass, which is very dear, and oftentimes very scarce to be come by, some virtuosi, considering that the great clearness of an object-glass is rather an inconvenience, than a very desirable qualification, have newly taught some of the artificers to employ that coarser and cheaper sort of glass, they call green-glass, which is made here in *England*, instead of the other, which now begins to be thought by the skilful (with whom my observations disagree not) to be inferior to it. And several dyers employ our woad, which is not far fetched and much cheaper, instead of the eastern indigo, for dyeing of some (if not all) sorts of blues, and those other colours, which that grand tincture prepares the cloth to receive.

FOURTHLY, another sort of deficiencies or inconveniencies may be the want of durability, either as to the very being of the thing produced by the artificer, or as to the beauty or the goodness of it.

Of the former sort may be (not to mention the decay and souring of cyder, perry, &c.) the cracking of glass of its own accord, and particularly that which is complained of by divers who deal in telescopes, that the object-glasses, which are wont to be made, as I was saying, of fine Venice glass, will sometimes, especially in water, flaw of themselves, and so grow useless, to prevent which, some, that are very curious, carry them in their pockets.

Of the latter sort is the fading of the bow-dye of water colours in limning, and the rust of shining arms, and other polished steel. Divers of these inconveniencies also the naturalist may obviate or remedy; as some of the virtuosi above-mentioned, by teaching the glass-grinders to make the object-glasses of their telescopes of green glass, have taught them a way to make them durable in spite of the vicissitudes of weather. And I have had pieces of artificial crystal, whereof some, though in no long time, cracked in so many places, that they changed their transparency for whiteness; yet another, though much larger, did, as I conjectured it would, hold sound during some winters, nor was ever broken but by accident: and I remember, I told the artificer, in whose furnace the crystal, that lasted not, had been made, that I took, as I do still, the reason of the difference to be, that the durable crystal had but a due, and the other an over-great proportion of fixed salt. The reasons of which conjecture I shall have occasion to give you in another place.

AND, as to the scarlet dye (whereof I lately made mention) that it may be much advanced, as to point of fixedness and lastingness, beyond the common bow-dye, I was persuaded by an honest merchant of *Amsterdam*, who had got a great estate by colouring of cloth, and was particularly curious about the scarlet dye: for he presented me with a piece of scarlet (of which he said he could make enough at a reasonable rate, wherein he almost desired me to find either any part undyed, or to stain it with vinegar, lixivium, and other liquors that he named; and indeed by cutting it I found, that though it were a thick piece of cloth, the middle of it was not (as is usual in scarlets) white or pale, but it was dyed quite thorough; and though of scarlet I shall elsewhere have occasion to speak farther, yet I the rather mention it in this place, because it affords me a notable instance, that trades may be considerably improved by those that do not profess them. For the most famous *Cornelius Drebel*, who was the inventor of the true scarlet dye, was a mechanician, and a chymist, not a dyer; and as an ingenious man (that married his daughter) related to me, was so far from having been versed in that profession, when some merchants put him upon the advancement of a certain way of dying a fine red, or rather crimson, that had been a while before casually lighted on in *Holland*, and proved very gainful to the finders, that he did not know so much as the common way of dying the ordinary reds, though the merchants having once taught him, that, by the help of a sagacious conjecture (to be told you in one of the following essays) he soon invented the true scarlet dye, which has since been so much esteemed.

It now remains, that I mention in a few words the optatives, that may be proposed by the naturalist about the particular trades he would improve. By which name of optatives I mean all those perfections, that being desirable, are rather very difficult, than absolutely impossible, to be obtained. Of which optatives there may sometimes belong several to one craft or profession.

Of this sort, in the blacksmith's profession, may be the making iron to be fusible, with a gentle heat (as the flame of a candle) and yet hard enough for many ordinary uses.



uses. In the glass-men's trade and the looking-glass-makers, may be the making of glass malleable or flexible. In the clock-maker's trade, the making the newly devised pendulum clocks, useful in coaches, boats, ships, and in other cases where they are put into irregular motions.

In the brasier and copper-smith's trade, the making of malleable solder. In the shipwright's art, the making of boats and other vessels to go under water. In the diver's profession, some small and manageable instruments, to procure constantly, at the bottom of the sea, fresh air not only for respiration, as long as one pleases, but also for the burning of lights.

In the assay-master's trade, the quick melting down of ores, and cupelling of them, or at least of metals, in a trice, without bellows or furnace.

In the carver's and joiner's trades, the way of giving a shape to wood in molds, as we do to plaster of Paris and burnt alabaster.

I know, *Pyrophilus*, that such optatives may be thought but a civil name for chimerical projects; but I shall hereafter more fully declare to you, why I think it not altogether useless, that such optatives should be proposed, provided, as I hinted above, that they be very difficult, and not impossible; that is, that they be such as are not repugnant to the nature of the things, nor the general principles of reason and philosophy, and seem no otherwise to be chymically or mechanically impossible, than because we want tools, or other instruments, and ways to perform some things necessary to the compassing of the proposed end, or to remove some difficulties, or remedy some inconveniences, that are incident to us in the prosecution of such difficult designs.

And let me here tell you, *Pyrophilus*, that this advantage may be derived from the devising of such optatives to bold and sagacious men, and if they despair of attaining to the perfection they are invited to aim at, they may at least endeavour to reach some approximation to it. Thus unsuspected eye-witnesses have informed us, that in some countries they are wont to shoe horses without the help of a forge, bringing their iron to such a temper, that, having a company of shoes ready made, they can easily hammer them cold, so as to fit them to the size of any horse's foot, which the heat of the climate, where this is used, makes the greater convenience. Nor do I much doubt, but, that by various tempers, iron may be made very soft and afterward hardened; and the rather because, as I elsewhere tell you, we have, without antimony or sulphur, melted it in a crucible, so as to pour it out like lead, and yet afterwards it grew harder than it was at first. So, that flexible looking-glasses may be made with the help of selenitis, you will elsewhere be shewn; as also to foliate with ease all kinds of hollow glasses, and so turn them into specula. That malleable solder may be made, though we have not yet performed it, we do not much despair, and by good silver-solder some approximation to it has been already made.

SUBMARINE navigation, at least for a short space, has been successfully attempted by the excellent *Cornelius Drebel*, as *Mersennus* assures us; and as I have been informed, both by *Drebel's* son-in-law, and by other judicious persons, that have had the account of trials from the very men that went in the vessel under water for a good while together; who affirmed, that though there were many in the boat, yet they breathed very freely, and complained not of any inconvenience for want of fresh air. And here also give me leave to take notice, that this inventive *Drebel* was no professed shipwright, nor so much as bred a seaman.

As for the optative proposed for the divers, I know one of them, who by a slight instrument, that is all under water, and has not, as others, any chimney open to the air above

above the surface of the water, has been able to stay divers hours at the bottom of the sea, and remove his respiratory engine (if I may so call it) with him; and *Mersennus* assures us; that a much better way, and in my opinion an admirable one, (if the thing be certain) was found out and practised in his country, by one *Barieus*, who was able to stay six hours under water, by the help of an almost incredibly scant proportion of air, and even to preserve, at the bottom of the sea, the flame of a lamp or candle, in a vessel not much bigger than an ordinary lanthorn.

As to the optative proposed in the assayer-master's trade, I shall in the next essay teach you a way of cupelling in small quantities, without a furnace, or coals, or ordinary cupel, or other vessel.

And I remember, that by way of approximation, I made a certain powder, with which, without a furnace, I have, in a trice, melted lead-ore (which very often holds silver) into metal, and perhaps consumed some of the baser metal too.

And lastly, as for the making of embossed works of wood in molds, I am credibly informed by a learned man, that it was actually performed lately at the *Hague* by the secretary of a foreign ambassador; but of the way I could not procure the least hint, though supposing the truth of the relation, I suspect it was done either by some menstruum, that much softened the wood, which may afterwards be easily hardened again, by which way tortoise-shell may be molded; or else, by reducing the wood into powder, and afterwards uniting the parts into one body with some very binding and thin kind of glue, whose superfluous parts may afterwards be pressed out. And I remember, I began (but was accidentally hindered to proceed) a trial to make an approximation to this by the help of a rare glue, of which I had the hint, without being much beholding to him for it, from the practice of an ingenious tradesman, which, as I now prepare it, is made by soaking the finest ichthyo-colla (i. e. isinglass) for twenty-four, or at least for twelve hours, in spirit of wine (or even common brandy, for the menstruum need not be very good, unless for some particular uses). When by this infusion the liquor has opened and softened the body (which will much swell) both the ingredients are very gently to be boiled together (and kept stirring, that the ichthyo-colla burn not, till all be reduced to a liquor, save perhaps some strings, that are not perchance very dissoluble) when it is boiled enough, a drop, suffered to cool, will soon turn to a very firm jelly, and whilst it is hot, it should be strained thorough a piece of clean linen into a glass or other vessel that may be kept well stopped; a gentle heat suffices to melt this glue into a transparent liquor with little or no colour, and yet this fine thin glue holds so strongly, and binds so very fast, that having sometimes taken two ordinary square trenchers (for the round ones are wont to be too thick) and laid the one a pretty way over the other, a little of this liquor put between them, and suffered to dry of itself, united the trenchers so fast, that when force was employed to break them, it did it elsewhere, not where they were joined together: so that it seems, the gluten that fastened the trenchers together, was stronger than that which joined the parts of the same trenchers to one another. The other uses of this jelly (which by reason of the spirit of wine will not easily corrupt like other jellies) belong not to this place. Only I shall add to our present purpose, that having taken some common saw-dust, and after having imbibed it with melted glue, strained out slightly what was superfluous through a piece of linen, and shaped the rest with my hand into a ball, this negligent trial (which was only made to see whether a more accurate might be hopeful) made the ball, after it had been leisurely dried, so hard, that being thrown several times against the floor, it rebounded up without breaking; but as I was saying, an accident hindered me from prosecuting the experiment, which therefore I recommend to you.

I WILL



I WILL not now stay to tell you, *Pyrophilus*, how it may assist you toward the making such approximations, as we have been speaking of a little above, to take each of the difficulties, you would surmount, into the several parts it may be conceived to consist of, and make an enumeration of the possible ways of mastering each of these, according to some methods that might be proposed; because to discourse of this subject would take up too much of the time allotted to the following essays, and therefore I shall conclude this, by observing to you, that as you are, I hope, satisfied, that experimental philosophy may not only itself be advanced by an inspection into trades, but may advance them too; so the happy influence it may have on them is none of the least ways, by which the naturalist may make it useful to promote the empire of man. For that the due management of divers trades is manifestly of concern to the publick, may appear by those many of our English statute-laws yet in force, for the regulating of the trades of tanners, brick burners, and divers other mechanical professions, in which the lawgivers have not scorned to descend to set down very particular rules and instructions.

## A P P E N D I X.

**H**AVING in the foregoing essay mentioned a way of making spherical, and other hollow looking-glasses, with an intimation that it should not be a secret to you, I shall no longer delay to acquaint you with it, partly, because, though it may seem but a curiosity, yet it may not prove useless to you in making, very easily, divers catoptrical experiments, that are otherwise difficult enough; and partly, because trial hath informed me, that some ways prescribed, of thus foliating glasses, were much inferior to what was pretended. And even in a recent and famous writer, I lately found a process of performing this, which when I had read over, I foretold it would not succeed, which prediction was soon verified by experience; and indeed they that know the way and difficulty of foliating much more tractable glasses, than hollow ones, will scarce wonder, that it should not be found a very easy matter to foil, especially without heat, spherical, cylindrical, and other concave glasses on the inside, to which the figure of the glass prohibits ordinary foils to be fastened: yet a mixture, that by the success appeared to be fit enough for such a purpose, I chanced to see employed by an illiterate wandering fellow I met with in the country, the consideration of whose practice did, I confess, suggest to me another mixture, that I afterwards several times tried, and found it to foliate not only spherical glasses (to which he confined himself) but other concave glasses, at least as well as his, if not better, which he held for a great secret, and which indeed excelled any I have met with in print. To give you then the way I have practised myself, take tin and lead, of each one part (by weight) melt them together, and forthwith add of a good tinned glass (or bismuth) two parts, carefully skim off the dross, and afterwards, taking the crucible off the fire, before the mixture grow cold, put to it ten parts of clean quicksilver, and having stirred all well together, keep this foliating liquor in a clean new glass for use. When you would employ it, strain it through a clean linen cloth, to sever it from dross, and then by the hole of the spherical or cylindrical glass, put in a long and narrow funnel of paper, reaching almost to the bottom of the glass, that the falling liquor may not sputter to the sides. By this funnel you must softly pour in some ounces of the mixture, and then.

then dexterously and leisurely inclining the glass every way, endeavour to make it fasten on all the inward cavity thereof: this being done for the first time, and the vessel being laid aside for some hours, that the soil may the better stick to it, it is best to take it in hand again, and after the former manner frequently, but slowly, pass the liquor over those parts of the glass, which by holding it against the light you shall discern not to have been sufficiently foliated the first time; afterwards the glass being again laid aside for some hours more, the former operation is to be reiterated once (or if it be needful twice) more, until you find the glass equally and sufficiently foiled, which, when you perceive it is, you may gently pour out the superfluous liquor, to be reserved for the same use in other glasses. Lastly, with a cloth well sprinkled with putty or scraped tripoli, or for need, powdered chalk, the outside of the glass must be carefully rubbed, to take off the foulness it may have contracted by being handled, and to make it look clean and polished.

THIS way I have made use of in glasses of several sizes and figures, and preferred before that, which I remember, I once saw tried, and was ascribed to a learned Italian, one *Caneparius*, as being more easy than it, and more safe in regard ours need no arsenick. I found it also much better than another, which is kept as a secret and highly esteemed, because though the ingredients, abating the tin, be the same in both, yet in the way already delivered, the liquor or amalgam being used cold, there is no danger of breaking the glass to be poleated, or mistaking the degree of heat to be given it, to both which inconveniencies trial taught me, that the other way is obnoxious.

II. AND on this occasion it will not be amiss to acquaint you, that I made this improvement of our way; that having made the outside of glasses so foliated very clean, I have (by laying on very thinly such a kind of varnish, as that yellow one elsewhere described, as fit to make gilt-leathern hangings) made them appear richly gilt, and yet so bright and polished, that they would, notwithstanding this gilding, serve very well for looking-glasses.

III. WHAT other improvements I made of this experiment, I must not here insist on, especially that I may comply with the haste, which obliges me to omit what I had thoughts of annexing here about varnishes; so that though I have made many trials (whereof another time you may command an account) about several sorts of them, some, that emulate gilding upon metals as well as leather, others, that imitate and diversify (if not also excel) the China varnish, and others designed for differing purposes, yet I can at present only tell you in general, that they are an useful, as well as ornamental, sort of productions, and capable (if I mistake not) of much improvement.



OF DOING BY

## PHYSICAL KNOWLEDGE

What is wont to require

## M A N U A L S K I L L.

O R,

That the Knowledge of Peculiar Qualities, or Uses of Physical Things, may enable a Man to perform those Things Physically, that seem to require Tools and Dexterity of Hand, proper to Artificers.

THE particulars to be mentioned in this eighth essay might have been ranged partly under the preceding discourse, and partly under the eleventh essay, (which will be the last of this treatise,) whose titles are comprehensive enough to take in the instances, that make up this present discourse; which yet I have rather chose to deliver apart, not only because they seem somewhat differing from the examples alledged in the two mentioned essays, but chiefly because the uses, that may be made of such instances, may make them deserve a distinct and peculiar mention. For it is both a notable argument of the industry of mankind, and may prove a great encouragement to it, that the help of philosophy may supply the office of manual dexterity, strength, or art; and a knowing head may do what is thought not performable but by a skilful hand, or an arm assisted by some instrument or engine. And of these instances (which may be justly looked upon as so many trophies of human knowledge, and so many incitements to human industry) it will be needless to make any division; and therefore I shall barely set them down as they come into my mind, no other order being necessary for particulars that are brought but as proofs, and have not a dependency upon one another.

THE assertion, that makes the title of this discourse, the king of *Spain* finds true so much to his advantage, that, if I mistake not, it amounted for a good while to divers

millions yearly. For whereas formerly in the silver-mines of *Potosi* in *Peru*, (accounted the richest in the world) it was wont to be a very tedious, laborious, and consequently chargeable work, to sever the silver particles of the ore from the ignobler parts of it, by many slow and costly, both manual and metallurgical fusions, and other ways of segregation, much of that labour is now saved by *Pero Fernandes de Valesco*, who, as *Acosia* informs us, first made use at *Potosi* of the property of quicksilver to amalgamate with the nobler metals. For now, by accurately grinding the powdered and seared ore with quicksilver (strained through a cloth) and salt, and decocting them for five or six days, in pots and furnaces fitted for the purpose, the greedy mercury licks up the silver and gold (which it sometimes meets with) without meddling with the ignobler parts of the ore; and being enriched with as much of them as it can imbibe, and diligently washed from the adhering fordes, the amalgam is, by distillation with a strong fire, freed from the mercury; which coming over revived into the receiver, leaves behind it the fixed metals, viz. gold and silver, which may be afterwards (if need be) easily reduced into bodies, and parted by the common way. And by a not unlike way some of our goldsmiths and refiners are wont (as themselves inform me) to regain out of the dust and sweepings of their shops, the filings and other small particles of gold and silver which fall to the ground in their operations, and in process of time may amount to a considerable value.

To make an head, exactly representing the size, shape, and lineaments of the face of any living man, seems to require an exquisite skill in the statuary's art; and yet at my desire, and in my presence, that was lately performed by a tradesman after the following manner: the party, whose face was to be cast off, was laid flat upon his back, having round about the edges of his forehead, his cheeks, and his chin, something placed to hinder the liquid plaster from running over on his hair: then into each of his nostrils was put a hollow piece of stiff paper, of about a quarter of a foot long, and of the figure of a sugar-loaf, and open at both ends, that the affusion of the plaster might not hinder him to take breath. And of these pipes, which were carefully oiled over, the acuminate extremes rested upon his nostrils, and the other were supported by one of the assistant's hands. Then his face being lightly oiled over, to hinder the plaster from sticking to it, with oil olive, and his eyes being shut, alabaster newly calcined in a copper-kettle, till it was as white as before, was tempered up with fair water to the consistence of batter, and by spoonfuls nimbly put all over his face, till the matter lay every where near an inch thick. Almost as soon as it was all laid on, it began to grow sensibly hot, and in about a quarter of an hour hardened into a kind of lapideous concretion; which being gently and easily taken off, shewed us, in its concave surface, the exact impressions made there by the parts of the face, and even by the single hairs of the eyebrows. In this mould they cast a head of good clay, by working it in, and on that head they open the eyes, which in the prototype and mould were shut, and, if need be, heighten the forehead, and make what other amendments they think fit; and anointing this new face with oil, they after the former manner make a second mould (of two parts, contiguous all along the ridge of the nose) with calcined alabaster, and in this second mould (lightly oiled on the inside) they cast with the same matter the fore-part of an head, more like the original, than ever I saw made by the most skilful statuary, and yet with so much ease, that the very first trial I made myself to cast a face thus, succeeded.

To take the impression of a leaf, or other flattish part of a plant, it may seem requisite that a man be a good painter; and yet I found, that the thing may be performed, only by holding a whole leaf (or sprig of rosemary, &c.) in the smoke of a piece of common gum sandarack, resin, camphire, or some such body, that emits a copious and fulgi-



nous steam (for which purpose I have made use of a common link, when that was most at hand); for the leaf being well blacked by these fumes, and placed betwixt the leaves of a sheet of white paper, if you carefully press the paper upon the leaf with the haft of a knife or some other smooth thing, you may thereby print on the paper in a few moments the exact size and figure, but not colour, of both sides, but especially the back-side of the leaf, with the very ramifications of the fibres that are disseminated through it. And this may be performed, though not so lively, by blacking the plant, whose picture is required, with the fumes of a candle or taper (especially if it be of wax) instead of those of the aforementioned resinous concretes, and afterwards proceeding as in the former experiment; which sometimes may be of good use to you, when you turn botanist, and in your travels meet with plants, whose pictures you think worth having, but have not time or conveniency to draw them.

ANOTHER instance, of the same import with the foregoing ones, may be afforded us by the art of etching, whereby copper and silver-plates may be enriched with figures, which may seem to have been made by the tool of some excellent graver; and yet those engravings do not require the presumed manual skill, and are made without such tools, by having a peculiar sort of varnish (for on the goodness of that depends much of the success of the operation) on the plates, and drawing on it the figures to be engraved. For all those lines, where the plate is freed from the varnish, by skilfully tempered aqua-fortis (from whose corrosive violence the remaining varnish secures the rest of the plate) may be so curiously wrought on by those few artists that are skilful in it, that I have very seldom seen lovelier cuts made by the help of the best tempered and best handled gravers, than I have seen made on plates etched, some by a French, and others by an English artificer.

BUT the knowledge of the physical properties of things may sometimes enable a man to perform, not only things to which mechanical tools and manual dexterity seem to be necessary, but some things also, whereto even mathematical instruments, and skill in mathematicks are thought requisite; of which I shall at present propose a couple of instances.

IN the elsewhere mentioned French abridgment of *Galileo's* Italian book \*, I find a passage very pertinent to our present design, which agreeing very well with our observation of that kind, we shall propose it a little more clearly, as follows:

SUPPOSE in a high church (the book exemplifies *Nostre Dame*) the great candlestick that hangs from the top of the church being made to swing, a philosopher that has observed that the vibrations of a pendulum, though the arches it describes be unequal, are in the sense formerly declared equitemporaneous; and that, when the strings at which such pendulums hang, are very unequal, their lengths will have the same proportion as is between the squares of the numbers of their single vibrations performed in the same time; suppose, I say, that such a person have a pendulum with him, whose string (which may be of any length, so it be determinate) is, for example, a yard long, it will not be difficult for him, without any quadrant or geometrical instrument, to find out the length of the string that supports the candlestick, and consequently the height of the church. For the candlestick and the short pendulum being made to swing, beginning both at the same time, let us suppose, that when the candlestick has made nine vibrations, the pendulum of a yard long has made 54, the squares of these two numbers will be 81 and 2916; and because, as we lately said, the length of the pendulums will have the same proportion with the squares of the number of their vibrations, dividing 2916 by 81, the

\* *Nouvelles pensées de Galiléo* liv. I.

product will be 36; which shews, that the string at which the lamp hangs is 36 times as long as that of the shorter pendulum, and consequently a yard, containing three feet, amounts to 36 yards, or 108 feet.

UPON the knowledge of another physical property of heavy bodies, I remember I have grounded a way to measure vast heights and depths without any geometrical instruments, and in such cases where such an instrument cannot be employed, by the help of a pendulum; which, because in this case it must be very short, will require an attentive and expert observer. For it being known, that a stone, or a piece of lead, or the like solid weight, falling from a height, does so accelerate its descent, that the differing spaces it has transmitted at any differing times assigned, will have betwixt the same proportion with the squares of the times, wherein the respective spaces were transmitted; if it be once known by diligent observations, how far a stone, or such a solid body (whose greater or lesser bulk is not here considerable) does fall at the end of the first second-minute of its motion downwards, it will be easy enough for a naturalist versed in the doctrine of proportions, to collect, from the time that the stone employs in descending perpendicularly from the top of a high tower or steeple, how high that building is. This way of measuring, provided attention and accuracy be not wanting, we found agreeable enough to divers observations of our own and our friends; and by this way, one may measure the depth of a well (to the surface of the water) how deep soever, though the bottom, as it is usual, by reason of the darkness, cannot be seen, which makes the depth unfit to be measured by quadrants and such like geometrical instruments. For if at the same time that you let fall a stone or other weight, you also let go a pendulum that vibrates quarter-seconds, that is, makes two excursions, and as many returns in the sixtieth part of a minute, and reckon its vibrations, till you hear the noise made by the stone dashing against the water in the bottom of the well, you may easily enough collect the depth. For let it be supposed, that it be found by experience, that a falling stone, or other like weight, do in the first second-minute of its descent dispatch (as the diligent *Mersennus* affirms himself to have often found) 12 feet (which I understand of French, not having found it hold in English measure) and let us also suppose the pendulum to have perfected six single vibrations before the dashing of the stone against the water was heard; if we proceed according to the rule formerly given, we shall find, that if the time wherein the falling stone transmitted those spaces, that are to direct our calculation, be one and six, the square of those two numbers being 1 and 36, the stone must have fallen at the end of the sixth second 36 times as far as at the end of the first. And since by observation (about whose accurateness we need not be solicitous here, where we design only the giving an explanatory example) a falling stone in the first second descends 12 feet, we need but multiply 36 by 12, to obtain in the product 432, the perpendicular depth of the well to the surface of the water. And the same number may be collected, and perhaps you will think more easy, by supposing, as *Galileo's* experiments seem to prove, that a falling body accelerates its descent according to a progression of odd numbers, beginning from an unit; so that, if in the first second minute, or any other determinate part of time, it falls one space, whatever that be, in the next second it will fall three spaces, and in the third five spaces, and so onwards; according to which reckoning, if the falling body be supposed to descend 12 feet during the first second, it will descend 36 (besides the former 12 in the next second) in the third 60, in the fourth 84, in the fifth 108, in the sixth 132, which, summed up together, amount to 432. And, by the same way, one may measure the height of divers precipices how great soever, as far as one can reach downward in a perpendicular line. And one may also give some guess at the depth of some volcanos, which are not accessible to those that know but the common ways



ways of mensuration, or which have burned the ropes, and even melted down the chains and weights, by which some curious persons have attempted to fathom their depth. It is true, that in mathematical rigour, some abatement ought to be made, because the stone strikes the surface of the water, or the bottom of the precipice, some little while before the sound, produced by that stroke, can arrive at our ears. But unless the height or depth to be measured be very extraordinary, this allowance, for the delay of the noise, either may be neglected without much inconvenience, or in probability will scarce exceed a quarter (or at most half) of a second; since, as has been elsewhere noted, it has been found by observation, that a sound in the air moves above twelve or thirteen hundred feet in one second. And in what I have here delivered concerning the way of measuring depths and heights by the falling of a heavy body, I have been much confirmed by an observation I chanced to meet with in an outlandish book, which I have not now by me to look out the place, where the mathematician that writes it, who seems to have been a diligent observer, affirms, that he found a weight let fall from the top of a church, or steeple (for I remember not which, nor is it material) so high as to amount to 300 feet, to reach the ground in about five seconds; which agrees very well to what we have been delivering. For supposing the weight to fall 12 feet the first second, at the end of the fifth second it must have fallen 25 times as far, (1 and 25 being the squares of the numbers of the seconds of time) and consequently 300 feet.

To slit (or divide transversely into flakes or leaves) so thin a piece of metal as an old groat, which seems not to exceed, if it so much as equal the thickness of a leaf of white paper, may be thought, if it be feasible, to require some very subtle dividing instrument, with an edge finer than that of a razor; and yet the way of performing this by physical means is but an almost ludicrous experiment, which (if you know it not already) is easily thus made. Take three pins, and stick them in the form of a triangle, at such a distance from each other, that the groat may rest upon the heads of them; put upon this thin piece of metal almost as much flour of brimstone, or, at least, finely powdered sulphur, as will conveniently lie on it; then kindling the sulphur, let it burn out of itself; which done, take off the groat, and throwing it hard against the floor, the upper part, with the adhering remains of the sulphur, will be parted from the lower; which (lower) if the coin were not very thin, will retain its former shape. I have observed in this experiment, a pretty circumstance or two, the knowledge of which is very apt to be misemployed, and need not here be mentioned; though I would not silently pass by the experiment itself, because as ludicrous as you may think it, it may suggest uncommon speculation to a considering naturalist, and also intimate a way of preparing silver, of which I may elsewhere tell you the practical use.

He that takes notice of so pretty a variety of colours and shapes as may be discerned on a skilfully made sheet of marble-paper, will be apt to conclude, either, that the differing colours were laid on one by one with a pencil, which would require a great deal of time and pains; or that the sheet was marbled by being printed off from some plate, on which the differing shapes were cut or engraven, and the differing colours singly placed, which would require yet more labour, and a greater apparatus; whereas the whole sheet is painted thus variously and delightfully at once, and in a trice, by the contact of the surface of a vessel full of water, on which the colours (first blended a little, by a quick and easy motion of the artist's hand) are so ordered as to swim without being confounded. This artifice hath, as I am informed, been delivered by the curious *Kirckerus*. But if you have a mind to know the particulars of it more fully, you may command me to acquaint you with what I have learned from experience, by which the practice is supposed to have been of late improved.

If it were propos'd to free weak spirit of wine, or aqua vitæ, from a great part of its phlegm, the generality of distillers would think it not to be effected but by the help of fire and a furnace, an alembick, or some other distillatory vessels; and yet, without the help of any of all the'e instruments, I have sometimes taken pleasure to dephlegm brandy (as they call weak spirits of wine of the first distillation) only by putting it into salt of tartar. For considering the faculty this alkalizate body has to attract (as men commonly speak) or imbibe the aqueous particles that swim in the air, and resolve itself, with them into that liquor that the chymists call oil of tartar *per deliquium*, there seem'd sufficient reason to expect, that the same salt, being put very dry into phlegmatick spirit of wine, would embody with the phlegmatick parts, with which, if it were not overcharged, it would probably keep them separate from the more spirituous liquor; since such oil of tartar as I have just now mentioned, and dephlegmed spirit of wine, will swim upon one another without mixing; and accordingly, I have sometimes taken pleasure, by putting a sufficient proportion of dry salt of tartar into brandy, and leaving it there for some time (for the experiment will, to be completed, require some while) to make some separation of a great part of the phlegm, which by degrees dissolving the salt, will reduce again part of it into a liquor that will keep its surface distinct from that of its supernatant spirit, and if confounded therewith, by the shaking of the glass, would speedily part from it, and regain its own station; and if you would have a separation of the phlegm begin to appear quickly, you may compass what you intend, by tying up a convenient quantity of dry salt of tartar in a dry rag of linen cloth, and immersing it a little while in the brandy, and then lifting it up a little above the liquor; for the phlegmatick parts being copiously imbibed in the salt, which will be thereby resolved into a ponderous liquor, will in drops (whose descent will be distinguishable enough, if the glass be held against the light) fall to the bottom of the spirit of wine. And lest you should suspect, that this descent comes not from their weight, but from the force they acquire in falling through the air, you may keep the rag immersed beneath the surface of the liquor, and yet may perceive the efflux and subsidence of the lixivium we have been speaking of.

THERE are some cases, wherein bodies that are to be held very softly, are either so brittle that it would be hard to hold them fast enough without danger of breaking them; or else so small, and so inconveniently shaped, that it would be very difficult to procure instruments to lay hold on them, and keep them moveless in the instrument; and in several such cases, the use of tools, to hold fast such bodies, may be advantageously supplied by artificial cements. As I remember I have known the glass grinders, instead of more mechanical tools, employ pitch, melted and made up with ashes, very well stirred and incorporated with it, into a stiff paste. For this mixture, being by a fit heat brought to a convenient softness, the glass to be ground or polished is bedded in it, in what posture and as far as the artificer pleaseth; and by the same mixture the glass being fastened at the end of a stick or some proper instrument of wood, the glass, upon the cooling of the cement, remains firmly fastened, until the artificer have done with it what he designed; after which, by softening the cement with heat, he can readily take it off again.

AND even the diamond-cutters, who, to grind those stones into shapes are wont to employ a very vehement attrition, make use, for holding their diamonds, especially when they would polish them, of a cement, the like to which I remember I have sometimes made to other purposes; for themselves have confessed to me, that they made theirs chiefly of resin, melted and brought to a stiff paste with fine brick-dust, to which



one of the eminentest of them for skill adds a proportion of sealing-wax (I told him I preferred plaister of Paris before brick-duft, and he told me he did the like).

AND indeed by variety of cements we may be assisted to make divers experiments that we could not otherwise make so well, if at all; for which reason I have been somewhat curious about making a pretty number of such mixtures, whose compositions you may command of me.

THERE are divers artificers, especially those that slit and polish crystal, agates, and other hard stones, and cut seals in gems, who have need of powders of emery, of differing degrees of fineness, and some of them extremely subtle; to obtain these, one would think it necessary to have variety of searces, and some of them as fine as it is possible. But the skilfullest artificers judge they can obtain their desire much better by fair water, than by the best searces. For having in a mortar beaten the hard body of emery as long as they think necessary, they put the powder into a pail or other fit vessel full of water, and then with a stick, or some such thing, they stir very well all that is at the bottom, that it may be raised and thoroughly mingled with the liquor; then pouring it out into another vessel, the grossest and the most ponderous grains of the dispersed powder will first fall to the bottom, and give a powder less gross than that which remained in the first vessel, which may be again beaten small in the mortar. Afterwards they pour the troubled water of the second vessel into a third, and there suffer the dust to subside, and then decanting the liquor, if this dust be not yet fine enough, they trouble the water again, and after a little while pour it off either into one vessel, or two, or more, successively, according to the exigency of their uses; and then suffering the transfused water to settle for some hours (more or fewer) as the dispersed dust is more or less light, they decant the liquor, or suffer it to exhale, and take the remaining powders, of which that which settles slowest, will oftentimes be strangely subtle. And by this way, if a man will have patience to pour successively the troubled liquor into vessels enough, and give the dispersed powder a competent time to let fall the less light parts, before the upper part of the water be poured off into the vessel it is finally to settle in, he may obtain several degrees of powders, less and less gross, and some so fine, as one would admire how it was made so. And this (*Pyrophilus*) I the rather mention to you, because it is not only from emery, but from divers other bodies, that one may obtain extremely minute, and (as they speak) impalpable powders, of great use in some of the most curious trades, and perhaps in physick too. For I may elsewhere tell you how I apply this way to magisteries of crystal, and of gems, and even to *Crocus Martis*; the naming of which last puts me in mind to add, that a chymist, much prized for finer *Crocus Martis*, than others of his profession, and thereby enabled to sell it at an extraordinary rate, confessed to me, that it was to the artifice I had been commending that the *Crocus* he sold owed all its advantages.

IT has long been, and still is in many places, a matter of much trouble and expence, as well of time as money, to cut out of rocks of alabaster and marble, great pieces, to be afterwards squared or cut into other shapes; but what by the help of divers tools and instruments cannot in some quarries be effected without much time and toil, is in other places easily and readily performed, by making, with a fit instrument, a small perforation into the rock, which may reach a pretty way into the body of it, and have such a thickness of the rock over it, as is thought convenient to be blown up at one time; for at the farther end of this perforation (which tends upwards) there is placed a convenient quantity of gunpowder, and then all the rest of the cavity being filled with stones and rubbish strongly rammed in (except a little place that is left for a train) the powder, by the help of that train being fired, and the impetuous flame being hindered from expanding

ing itself downwards, by reason of the newly mentioned obstacle, concurring with its own tending another way, displays its force against the upper parts of the rock, which, in making itself a passage, it cracks into several parts, most of them not too unwieldy to be manageable by the workmen.

AND by this way of blowing up rocks a little varied and improved, some ingenious acquaintances of ours, employed by the publick to make vast piles, have lately (as I received the account of themselves) blown up or scattered, with a few barrels of powder, many hundred, not to say thousand tons of common rock.

To give small glasses the shape that is requisite to fit them to serve for covers to the dial plates of watches, and for other purposes to which artificers sometimes employ them, one would think it necessary that they should be ground or otherwise wrought with tools, by a skilful hand, to give the glasses the concave, as well as the convex figure they ought to have. And yet I have learned by trial, that a flat plate of glass of a competent thickness that has its two surfaces smooth and parallel to each other, being carefully laid upon a deep ring of iron, or a shallow and hollow cylinder of the same metal, and of the diameter required, so that the edge of the glass (which is to be reduced to roundness) may every where rest upon that of the cylindrical piece of metal; the heat of the fire warily and skilfully administered will so soften this plate of glass, that its own weight will so depress the middle parts, that the glass will thereby obtain the figure required. And though such glasses do not constantly fall just into the desired figure, yet when they are skilfully ordered, they fall into it so often, that I am told that some ingenious artificers have quitted the ordinary way of making covers for watches, for that we have been describing; which, though not free from casualties, is yet so much more cheap and easy.

WE have in some parts of *England* various kinds of talk, or *lapis specularis* (several of which I have been possessor of) and of some of them there is so great plenty, that one may procure good store for little or no charge; but the reducing of a great lump of this talk to fine powder, if it must be done the common way, by beating it in mortars, and searcing it often, will require much time and pains; but as I have several times tried, the smaller pieces may, by the help of an actual flame, be quickly reduced to a snow-white calx; so by the experiment of a sagacious acquaintance of mine, even great lumps of it may, almost in a trice, be brought to fine powder, by heating them red-hot, and casting them, while they are so, into cold water, whereby there will presently be made a comminution of them into a fine, and, as it were, mealy calx.

THE ground of this operation is much the same with that, whereby some chymists granulate masses of gold and silver, when they pour the strongly melted metal from a competent height into cold water, whereupon there happens a dissilution of the parts of the metal; many of which fall to the bottom in little fragments. But the more easily fusible metals, tin and lead, may be quickly and better granulated by the mechanical way, freshly mentioned, as to talk. I remember I was wont (especially if the ignition and extinction were repeated two or three times) to reduce crystal flints, almost in a trice, to a fineness to be easily brought to a very subtle powder, proper to make amaules (or counterfeited gems) of.

THE mention I have already made in this essay, of what may be performed by the faculty, that burnt alabaster, made liquid with water, has to grow hard again, puts me in mind of another instance, very properly referrible to the subject of this essay. For one that beholds how curiously oranges and lemons, and other fruits are counterfeited in wax, would imagine that so lively a representation of them could not be effected but by a hand as skilful at least as that of a painter; since by this plastick art, not oranges, and lemons,



lemons, &c. in general, but this or that particular orange or lemon may be most lively represented; and yet you may learn this art within one hour or two, the thing being performable easily and quickly; for having the orange, &c. we would imitate, we bury it half way in a coffin of clay, whose brim, together with the extant part of the fruit, being oiled over to keep the mixture from sticking, the tempered alabaſter (or plaſter of Paris) is nimbly laid on to a good thickneſs, and, upon its concretion, removed, whereby you obtain an half mold for that part of the orange; then the formerly latent part of the fruit being likewise placed uppermoſt in the half-mold, which ſhould have ſome pretty deep notches cut in the rim of it, which, with the protuberant part of the fruit, ought to be oiled, the tempered mixture is likewise put upon that, and thereby an exact mold is completed, at any convenient part of which a hole being made, to pour in a little tempered and coloured wax, when it is brought by fuſion to a due heat (for every degree of that quality is not convenient) ſhaking the mold nimbly and every way, the wax comes to be ſo applied to the internal ſurface, that when the mold is cold, and the parts taken aſunder, you have an orange of wax very lively repreſenting the original.

THERE are ſome circumſtances belonging to this eaſy and delightful art of molding and caſting in the wax (which is pleaſant enough to be praſtiſed even by ladies) that I purpoſely omit; what has been mentioned being ſufficient to ſhew you as much as is neceſſary for my preſent purpoſe. And I the rather pitched on this experiment, becauſe it may afford you another inſtance not impertinent to the deſign of this tract. For one that ſhould ſee how great a cavity is left within the counterſeit orange, would think that there were ſome great and rare artifice requiſite to caſt it thus hollow, and make ſo ſmall a quantity of wax reach to the counterſeiting of ſuch a fruit; whereas the bare ſhaking of the mold, when the melted wax is in it, together with the expaſive endeavour of the included air, applies the wax to every part of the inſide of the mold, and thereby turns it into one great film, which one would think it very difficult to ſeparate, without injuring it, from the mold, to which it is applied ſo cloſe; and indeed it might be ſo, if nature did not again aſſiſt the artiſt, by making the mixture, when it cools, ſhrink a little, and thereby part eaſily from the mold it ſtuck to.

BUT one of the prettieſt and the ſtrangeſt artifices that belong to this eſſay, is that, whereby the knowledge of a few unheeded phyſical properties of two or three bodies may enable a man to perform that, which ſeems to require not only good tools and great dexterity in the art of graving, but likewise an exquisite ſkill in caligraphy or the art of writing fair; for I know a graver, famous for ſkill in his profeſſion, who writes, as I have had good opportunity to obſerve, but a bad hand; and yet this man with his tool writes rarely well, and will imitate and emulate the fineſt copies of the choicest writing-maſters, ſo that even virtuofi have much admired how a man, with a ſtiff iron tool upon a tough copper-plate, can write incomparably fairer than the ſame perſon can with a good pen upon paper. But to eaſe you ſomewhat of your wonder, I ſhall add, that though this artifice be kept for a choice ſecret, and though I could not learn a conſiderable particular or two, which belong to the delicacy of it; yet (partly by putting queſtions, and partly by ſome trials of my own) I attained to the ſubſtance of this myſtery, as they call it, which ſeems to be this.

A WRITING-MASTER, or ſome other that writes a very fair hand, is deſired to write a copy, or what elſe is to be engraven, with a peculiar kind of ink, which differs not in ſhew from common ink, being fully as black as it: then they take a very clean and well-smoothed copper-plate, which being moderately warmed, is to be ſo rubbed over with a certain white varniſh, or ſomething equivalent (to be mentioned a little beneath) that when the plate grows cold again, it may be thinly and evenly caſt over with a kind of ſkin or film (if I may ſo call it) of varniſh; then lightly moiſtning the paper, that it

may part with its ink the more readily, the written side is to be laid on the prepared side of the plate, and that, together with the paper, being passed through a rolling-press, enough of the ink will stick (but in an inverted posture) to the varnish, whose whiteness renders the black letters very conspicuous; so that it is easy with a needle fitted with a wooden handle, to draw over the very same lines and strokes through the yielding varnish upon the metalline plate, whence they may, after the plate is by heat, or otherwise, freed from the varnish, be completed with a graver; and lastly, when the whole engraving is finished up, may be printed off in a rolling-press like ordinary cuts. And even without a rolling-press I have sometimes taken off written characters, only by laying the moistened paper very smooth upon the varnished copper, and rubbing it hard thereon with a convex piece of glass or some such smooth and hard body, whose pressure makes the ink stick to the varnish, for which I have used the purer sort of virgin wax, if the ink be good, and have been laid on plentifully enough by the pen. That ink, which I most used, I made only of fine Frankfort black, as the painters that sell it are wont to call it; by grinding it little by little, but very diligently, with water, till it had attained the consistence of a somewhat thick ink; in which this only circumstance is carefully to be observed, that no gum be added, as is usual in other inks, lest that hinder its coming off.

AND here it will not be impertinent to the argument in hand to add another artifice, whereby a printed cut may be so far taken off, that at least the out-lines and the principal strokes may be ready copied for the graver's hand; by which way, besides other uses that may be made of it, copies of rare and choice pieces may be procured, and the perishing or want of the originals supplied; if then the print to be taken off be recent enough (as it is wont to be, if it exceed not a year, or perhaps two) then the paper needs only be well moistened, as if it were to be printed off at a rolling-press (with the ink proper to which it is supposed that the cut was, as usually cuts are printed off): but if the picture or scheme be more ancient, it must be laid all night to soak in water, and then hung in the air till it have but such a degree of moisture as makes it fit for the rolling-press. The paper being thus prepared, either by bare wetting, or by steeping, the printed side is to be laid upon a copper-plate, thinly cased over, as was formerly directed, with virgin-wax; for the plate and paper being put into a rolling-press, the compression of that will make the moistened ink stick to the pure wax, which consequently will take the impression of the cut, or at least of the outlines and chief strokes of it.

THERE is another thing which seems above all these to require the express and immediate operation of the hand, and it is a physical way, if I may so speak, of transcribing a whole page of a letter, or other writing, all at once. Whether this can be performed cheaply and easily enough for common use, is hereafter to be considered. But that, abstracting from these circumstances, it is possible to be done (by an artificial application of physical things) I have been persuaded by some experience; of which I may in one of the following papers give you a more particular account than I now conveniently can.

In the former part of this essay, *Pyrophilus*, I have presented you some instances wherein physiological knowledge may be substituted for manual dexterity, mechanical tools, and even mathematical instruments; but now to shut up this discourse, I shall subjoin a relation that will manifest, that even a mathematician and an engineer may sometimes perform that by the knowledge of a slight physical quality of obvious bodies, which, without that knowledge, all his skill in mathematical disciplines, and his vast and artificial engines, will not have enabled him to accomplish. For who would think, that by a comparatively few pounds of water (perhaps the moisture of the air in wet weather might have sufficed) a massy body of peradventure some hundred thousand pounds in weight



weight should be raised; and yet, that this was performed at *Constantinople*, is one of the remarkablest things I remember I met in the ingenious account of his voyage that is given by the learned *Busbequius*, ambassador from the king of the Romans to the Turkish emperor. His words are these. \* *De obelisco, cujus supra memini, qui est in hypodromo, sic Græci commemorant; à basi convulsus multis seculis jacuisse humi: tempore posteriorum imperatorum repertum architectum, qui operam suam in eo suæ basi restituendo deserret; illumque, postquam de pretio conventum esset, ingentem apparatus organorum ex trocleis & funibus præsertim instituisse, quibus lapidem illum ingentem erexerit, sublimemque eo evexerit, ut uno tantum digito abesset à dorso astragalorum, quibus imponi debebat, tum indicasse populum spectatorem oleum illi operam tanti apparatus periisse, magnisque denuo laboribus & impensis opus instaurandum: at illum minime diffusum perito à rerum naturalium scientia subsidio iussisse offerri immensam aquæ vim, qua multis horis in macinam illam injecta, funes, quibus obeliscus librabatur, sensim madefactos rigentesq; (ut eorum est natura) se contraxisse, sic ut obeliscum altius sublatum in astragalis statuerunt, magna cum admiratione & plausu vulgi.* And for confirmation of this narrative, it may be added, that the same thing is mentioned by good authors, as having been practised elsewhere; and a like story is allowed, and somewhere made an argument of, to another purpose, by that great master of mechanicks *Galileo* himself.

To catch any store of fish the ordinary way, you know it is customary, that even in rivers, either store of angles, and some skill in using them, or nets, or some other artificial instruments be made use of; and if it be in the sea that men are to fish, large nets or some peculiar contrivances are employed as necessary; and one would not expect from such people as the Americans, easier ways of fishing than these, and yet these illiterate barbarians, by having found out (probably by chance) the physical property of a wood, make that serve them to catch fish in great plenty, and with as much ease. For our late English navigators have observed, as their voyages witness, that in some parts of the *West-Indies*, the natives, by impregnating the water with this wood, do so stupify the fish, that rolling up and down upon the surface of the water, as if they were foxed, they are easily taken up in great numbers in their hands; which relation of our seamen I therefore, notwithstanding its strangeness, scruple not to alledge, partly because, that though we do not use a simple drug, much less a wood, for the same purpose, yet our foxing-stuff, as they call it, which is but a slight composition, produces effects not much inferior; and partly, because having purposely inquired of a learned physician that came not long since out of a part of *America*, where this practice is in request, he assured me, that he saw the English themselves use this way of fishing, only by tying a log of this wood, to which, for what reason I know not, they have given the name of dogwood, to the stern of their boats; so easily does the odd property of this wood enable them that make use of it, to catch fish.

To take off the hair is generally supposed to require both a razor and other implements, and the manual skill and operation of a barber, especially if the hair be grown under the arm-pits, and in other places, which an inconvenient situation or figure makes to be of difficult access; and yet by the knowledge of a property of that natural production, formerly mentioned in the sixth essay, under the name of *Rusina*, the hair may be, without instruments, taken off from any part of the body, and that not only in much shorter time than is required to shaving, but, as far as the eye is wont to discern, by the roots, which makes it much longer before the part be again covered with hair of the former dimensions. The way used in the east to affect this the forecited *Bellonius*

\* Aug. Busbequii, Epist. I. pag. 69.

annexes, instead of which I shall tell you what I tried with a parcel of it, brought into England before I met with his observations about it. We mixed the fine powder of it with an equal weight of strong powdered quick-lime (*Bellonius*, probably not without reason, prescribes but half as much quick-lime) and having suffered them to soak together a short while in a little fair water, we thinly spread the soft paste or slime, made by the water and ingredients, upon that part of the body which we designed to free from hair; and having suffered this mixture to stay on about three minutes, or sixtieth part of an hour, measured by a minute-watch (our author prescribes as long time as is requisite to the boiling of an egg) we wiped it off with a linen cloth dipped in warm water, and found the hair taken off by the roots, without any inconvenience to the part that we could discern, though I several times shewed the experiment to others, and the trial of it was more than once made upon myself.

It may seem scarce possible, without the help of water, or any engine made with springs or wheels, to measure time, though but for a little while, as exactly as our best clepsydras, clocks, or watches are wont to do. And yet (which is now a known and almost vulgar thing) such an account of time may be kept by him, that has observed that the vibrations or diadroms of a pendulum are made in sensibly equal spaces of time, though the arches continually decrease, that are made by the swinging pendulum (as you know they now call a bullet, or the like weight hanging at the end of a string from a nail or other fixed supporter). For by so slight a thing as I have been mentioning, if you watchfully observe and reckon the returns that the swinging weight makes towards you in a minute or other determinate space of time, doubling the number of those returns, and adding thereto an unit, if you left off counting, when the weight was at the further end of the arch described by its motion, you may obtain a more accurate division of time, than by any of the formerly known ways of measuring it. For if you make your pendulum of the length of very little (perhaps a tenth of an inch) less than ten inches (or twelve parts of our English foot \*) accounted from the nail, or other thing, whence it is suspended to the center of the pistol-bullet (or the like small round weight); and, removing this a pretty way from the perpendicular it naturally rests in, suffer it to fall gently out of your hand, each of its two swinging motions (the one whereby it is carried from you, and the other whereby it returns to you) will be (especially whilst the arches are of a moderate length) physically *æqui-temporaneous*; and these motions will very distinctly enough, to an attentive eye, divide a minute or sixtieth part of an hour into an hundred and twenty parts (called half-seconds) and will consequently divide an hour into seven thousand two hundred parts, if not perfectly equal, yet less unequal, as to sense, than the divisions of time made even by good watches are wont to be. And therefore this way may be of very great use, in making astronomical and other observations that last not long, but require exact measures of time. And by the help of a pendulum, a skilful musician of my acquaintance teaches his unpractised scholars to keep time when they sing in his absence. But when we measure experiments by the excursions and returns of a weight, the best way is to make the duration of the pendulum's whole motion (before it come to rest) as long as the place where the experiment is made will permit, renewing now and then, if need be, the impulse given to the weight, when the arches begin to grow too short; that being increased, the vibrations may be the better reckoned.

\* N. B. The author has elsewhere shewn, that the English foot differs very little, if at all, from the ancient Romans.



THE mention I have been making of the uses of pendulums, joined to that I lately made of æquivelocity of sounds, brings into my mind another instance pertinent to this part of our discourse. For it is not impossible, by the knowledge of the velocity of a sound's motion in the air, and the æquivelocity (as to sense) of great and small sounds, to measure without geometrical instruments, in some cases, the breadth of a river, though exceeding wide, or the distance of the place one stands in, from the top of a high tower or hill on the other side of a river, or situated in some inaccessible place, and this, in cases where the difference of stations usually in geometrical mensurations is not allowed. The way is evident by what is elsewhere delivered. For it having been found by *Mersennus's* trials, that sounds (as well small as great) do move in a second (as they call the sixtieth part of a minute) 230 fathoms, or thirteen hundred and eighty feet; if I see my correspondent fire a gun on the other side of the river, or if I see muskets or other guns casually fired on some tower or bastion, though never so far distant, and never so inaccessible to me, it is easy for me, by letting fall a short pendulum as soon as I see the flash of light produced by the kindled powder, and by reckoning the vibrations (made by that short pendulum, which distinguishes seconds into halves or quarters) that shall happen to be made before the noise arrive at my ear, to know how far off the place where the gun was discharged is from that I am in. As if a correspondent, standing over against me on the other side of a river, or some soldiers being there exercising, I see the flash or smook of a musket, or other gun, two seconds sooner than I can hear the report of it, I may conclude the river to be 2760 feet broad; and if a piece of ordnance being fired upon the tower of a besieged place, the noise arrive at my ear in half a second, I may collect 690 feet to be the distance betwixt that gun and my station. And by this means may that problem be performed, that we elsewhere mention as a thing, which, when nakedly proposed, may seem impossible. For if I see a ship at sea be shooting, whether in earnest, or for salutation, or for joy, it is very possible for me to measure, without geometrical instruments, how far it is off, though the ship itself be under sail. For vessels that fire guns, usually firing more than one, whether to offend their enemies, or to salute their friends, it is easy to take warning, by the first gun, to be in readiness with a short pendulum against another to be fired, and in this way of measuring (though not in any other yet known) one may take distances in the darkest night. For it matters not, whether I see the ship or place, whose remoteness from me I would know, provided, by some candle or taper I see the pendulum before the flash of the fired gun, which will sufficiently discover itself by its own light. And (to add, that upon the by) I have had sometimes thoughts, that if the velocity of echoes, which are but reflected sounds, be so well determined as that of direct sounds, navigators might sometimes make useful estimates in dark nights, whether they be near coasts, or considerably great rocks. For though upon discharging a gun, they cannot conclude how near the shore they are, because there may be parts of it less remote than those that send the echo; yet if they follow very quick upon the discharge of the gun, they have reason to suspect, that the shore, whose approach the seamen do so justly fear in the night, is at least as near as the vibrations of the pendulum inform them that the echoing place is.

## E S S A Y X.

O F

## M E N ' s G R E A T I G N O R A N C E

Of the U S E S of

## N A T U R A L T H I N G S :

O R,

That there is scarce any one Thing in Nature, whereof the Uses to human Life are yet thoroughly understood.

**T**HIS being an entire proposition, and clear enough of itself, will not need to be explicated, but evinced.

AND evinced somewhat solemnly it will require to be, not only because it is a paradox, but such an one as will meet with a peculiar indisposition to be entertained; since men cannot allow this paradox to be a truth, without such a confession of their ignorance, as must implicitly accuse them of laziness too. But however, I think, we may justly enough apply, with a little variation, to our present purpose, that true saying of *Seneca*, *Multi ad sapientiam pervenissent, nisi, &c.* and affirm, that many had attained to a greater knowledge and command of nature, if they had not presumed, that what is arrived at already, is much greater, and more considerable than indeed it is; especially in comparison of what is still behind, and yet attainable; and therefore, I think it not fit to suppress the considerations I was about to mention, since the displaying them may perhaps do you and others service, if they rouse up your curiosity, by shewing how much it has been defective, and if (which they ought to do) they encourage it also, by shewing you how much of nature undiscovered there yet remains, to recompence, as well as exercise your industry.

BUT because that of the particulars whereby our paradox may be confirmed, there are divers, that properly belong to the next ensuing essay, the proofs that we shall mention in this discourse, though I hope they will appear sufficient alone, will yet be strength-



strengthened, both as to number and weight, if you please to add to them those instances to be mentioned in the next discourse, that may be conveniently referred to this. In which I shall therefore insist but upon five general considerations; in all which I hope you will not forget, that I have already taken it for a supposition, which I doubt not of your granting me, that the usefulness of the works of nature to us depends chiefly upon the knowledge we have of their properties and other attributes; and consequently, that the more we know of these, the greater use we are like to be able to make of those physical things (and on the contrary); and therefore that ought to be looked on as an use of a physical thing, even though not immediately practical, that helps us to make discoveries of things that properly may prove so.

## SECTION I.

AND I consider in the first place, "That there are very few of the works of nature" that have been sufficiently considered, and are thoroughly known," even as to those qualities and other attributes of this and that body (or other physical thing) which belong properly to it, and are not thought to be so relative to other bodies. It is not only in the terrestrial globe, but in almost every body to be met with in it, that there may be a kind of *terra incognita*, or undetected part, whose discovery is reserved for our further industry.

THIS will appear the less improbable, if we consider these two things; whereof the one is, that there are divers ways of investing the attributes of bodies, as chymical, optical, statical, &c. which being artificial, and requiring skill, and industry, and instruments, there are very few men that have had the curiosity and ability to examine them after these several ways; without which, nevertheless, divers other attributes, some of which probably are capable of useful application, are not like to be discovered. To the proof of which, if it were needful, a multitude of passages in these present essays, as well as in our other writings, might be easily referred.

I SHALL therefore rather insist a little on the second of the two particulars lately mentioned. For it will easily appear not unlikely, that there should be many things undiscovered in the other works of nature, when there are so even in those obvious and familiar objects, that men are frequently conversant with, and have occasion to take notice of; nay, even in those noblest of mere corporeal things, our own bodies, whose structure does so much merit our curiosity, and of which it so highly concerns no less than our healths and lives, that we have an accurate knowledge. How many new discoveries have been made in the present age, beyond what the industry of the physicians and philosophers for above two thousand years has been able to take notice of? Witness the circulation of the blood, the Asellian, Pecquetian, and Bartholinian vessels; to which may be added, the Ductus Pancreaticus, and to which I doubt not will be added divers other discoveries, to recompense the industry of the anatomists of this inquisitive age.

IN so familiar bodies as eggs and chickens are, which so many thousand persons do daily see and handle, and perhaps eat, though many ages since, even *Aristotle* was solicitous about the history of them, concerning which he has delivered divers not inconsiderable particulars; yet there has been little within these few years so much undiscovered, that whilst men were hotly disputing whether the chick was first formed of the yolk or the white, our excellent *Harvey* made it evident (which our own observations have confirmed to us) that it is made of neither, nor yet of the treadle (as some modern observers have taught) but of the cicatrix, or speck that appears on the coat of the yolk.

WHO

WHO would imagine, that in a body so familiar, and so often treated of by philosophers, as snow, mankind should, for so many ages, take no notice of a thing so obvious as the figure of it frequently is? and yet *Kepler* is, by a very learned writer, acknowledged to have been the first that acquainted the world with the sexangular figure (as it is wont to be called) of snow, in a discourse by him published on that subject. And though I find mention made of it in *Olaus Magnus*, and have observed it so often (but not constantly in the same shape) especially about the beginning of the season of snow, that I cannot but admire men should not have very early heeded so obvious a phenomenon; yet I find not the discovery of it had been made so much as an age ago.

As many ages as vinegar has been one of the commonest liquors in the world, yet, that it oftentimes abounds with shoals of living creatures, that move, and in the microscope look like little eels, was looked upon but few years since as so new a discovery, that when, as I formerly noted, I first proposed it here in *England* to divers very learned men and virtuosi, as a thing to be seen even without the help of a magnifying glass, they took it to be a deception of my eyes, till their own assured them of the contrary.

THAT the milky way, though consisting of innumerable stars, should for two thousand years pass for a meteor, the inconspicuousness of those stars keeps me from much admiring. And, for the same reason, I wonder not that the men that lived before *Galileo* reckoned no more than seven planets, or suspected not that *Venus* herself is sometimes horned, and has her full and wane as the moon. Though these instances may serve to confirm what I lately told you, that many of the attributes of bodies are not like to have been discovered by those that employed not artificial helps. But what may we not expect that mankind may overlook, when the sun himself, which is not only the most conspicuous body in the world, but that, by whose light we see all the others, may have vast and dark bodies (perhaps bigger than *Europe* or *Asia*) frequently enough generated and destroyed upon him, or about him; and men, without excepting astronomers, never took notice of it, till of late years the excellent *Galileo*, or the industrious Jesuit *Scheiner* informed the world of them. And though I grant, that they discovered them by the help of telescopes (instruments unknown to the ancients) yet if men had been as watchful as the robleness and conspicuousness of the object would make one expect, they might have discovered some spots at least without those helps. For I find by an Italian letter of *Galileo's*, that some curious persons of his acquaintance, after his discoveries had awakened them, descried and discovered some of those solar spots with their naked eyes, unassisted by his tubes.

It may belong to this first section of our present essay to take notice, that one account, on which we may reasonably suppose men to be ignorant of the uses even of those things wherewith they think themselves well acquainted, may be, that the bare difference of climates and of places, may even in such bodies, as we familiarly converse with, beget such new relations betwixt them, as may endow them with qualities, and fit them for uses we dreamed not of.

I WILL not here mention the differing qualities, that bodies vulgarly referred to the same species of plants, animals, and other bodies, in almost all countries, are endowed with in some countries (as, that spiders are not venomous in *Ireland*, and Irish wood in general, if the received tradition be true, has an hostile faculty against venomous creatures) because the insisting on this subject would take up too much room in this place, and is reserved for another; and therefore I'll only add a couple of instances, the one to manifest what difference of climates may do, and the other to shew the unexpected influence of difference of places, though perhaps in the same climate.



THE first of these examples is afforded us by water and ice; for those that live in those warmer regions where it never freezes, and who have divers of them derided the relations of what happens in gelid climates as ridiculous, in probability would never dream that it could be a familiar use of a liquor they were so well acquainted with as water, to be broken or beaten in mortars like a dry body, and carried in carts or wheelbarrows from place to place, and kept all the year in that form, to make other water intensely cold in the greatest heats of summer. And even amongst us, those that have not been very inquisitive, can scarce imagine that one of the uses of water should be to serve for highways, whereon armies may march for divers days together with all their carriages and artillery, and whereon they encamp and fight battles with as much assurance as on the firm land; and yet those that have been in *Russia*, and the neighbouring northern countries assure us, that during the winter, when the rivers are frozen over, they usually take great journeys on them, and oftentimes rather than in summer, and choose that rigorous season, which allows them to march every where without sinking into the ground, to prosecute their wars in.

THE second of the forementioned instances we are supplied with by the declination of the magnetick needle from the true north and south points, and the variation of that declination. For though the loadstone were highly admired as well by philosophers and mathematicians as the vulgar; and though, since the great and happy use of it to navigation has been generally known, men have been upon several accounts invited to consider it with a peculiar attention and regard, yet that in some places the magnetick needle does not point directly, perhaps not by a great many degrees, at the pole, as in others it does, is no ancient observation, since it is ascribed to *Sebastian Cabot*; and it appears by the writings of our famous countryman *Gilbert* himself\*, that it must be somebody that lived since he wrote that must have the honour of being allowed the first observer of that strange and unexpected phenomenon, that oftentimes in the self same place, the declination of the needle towards the east or west does in process of time considerably alter. Which discovery I could confirm, by comparing some observations I have had opportunity to make, with those recorded by some modern authors.

AND as the same kind of bodies may have differing qualities, and consequently uses in differing places; so they may have, if examined or employed at differing times, comprizing under that name, together with the four seasons of the year, those peculiar seasons or periods of time, to which some signal change of qualities or state in particular bodies do belong.

THE mutations, upon the account of time, which I am here speaking of, are not those that are obvious to every eye, such as the differing qualities of fruit green and ripe, or the degeneration of wine into vinegar; but such as are not vulgarly taken notice of, and require either skill or curiosity, or both, in the observer; and of these a few instances will suffice for a taste.

WHEN common urine either is freshly made, or has not long been kept, the volatile and pungent salt is so clogged with other particles wherewith it is associated, that usually to obtain it one must evaporate or distil away near eight or nine parts of ten of the liquor, and then at length give a not inconsiderable heat to force up the last; but though the tradesmen that deal in urine do commonly overlook the difference, yet if the crude liquor be kept six or seven weeks, though not near the fire, the saline and noble parts will have to extricated themselves, that a very gentle heat will make them ascend, and leave behind them that phlegm that formerly would have preceded them.

\* *Gilbert de magnete*, lib. I. c. 1.

See the same *Gilbert*, lib. IV. c. 3.

THAT the Thames water, which our navigators are wont to take with them in long voyages, after a while, if they sail into hot climates, stinks very often too offensively to be potable, that, which happens usually to water, which is vulgarly observed to putrefy by long standing, will easily persuade us; and yet it is found, that this water, by being kept long enough in the same vessels, though it be in the same, or even in an hotter climate, will at length lose its stink, and grow potable again; as I have, upon enquiry purposely made, been assured, not only by the vulgar tradition, but by two very inquisitive persons upon their own knowledge; the one having particularly observed it, sailing betwixt *Europe* and *Africa*, and the other in a voyage to and from *America*. And I the rather mention this, because I am very credibly informed that there are divers other waters that have this faculty of recovering after putrefaction, which is supposed to be peculiar to the water of the Thames.

AND, if I much mistake not, one or both of these very persons named another river to me, with an affirmation of its having the same power of self-recovery. And having held some curiosity to try experiments, how pump-water, or the like rough water, as they call them, that would not bear soap, may be helped; an industrious person I employed assured me, that he met with pump-waters, which after having stood a few days, without having any thing done to them, would bear soap, which before they would not do.

CORIANDER seeds being freshly gathered have been observed to have so much acrimony, that divers of the ancient physicians reckon them among venomous plants; and in dispensaries they are usually prescribed to be prepared with vinegar, or some other corrective; whereas the more accurate observers take notice, that within a competent time after the seed is gathered, it loses of itself that excessive acrimony that at first blemished it. And the like I find observed, by good apothecaries, of the roots of aron, which are mitigated by keeping (and which some noted physicians of my acquaintance do little less magnify to me than does *Quercetan* himself).

[THAT vegetables, what known way soever they are wont to be laid up and ordered, do not afford, unless first reduced to foot, any dry volatile salt, like that of animal substances, I elsewhere more particularly declare, and those that have had the curiosity to try it, will confirm; but yet by some discourse I lately had with a very ingenious person, and some subsequent trials made after a way I devised to examine distilled liquors, I was satisfied that there are divers vegetables, and those very commonly growing here in *England*, which being gathered and laid together at a certain season, and distilled also at a certain nick of time, will yield, instead of the vinegar-like, and other liquors, wont to be afforded by such plants distilled the common way, a volatile spirit; which in smell, taste, and divers operations, as turning syrup of violets green, hissing with acid spirits, &c. resembles the volatile spirits and salts of animal substances; and which I doubt not but you will wonder at, this great change, whose secret I wish I durst teach you, is effected without the help of any additament.]

AND, that you may not think that it is only in vegetable and animal substances, that are commonly of a more loose or alterable texture, that the trying things at one time rather than another may be very considerable, I will add a couple of instances even in mineral bodies.

It is a chymical complaint, even of the curious and experienced, that though authors teach us to make the salt of violently distilled or calcined vitriol, by forthwith taking the caput mortuum (from which all the oil has been by the violence of fire forced out) and extracting the saline part by effusions of water; yet those that make exact trials of it find, that when the dark red mass of powder is newly taken out of the vessels, it is



so totally robbed of its saline particles, that no affusion of water will at all obtain from it the expected salt. Notwithstanding which, having purposely enquired of some that distil great quantities of oil of vitriol, whether or no, when they had made an end of one distillation, if they lay by the *caput mortuum* for a pretty while in the air, they could not find it impregnated enough with new saline particles, to be fit to yield more menstruum, and be worth another distillation? I was answered in the affirmative, provided, that (as I mentioned in the state of the case) there were a competent time interposed between the former and the latter distillations. (The reason of which, according to my trials and conjectures, may be assigned of this odd phenomenon, belongs not to this place, but you will hereafter meet with it in another).

THE second instance I promised you, is afforded me by stones; for there are, and not far from this place, quarries of solid and useful stone, which is employed about some stately buildings I have seen, and which yet is of such a nature, wherein divers other sorts of stone are said to resemble it, that though, being digged at a certain season of the year, it proves good and durable, as in those structures newly mentioned; yet employed at a wrong time it makes but ruinous buildings, as even the chief of those persons whose profession makes him more conversant with it, has himself acknowledged to me to have been found by sad experience. But concerning this observation, you may expect to meet elsewhere with a farther account.

AND though time and place be two of the principal, yet they are not the only circumstances whose variations may make some such attributes discovered in natural things, as are not usually heeded; of which I shall mention but a couple of instances, because they may serve to shew you that such circumstances as are thought the slightest, may afford new uses even of solid and lasting bodies. Skillful artificers that grind optical glasses for tubes, have complained to me, that oftentimes the convex glasses they fashion will prove veiny, and consequently, after all their labour, of little value; and yet they are not able to discover these unwelcome veins in the glass, by the most careful viewing it against the light, till they have spent a pretty deal of time about working of it; and even then they are unable to descry these blemishes, if they hold the glass at an ordinary distance from the eye; but they are obliged to remove it a great way (perhaps six or seven feet) farther, so much may an increase of distance become serviceable even where one would expect the quite contrary.

BUT probably you will look upon posture as a slighter circumstance than distance itself, and yet Dr. Gilbert has observed, and I have found it true by many trials, that long irons, as the bars of windows, that have stood upright for a great while, do, by that perpendicular posture, acquire a verticity or magnetick virtue, as having acquired magnetick poles. So that if you apply the needle of a dial, which I mention as the readiest way of trial, to the lower part of the bar, it will draw the south end of the needle; whereas the upper extreme of the bar will seem to drive away that end, and will draw the northern.

BUT here I must not forget to take notice, that I can scarce think men will be able to know all the properties and uses, even of familiar bodies and other things, till they have mathematically considered them; there being several attributes belonging even to such things, which a naturalist, though curious, will probably never find out, unless he be both acquainted with mathematical disciplines, and have the curiosity to apply them to physical subjects. And though in other essays of this book, divers things are delivered that do directly enough tend to manifest what I have now said; yet it is of such importance, that naturalists should be thoroughly persuaded of a truth, that may be so much more useful than it is yet generally admitted, that I am content to inculcate it, by setting

down here a few instances of somewhat a differing sort from those elsewhere delivered, and more appropriated to the present subject of our discourse.

You will not doubt, but that ever since the first ages of the world, the majority of men have had some occasion or other to see bodies swing; and yet, till *Galileo* (for he is generally believed the discoverer) took notice of the vibrations with a mathematical eye, men knew not this property of swinging bodies, that the greater and smaller arches were, as to sense, equitemporaneous; from which discovery have been derived several practices of good use, some of which have been already mentioned in these essays.

THAT water, running out at a hole made in the sides near the bottom of the vessel, makes a parabolical line, or one that near resembles it, and that in such effluxions of water, there is a determinate proportion assignable betwixt the perpendicular height of the liquor, and the diameter of the hole, whereby the velocity and quantity of water that would run out may be computed, has not been, that I know of, taken notice of, till the observations of the above-named *Galileo*, and the diligent *Merjennus* (to which we may elsewhere add some of our own) have endeavoured to define those matters.

Meteo-  
rum, cap.  
VIII.

As constantly as we have occasion to take notice of the air, and water, and glass, yet the curiosity of our modern masters of opticks has observed many things touching the refraction of the beams of light made in those mediums in different quantities, and to and from the perpendicular, not to say any thing of the equality of the angles of incidence, and of reflection made on the surface of still water, unheeded by those that are not versed in opticks: the drops of dew that hang in numberless multitudes upon the grass and leaves, are things that every eye has been invited to take notice of by the orient colours the sun is wont to make them afford us; but till the excellent *Des Cartes*, contemplating them with a more critical eye, found, that in such a determinate angle made at the spectator's eye, between the ray of light coming from a certain part of the drop, and the imaginary straight line reaching from the eye to the sun's center, the drop appeared red, and in another determinate angle exhibited yellow, blue, and other colours, and at other angles, shewed no colour at all; the world ignored a considerable property of spherical diaphanums irradiated by the sun, and seems not to have dreamt of a neat hypothesis, with which some ingenious mens minds are no less taken, than their eyes are with those vivid colours of the rainbow, which it pretends to give a clear account of. And though we daily see pieces of wood and timber broken by the weight of over heavy bodies, yet till the often named, and still to be commended *Galileo* applied geometry, and the doctrine of proportions to matters of this kind, the resistance of solid bodies to be broken by weight (whether their own or that of other bodies) seems not to have been so much as suspected to be reducible to such an estimate, as he and others have brought it to. And a virtuoso of my acquaintance (for whom *Merjennus* laid the way) in a musical instrument that I have with pleasure heard him play on, can observe a property of metals that chymists thought not of, namely, that equal wire-strings made of differing metals, and having a due tension, will yield sounds differing as to sharpness by determinate musical notes, or the divisions of them. And to these I might add divers other remarks of *Merjennus* and *Galileo* about the force of guns (which were found to increase with their length but till such a number of feet, beyond which the length did but lessen it) and the parabolical line, in which bullets (that are thought of all other bodies to move the straightest) are said to move; and I know not how many other mathematical attributes, if I may so call them, of natural things, that geometricians, astronomers, engineers, &c. have already observed, might be here added, but that I think it sufficient to subjoin one instance more that may well serve to keep us from imagining, that even the most familiar objects in the world, and that seem likely to afford the least discoveries, have been sufficiently considered.



dered. For how few phenomena in nature are there that occur to us more frequently than the falling of heavy bodies? And yet, though the ancients and *Aristotle* himself took notice that there was an acceleration of descent in falling bodies, we find not that any so much as fairly attempted to determine that acquired velocity, till *Galileo's* observations reduced it to the proportion mentioned in some of the former essays, wherein most of the following mathematicians (for I have scarce met with two dissenters) have acquiesced; and whereby in the eighth essay we endeavoured to measure heights and depths without geometrical instruments. In a word, till geometry, mechanicks, opticks, and the like disciplines be more generally and skilfully applied to physical things, I cannot think otherwise, than that many of the attributes and applications of them will remain unknown; there being doubtless many properties and uses of natural things that are not like to be observed by those men, though otherwise never so learned, that are strangers to the mathematics.

AND AS I have hitherto observed of bodies, so I shall venture to add of qualities and divers other natural things, that even those that are very familiar may have attributes and uses, which the generality of men, without excepting those that are otherwise learned, are not wont to take any notice of.

THAT black bodies, for instance, as such, are much more strongly and easily warmed by the sun-beams than white ones, nay, though the disparity be not so great, than bodies of any light colour, *ceteris paribus*, is perhaps more than even you have taken notice of; and yet I shall hereafter have occasion to prove it by divers instances, and you may easily try it, either by exposing for some time to the summer-sun a white glove and a black, or a couple of eggs, whereof one is inked, or otherwise blacked all over.

COLD is one of the most familiar qualities men have to deal with; and though they otherwise are not wont to expect much from it, yet least of all would they expect that it should, contrary to the received definition of it, which is, *congregare tam heterogenea quam homogenea*, that it should, I say, perform the office of heat in spirit of wine, nay, and in presenting us ardent spirits from beer and other liquors inferior to wine; and yet, not to mention *Paracelsus's* process of making the essence of wine by freezing all the phlegm, we have the repeated experiments of navigators into the frigid zone, who assure us, that not only from wine, but from beer, by the congelation of the aqueous parts, there may be separated or obtained a liquor, strong, hot, and spirituous, almost like aqua vitæ.

AND even in our temperate climate some odd separations may be made by cold; for, not to anticipate those trials of mine that belong to other papers, there may, by such cold as we have here, be made a separation in oil, of a liquor much finer and more spirituous than the rest; for I know an eminent artificer who kept it as a choice secret to resort (as himself confessed to me he did) in hard frosts to the great jars of oil, where he often found greater or lesser cracks or chinks in the congealed part of the oil, in which crannies was contained an unfrozen liquor that appeared thinner and finer than common oil, and was much better than it to preserve things from rusting, as perhaps having left many of its saline parts in the concentered oil; and for that purpose was much prized, not only by him, but by some watchmakers that were made acquainted with the virtue of it.

BUT it were tedious to insist on all the instances that may be brought of the applications that may be made of colour, sound, levity, springiness, fermentation, and even putrefaction; and it would be not only tedious, but almost endless to prosecute those instances that might be afforded by other more general and operative states and faculties of bodies. For not only motion and rest, fluidity and firmness, gravity, and the like, have

have a more universal influence of natural things, than even philosophers are wont to take notice of; but those less catholic affections of matter, that are reckoned among but particular qualities, such as gravity and heat, may have so diffused an influence, and be applicable to so many differing purposes, that I doubt, whether all the uses of that particular degree or pitch of heat that reigns in fire, will have all its uses discovered, before the last great fire shall dissolve the frame of nature.

NOR must I pretermitt one consideration more, that belongs to my present subject, which is, that probably many more qualities or other attributes would be taken notice of, even in those natural things, that are reckoned among the most known, if men did not want a measure of curiosity that might justly be expected. For I speak not here of curiosity in general (which I doubt not would make far more numerous discoveries than were necessary to justify my present discourse) but I only speak of such a curiosity about the things of nature we familiarly converse with, as we could scarce want, if it were not out of laziness, or a prejudicate opinion, that makes us take that for granted, that we should find to be quite otherwise, if we did not choose rather to presume than to try.

THUS, that falling bodies, the heavier they are, the faster in proportion they fall, has been a received opinion in the schools since *Aristotle's* time, and has kept the equivocality, as to sense at least, of bodies of very differing bulks and weights falling from moderate heights, such as surpass not ordinary towers and steeples, from being taken notice of, till of late inquisitive men by experiments found it out.

THAT water by glaciation is reduced into a lesser room, has been and is still the opinion, not only of the vulgar, but of the generality of learned men; and yet, that water is not condensed, but expanded by freezing, he that will congeal that liquor in vessels strong enough, may easily find by trial. And the floating of ice upon water, and the bubbles that are usually to be observed in it, may alone suffice to make a considering man distrust the vulgar opinion.

THAT the common air we breathe and live in, is a body endowed with positive levity, has been for many ages, and continues to be almost universally believed; and yet if men had the curiosity to examine this supposition by one or other of those several ways, by which the gravity or levity of the air may be discovered, they would quickly find that is not devoid of weight. And even so slight a way as the condensing the air in a blown bladder, by tying a string something strong about the middle of it, may bear witness to what we say. For though we should oppose, as some have lately done, that in such cases the air is not in its natural state, but condensed; besides, that is an objection to which all the expedients of weighing air are no way liable, it makes rather against the objectors, than the conclusion, against which they urge it; since, if the particles of the air be really light, the filling the bladder the fuller of them ought to make it rather lighter than heavier.

THAT greater and lesser sounds do, as to sense, move with an equal swiftness, is that whose contrary is taken for granted; and the more excusably, because it is evident and confessed that great and small sounds do not move equally far; and yet, that this equivelocity of sounds has been made out by the late observations of the diligent *Mersennus*, and others, you may remember to have been delivered in a foregoing essay, where I also endeavoured to shew, that this property of sounds is not unapplicable to human uses.

THAT the loadstone, which by immediate contact will take up iron, should have so strange a property, as to take up far more when a cap or conveniently shaped piece of steel is interposed betwixt it and the body to be raised, is a thing so unlikely, that though the ancients knew and much admired the attractive virtue of the loadstone, yet they seemed



seemed not to have suspected it enough to vouchsafe it a trial : and yet since *Gilbert's* writings came abroad, he must be a great novice in magnetical affairs, that either ignores or doubts it. But I must not do any more than touch upon magnetical experiments, since they alone would afford me so many truths (which the generality of men would not have thought likely enough to be worth trying) that to enumerate them, though it might convince your understanding, would, I fear, exercise your patience.

THAT it is the property of unslaked lime to grow hot by antiperistasis, upon the pouring on of cold water, and other cold liquors, and consequently not to grow hot upon the effusion of liquors that are not cold, is not only generally believed, both by learned and unlearned ; but this property of lime has been employed as an argument to prove other matters, as well by divers of the new philosophers, as by many of them, that embrace the old Aristotelian principles : whereas I doubt not but a little trial might easily disabuse them : for by the affusion of divers liquors actually warm, I have made lime slake with its wonted violence, if not with a greater. And in other liquors actually cold, like unheated water, and one or two of them far more thin or subtle than it, I have kept lime long without slaking, and without imparting to the ambient liquor any sensible heat. The quality of these instances makes me think it needless to increase their number, since we can scarce wish a greater inducement to expect, that many new attributes may be discovered in the works of nature, if men's curiosity were duly set on work to make trials, than that divers have been found out, that seemed so unlikely, that men thought it would be in vain to try them.

To these several sorts of instances, that have hitherto been reduced to our first consideration, might well be added, that bodies, which have the same denomination, and from whence men are therefore wont to expect the same, and but the same, operations and uses, may yet have peculiar ones, and some of them very differing from those of the generality of other bodies, that bear the same name. But examples of this kind will more conveniently be mentioned in the last essay : and lest this should swell too much, dismissing this present consideration, we will advance to the next.

## S E C T I O N II.

I CONSIDER in the second place, that the faculties and qualities of things being (for the most part) but certain relations, either to another, as between a lock and a key ; or to men, as the qualities of external things referred to our bodies, and especially to the organs of sense, when other things, whereto these may be related, are better known, many of these, with which we are now more acquainted, may appear to have useful qualities not yet taken notice of.

I SHALL elsewhere, *Pyrophilus*, have occasion to shew you more fully on what grounds, as well as in what sense it is, that I take the most of the qualities of natural bodies to be but relative things. To our present purpose it may suffice to adumbrate my meaning by the newly hinted example of a lock and a key, where, as that, which we consider in a key, as the power or faculty of opening or shutting, supposes, and depends upon the lock, whereto it corresponds ; so most of those powers and other attributes, that we call qualities in bodies, depends so much upon the structure or constitutions of other bodies, that are disposed or indisposed to be acted on by them, that if there were no such objects in the world, those qualities in the bodies, that are said to be endowed with them, would be but aptitudes to work such effects, in case convenient objects were not wanting. As if there were no lock in the world, a key would be  
but :

but a piece of iron of such a determinate size and shape. And this comparison I the rather imploy, because it may be further applied to our present discourse. For as if some barbarous American should, among other pieces of shipwreck thrown by the sea upon the shore, light upon a key of a cabinet, he would probably look on it as a piece of iron, fit only for the inconsiderable uses of any other piece of iron made much broader at each end than in the middle; but, having never seen a lock, would never dream, that this piece of iron had a faculty to secure, or give access to, all that is contained in some well furnished chest or rich cabinet: so there is many a thing, that seems to us useless, whilst we look upon it only in itself, which will perhaps hereafter prove highly useful, when we shall light upon some other bodies peculiarly fitted to act upon it, or receive impressions from it. But this will be better apprehended by the following instances.

THOUGH iron be so common a body as it is, and its uses are very many, and have been known as long as since *Adam's* time, yet all those differing bodies, on which men of all sorts imployed it to work, and all those various ways, whereby chymists, physicians, and mineralists have wrought on it, during some thousands of years, did never discover to man one of its noblest and usefulest properties, which, for aught we know, was never found out till within these three or four ages: for a steel needle, being applied to a loadstone, manifested itself to be capable of constantly shewing the north and south in all seas, in all weathers, and in all times of the day and night to navigators, who, by this property, which depends upon the relation that iron has to one only stone, have been enabled to discover the new world, and enrich the old with the drugs and treasures of it.

AFTER all the vain attempts that even subtle chymists have made to arrest the fluidity of quicksilver, the knowingest persons that have meddled with that mineral, and especially if they have observed, that the keenest frosts, that are capable of freezing even *aqua vitæ*, are unable to congeal it, have been very much indisposed to reckon an easy coagulableness amongst its qualities; and yet we see, that though the mixture of no other known body will disclose its having any such affection, yet the vapour of melted lead will sometimes (for that experiment will not always succeed) reduce quicksilver, even in its mass, into a consistent and somewhat tough and hard body.

VINEGAR being a liquor that has been generally known and used for some thousands of years, men have imployed it upon great variety of bodies, and to very many uses, but especially to communicate a sourness to the things wherewith it was mingled; but when it came (probably by chance) to be applied to the dissolving of lead calcined or crude, it manifested, that it had a faculty to exhibit a more than saccharine sweetness, which, for aught I know, it exhibits with that metal only; for I have not yet known crude vinegar dissolve tin, though calcined: and though by a slight artifice, elsewhere mentioned, we have been able to make strong vinegar dissolve the calx of Jupiter, yet was the solution far differing from, and inferior to, the taste of the solution of lead newly mentioned.

SPIRIT of urine is a liquor, that has been long known to chymists, and might reasonably be looked upon as likely to be a good menstruum for several bodies: but it is not probable, that after it had been imployed to dissolve divers compact bodies, it should be suspected, that it would coagulate so thin, light, and fugitive a body, as spirit of wine itself; and yet we have often (as there will be hereafter divers occasions to relate) tried, that if both liquors be sufficiently pure and dephlegmed, they will afford that strange snow-white concretion, that *Helmont* calls his *Offa alba*; which, however by his followers ascribed to him as the inventor, I find mentioned in ancienter books than his:

and



and I remember, that even *Raymond Lully* relates, with what wonder he first saw this experiment (which indeed is considerable) performed.

AND as the spirit of urine has such an odd property, when it meets with ardent spirits dephlegmed; so the spirituous parts of urine, without being separated from the rest, have a faculty, that one may yet less expect, if they be duly employed, to operate upon musk: as I have had the opportunity to inform myself by inquiry of a scholar, who lived in *China*, and affirmed himself to have divers times seen musk made. For this person answered me, that he had observed it to be the practice of others, and had made trial of it himself in those eastern parts, that the musk being made up, and put into cuds or bags made of the skin of the same animal, (in which form I have received presents of musk sent me from the *Indies*) they do, either before or after, hang it in a house of office, so as it may, without touching the grosser bodies, receive the fetid exhalations of that nasty place; by which urinous steams, which it is exposed to for some days, the less active, or more immersed scent is, as it were, called out, and excited or heightened. And I found, by farther enquiry of the same person, that having carried musk from those eastern regions, where it is made, to other and remote parts of the same *Indies*, he found, that, by the length of the voyage by sea, his musk had very much lost its strength, which he afterwards restored to it, by following the advice of some skilful persons, according to which he tied the musk close in a bladder, wherein, having pricked many little holes with a needle, he hung it up for some days in such stinking place as has been newly mentioned. Whereto agrees very well what I have read in a late eminent physician of *Rome*, (where the art of perfuming is very much cultivated) who communicates it as the chief secret practised by the perfumers there, for recovering the scent of decayed musk, that it be kept for a competent time in linen well moistened with rank urine.

THE uses of gesso (as the Spaniards and Italians call it,) or gypsum, are numerous enough in the shops of stone-cutters, moulders in plaister or wax, and divers other artificers; but one would scarce suspect, that, besides the various uses these tradesmen put it to, it should have one so very differing from them, as to be an excellent medicine, if I may so call it, for wine: and yet, that they use great store of it about those choice ones, that come to us from the Canaries, is a noted tradition among those that deal in that sort of liquor, and has been confirmed to me by an eminent wine-merchant, that lived several years in those islands. And, that about *Malaga* they put up a good proportion of it into the juice of their grapes, when they tun it up, is affirmed to me by a curious eye-witness, who was there in vintage-time, and of whom I purposely enquired about it.

THOUGH silver be so noble a metal, and so much known and used, that it was the price of things as early as *Abraham's* time, yet one very fine use of it has been known but since the art of annealing upon glass came to be practised. For among other experiments of this art we find, that prepared silver (and I have sometimes done it pretty well with the crude metal) being as it were burned upon a plate of glass, will tinge it with a fine yellow or golden colour: there are also divers mineral earths, and other coarse fossils, of use in this art, which, by the help of the fire, makes them impart colours to glass, both transparent, and sometimes very differing from those of the bodies themselves, as I may elsewhere have occasion to specify. In the mean time, give me leave to name this reflection upon the art of painting, that it is very hard for us to be sure, that we know so much, as all the several sorts of uses that may be made of the particular bodies we converse with, since upon the invention of a new art or trade, of which we know

not how many remain yet to be found out, divers uses and applications of bodies come to be disclosed, that were never suspected before.

THE use of lyes made with common ashes to wash linen has rendered them for these many ages very familiar : but though their effects on the other bodies, upon which they have been employed, seemed not to have any affinity with what I am going to mention ; yet when a strong lixivium is applied to syrup of violets, (which is also a very known liquor) to which it has a peculiar relation, it will then immediately change the colour of that syrup from a blue to a perfect green, and so it will the violet leaves crushed on a piece of white paper, without the help of sugar, or any preparation.

REDNESS, though a colour as obvious as most others, and to the generality of men very pleasing, however it hath no offensive property, in reference to other animals, familiarly known amongst us, (at least, that we have taken notice of :) yet being presented to the eyes of turkey-cocks, it has such an incongruity with them, that oftentimes it is observed to make them very angry, as far as can be judged by the tokens of being displeased it produces in them.

THE leaves of oaks, that are such common things, and are not observed to have, in reference to any other body, which chance or industry applies them to, any such property as that I am about to name ; these leaves, I say, if when fresh, they be immersed in the water of mineral springs, impregnated with the subtle corpuscles of iron, I have several times found to turn the liquor blue or black, according to the proportion and vigor of the two ingredients.

ONE would not expect, that so dark and black a body as charcoal should be the main thing employed, not only to cleanse and brighten some metals, but to procure a clearness, and give a gloss to some transparent bodies. And yet I learned from the makers of mathematical instruments, gravers, and other artificers, that the best way they have, and which I have seen them employ, to polish their plates of brass and copper, (after they have been rubbed clean with powdered pumice-stone) is with charcoal, (which some of the more curious burn a second time, and quench in appropriated liquors,) as that, which both serves to fetch out the scratches of the pumice-stone, and itself scours without scratching, and thereby polishes very smoothly. And by the same way they may cleanse and polish the plates of horn, of which they make lanthorns, drinking-cups, &c. To which, as to the metalline plates, a gloss may be afterwards given with tripoli.

PERHAPS it will not be improper to take notice to you, *Pyrophilus*, in this place, that not only the nature of the body to be wrought upon, but some peculiar circumstances relating to it, may contribute to the effects of such experiments, as those treated of in this section. As for example, one would not expect that water, which is so apt to run out at the chinks of wooden vessels, should, without addition, become the fittest instrument for closing them. And yet I have more than once found by trial, as I presume many tradesmen have done, that when wooden barrels or firkins, and the like vessels, by having been long kept too dry, come to have clefts and commissures, this inconvenience may be remedied by pouring water into them. For though at first the liquor quickly runs out again, yet by frequent affusions of it, the wood, especially those edges between which the water runs out, becomes so softened and plumped up, that the little intervals or chinks are, by the swelling of the neighbouring parts, closed up, and the vessel becomes stanch.

AND upon a like reason seems to depend that odd experiment, much talked of by some of our eminent English seamen, who, for the hasty stopping of a leak that is not too great, much commend the thrusting into it a piece of powered beef ; for this being  
much



much more salt than the sea-water, that liquor pierces into the compact and (in great part) dry body, and by opening the salts, and soaking into the flesh, makes the swelling beef expand itself, so as to bear strongly against the edges of the broken plan s, and thereby hinders the water from flowing into the ship as it did before.

## SECTION III.

I CONSIDER in the next place, that a body in association with others may be made fit for new uses, and some of them quite differing from those, that were proper to it before.

THIS third consideration is, in some regards, of affinity with the first, but yet is not the same, since in the former we consider the power, that one body has to act upon another, or the disposition it hath to be acted upon by it; whereas now we consider the two bodies or more, as being by conjunction qualified to act on a third body, or suffer from it, as one entire concrete, upon the account of new and emergent properties, accruing to the compound by the association of the more simple bodies, that compose it.

You will meet with store of instances, both in these essays, and other of my writings, easily applicable to the illustration of what is here delivered, and therefore it will suffice to name in this place the fewer.

HE that takes notice, how flexible a metal tin is, and how dead a noise it yields, will scarce dream, that one of its uses, and that none of the despicablest, should be, to make another metal, which is less yielding, and has a less dead sound than itself, not only hard, but sonorous: and yet we see, that bell-metal, which, when cast into bells, makes a hard mixture, that sounds so loudly, is made principally, as has been already noted, by the addition of a certain proportion of tin to copper.

In the common experiment of making ink, the infusion or decoction of galls is yellowish, or reddish, and the solution of vitriol will, as the concrete participates more of iron or of copper, be either green or blueish; but from the mixture of these two liquors there will emerge an inky blackness.

THAT oil, that is a body so mollifying and slippery, and whose unctuousity make its moisture so much more difficult to be wasted or destroyed, than that of water, wine, or other not tenacious liquors, should be one of the two or three main ingredients, and the only moist one of a hard and durable cement, is that, which probably you would very little expect from it: and yet, not to mention what trials of that nature I have made, because I had not time to observe the full event, a very ingenious man, much employed about costly water-works and dams, assures me, that the best way he has to join together, and, if need be, piece and mend with a close and lasting cement the pipes that are used for subterranean aqueducts, that are long to hold running water, is to take good clay (such as tobacco-pipes are made of,) and having dried it, and reduced it to a very fine powder, and mixed good store of short flocks with it, beat it up very diligently with as much linseed-oil, as will serve to bring it to a stiff paste, almost like well kneaded dough. This paste he fashions into pipes of the length and bigness required, which, though they will be long a drying in the air, yet, when once thoroughly dry, are very staunch and lasting. And I remember, that before I learned this, having occasion to try divers experiments about cements, I chanced to meet with an ancient artificer, employed to keep in repair the conduits that brought water to *London*, and in exchange of a lute or cement that I taught him, he was forward

ward to satisfy the curiosity I had to know what cement he employed about so important a work, and he assured me, that oil was one of the main ingredients (and the only liquid one) he employed.

He that considers, that lead is one of the most opacous and flexible bodies, that the world affords, will not easily imagine, that one of its uses should be to make up about three parts of four of a mixture transparent, and exceeding brittle; and yet this is easily performed by divers chymists (and I elsewhere mention my having often done it) in making of calcined lead, and powdered flints or sand, a brittle and diaphanous composition, called by Spagyrist's Vitrum Saturni.

AND this mention of glass suggests to me another instance, fit for my present purpose: for who would imagine, that such a body as the fixed salt of kaly, which, as other alkalies, that take their denomination from it, has a strong and fiery taste, and is not only readily dissoluble in water, wine, or any such liquor, but will in a short time, being but left in the air, be reduced into a liquor; who would expect, I say, that it should be of any use, much less the main of this caustick, and easily dissoluble body, to be one of the two main ingredients of a substance both perfectly insipid, and indissoluble, not only in water, wine, &c. but even in aqua fortis, aqua regia, spirit of wine, quicksilver, spirit of urine, and other menstrooms, some of them highly corrosive, and others extremely subtle and piercing; and yet such a mixture is usually afforded us in glass (especially the more durable sort of it) wherein that there is actually a great proportion of alkalizate salt, I confess, I doubted, till having purposely enquired of an ingenious master of a glass-house, how much glass he usually obtained, when he put in such a quantity of sand, I found by his answer, that the glass obtained was many pounds in the hundred more than the sand that was employed to make it: whence I gathered, (what he also affirmed) that the alkaly did not only seem (as one might suspect) to promote the fusion of the sand, but does materially and plentifully concur with it to compose the glass.

AND whereas I intimated at the very beginning of this third section of this essay, that bodies when associated, may be applied, not only to new uses, but perhaps to some that are quite differing from those, that belong to some of the respective ingredients; this observation may be made good by several instances, and even by some that are very obvious, as well as by others that are not so familiar. For we may take notice, that though oil, and tallow, and other such unctuous bodies, be those, that do grease and spot linen and woollen clothes; yet those very bodies, being skilfully associated with others, though with but a lixiviate salt and fair water, do plentifully concur to the making up of soap, by the solution of which grease is readily washed out of linen cloths, and others, besides those, are also freed from the spots of it. But divers others instances applicable to this purpose belonging more properly to the following part of this essay, till we come thither, it may suffice, that I illustrate and confirm what hath been proposed by the single, but noble instance of Aurum fulminans. For though salt of tartar be a fixed body, and of a fixing quality, yet being skilfully associated with gold, dissolved in aqua regia, though that be thought the fixedest, not only of metals, but of bodies; yet the gold precipitated by this fixed and incombustible salt becomes so exceeding fugitive, that by a gentler heat than would kindle any known body in the world, it is made to fulminate like gun-powder, (but many degrees more violent than it;) and, which you will also think strange, though sulphur be a body of so quick accension as is obviously known, yet by an easy way, elsewhere to be taught you, of mixing those two only, you may, as trial hath informed us, make it (which you will easily allow to be one of the unlikeliest uses of sulphur) even by its being set on fire, to hinder



hinder the accension of this so easily kindled gold, which I have known thereby readily turned into a medicine, that some cry up for excellently diaphoretick, (though I doubt whether Aurum fulminans work not rather another way,) and which I remember I have, in a crucible, kept long in the fire without loss.

I SHALL only add to this third consideration this one particular, that is of too great moment to be pretermitted here, though it have been already in part taken notice of on another occasion; namely, that the effects and uses of mixtures do not only depend upon the nature of the ingredients, but may be oftentimes much varied by their proportion. And of this the mineral, which at the glass-houses they are well acquainted with, under the name of manganéz, will afford us a pertinent and considerable instance. For though it be a coarse and dark mineral itself, and though being added to the materials of glass in a fuller proportion, it make the black glasses, that are sold in shops; yet not only a moderate proportion of it is used to make glass red, but, which is more remarkable, a small and due proportion of it is commonly employed to make glass the more clear and diaphanous.

#### S E C T I O N IV.

IN the fourth place I consider, that a body, by a differing preparation of management, may be fit for new, and perhaps unthought of purposes. For the qualities of bodies depending for the most part upon the texture of the small parts they are made up of, those ways of ordering greater bodies, which do either by addition, detraction, or transposition of their component corpuscles, or by any two, or all of those ways, make any notable change of the former texture of the body, may introduce new qualities, and thereby make it fit for divers uses, for which it was not proper before.

WE see to how many several uses men, that were neither philosophers nor chymists, but for the most part illiterate tradesmen, have been able to put iron, by but varying the visible shape of certain portions of it, and connecting some of them after a peculiar manner; as is obvious in the shops of blacksmiths, locksmiths, gunsmiths, cutlers, clockmakers, ironmongers, and others. But to give you a more physical instance in the same metal, be pleased to take notice, how much a change, made by a natural agent, the fire, in the invisible texture of iron, does speedily alter it; when of the same bar of iron, by the help of fire and water, the artificer makes hardened iron, and iron of a temper fit for drills, and knives, and springs, and I know not how many other instruments, which require distinct tempers in the metal they are made of; that temper, which renders them fit for one use, leaving them unfit for another.

BUT we need not confine ourselves to instances, wherein no new ingredient is added to, or taken from the body to be altered; it being sufficient, that the additament upon its own account do not bear so great a stroke in the change produced, but that it be principally ascribed to the way of ordering the body wrought upon; and speaking of the management of a body in this sense, (which is usual and proper enough,) I shall subjoin a few instances, of the many I might add, to make good our proposition.

THOUGH paper be one of the commonest bodies that we use, yet there are very few, that imagine it is fit to be employed otherways than about writing, or printing, or wrapping up of other things, or about some such obvious piece of service, without dreaming, that frames for pictures, and divers fine pieces of embossed work, with other curious moveables, may, as trial has informed us, be made of it, after this or the like manner. First, soak a convenient quantity of whitish paper, that is not fine, about two

or three days in water, till it be very soft; then mash it in hot water, and beat or work it in large mortars or troughs, (much after the manner used in some places to churn butter) till it be brought to a kind of thin pap, which must be laid on a sieve, without pressure, to drain away the superfluous moistness, and afterwards put into warm water, wherein a good quantity of fish, glew, or common size, has been dissolved. Being thence taken out by parcels with a sponge, it must therewith (for the sponge will dry up the superfluous moisture) be pressed into moulds of iron, or of such plaister as statuarys use, wherein having acquired the figure, which is intended to be given it, it is thence to be taken out, and permitted to dry, and is to be strengthened, where need requires, with plaister, or grated chalk, made into pap with water, or some other convenient matter; and afterwards, having first been leisurely dried, it is to be either painted or overlaid with foliated silver or gold, as the artist pleases. I may elsewhere have occasion to mention another unlikely use of paper, namely, to stop the clefts and commifures of wooden instruments and vessels that are to hold water. For paper being thrust into these narrow places, the first water, that comes to it, being soaked up, occasions a forcible dilatation, which makes the swelling paper fill the chinks it is lodged in, according to what was lately delivered at the close of the second section.

THE sugar-cane has been a plant well enough known to many countries and ages, who were not unacquainted with the sweetness of its juice, and yet seem never to have made sugar of it, for want of knowing the way of so ordering it, as to coagulate into a durable, as well as delicious substance.

TOBACCO was likewise a noted plant in the *West-Indies*, which was yet suffered yearly to rot and perish like other herbs, till the industry of the moderns finding the way of curing it, as they call the method of ordering it, made it, by the help of mere skill, lift in an improved condition for divers years, and fit to be transported, as it plentifully is, over all the world.

THE leaves likewise of indigo, which would uselessly perish like those of other shrubs, by the mere way of ordering them, which too is rather by subtraction than addition, have been long made a lasting pigment or dying stuff, and one of the most staple merchandises that even the *East-Indies* send us.

I MIGHT add, the great use that we are enabled to make of madder, woad, and divers other perishable plants, by the way of ordering them; but there is one instance of this kind so considerable, that though I have formerly named it to another purpose, and though I am willing to mention but one example more of this sort, I cannot but pitch upon this; since it excellently manifests, what may be expected from a skilful ordering of nature's productions, by shewing us, what even the savages of *America* have been able to perform in this kind. For though their mandioca be confessedly a poisonous plant, yet without addition they make of it their cassavi-meal, whereof not only the Indians, but also many Europeans make their bread, which I also have made some use of without dislike. And with no addition, unless it be perhaps that of spittle, they make of the poisonous juice of the same root a not unpleasant nor strengthless drink, which divers, even of the English, compare with our beer. And of the bread made of that cassavi-root, they brew, in some of our American colonies, a liquor by the planters called Perino, which I have known, even by persons of quality, equalled, if not preferred to wine itself.

THE shreds of leather pared away and thrown aside by the glovers, by so slight a way of ordering them, as only the boiling them long in fair water, dissolves them in that liquor, and reduces them with it, the decoction being strained and cooled, into a kind of jelly, that they call size (which may be also made the same way of cuttings of parchment,



ment, and better yet with those of vellum) which is of great use towards the production of very differing trades: some of which productions are already touched upon in this book, to which I shall here only add, for the easiness of the experiment, that the fine red stands, and hanging-shelves, are made with ground vermilion, being only tempered up with it, and laid upon wood, which being thus coloured, is, when it is dry, laid over with common varnish, which preserves it from wet, and gives it a gloss.

It would scarce be suspected, that so white a body as ivory should, among other uses, be proper, without the addition of any black, or so much as dark-coloured body, to yield one of the deepest blacks that has been hitherto known; and yet many of our eminent painters count that black, which they call ivory-black, the perfectest that has been hitherto employed in their art. And this sable may be made of ivory, without addition, only by burning it a-while in a close pot; and we have made it by keeping it a-while among coals and ashes, only wrapped in store of wet paper to keep it from spending its denigrating sulphur in an actual flame; (to prevent which, the pots it is burnt in, are wont to be closed with lute, or otherwise sufficiently stopped) as if artificers were acquainted with the old rule, *adusta nigra, perusta alba*.

AND on this occasion I shall add, that this black made of ivory is so excellent in its kind, that I scarce know any thing so proper to make foils of, for that noblest sort of gems, diamonds. And I remember, that a very skilful jeweller, of whom I bought some of those stones, and whom I employed to set others for me, confessed to me, that burnt ivory was the thing he made use of for foils to the diamonds he had a mind to set well.

ANOTHER instance there is, which I must by no means pretermitt, now that I am endeavouring to shew what the preparation or management of a body, even by illiterate tradesmen, may do to make it fit for unlikely uses. For one would scarce imagine, that from so gross and foul a body, as the *intestinum rectum* of an ox or cow, there should be obtained a transparent substance, more thin by far than paper; and yet of so great a firmness and toughness, as is scarce at all credible to those, that have not been, as I have, convinced of it by experience. But it is certain, that some of our gold-beaters in *London*, and perhaps not there only, do, by cleansing and otherwise preparing the above-mentioned nasty gut of an ox, obtain exceeding fine membranes, some of which I keep, by me, that though clear and strangely thin, are yet of such tenacity, that when the thin plates of gold are put between them, or in their folds, the force of a man frequently striking them, with a vast hammer made of purpose, almost as heavy as he can well lift up, does usually, as I have seen with some wonder, attenuate and dilate the included gold, without being able to break these so fine skins.

THESE instances, *Pyrophilus*, we have hitherto produced, are almost all of them such, as either nature herself, or nature assisted but by tradesmen, and other illiterate persons, has presented us. And therefore questionless, the power that a skilful management may have to produce great changes in bodies, and thereby fit them for new uses, will be much advanced, when they shall be ordered by such, as are either good chymists, or dexterous at mechanical and mathematico-mechanical contrivances, especially, if in the same persons a skill in these two sorts of knowledge should concur.

THAT skill in mathematicks may teach a man so to manage natural things, as to enable him to make other uses of them, than those that want it will dream of, we may be persuaded by several particulars. For we see, that from a bare giving to a piece of ordinary glass a prismatical shape, that diaphanous and colourless body may be made to exhibit in a moment all those delightful and vivid colours, for which we admire the rainbow; and though merely by giving a piece of foliated glass or metalline speculum a concave

a concave figure, it may be made to burn strongly by reflection, yet by giving a piece of glass a convex figure, you may qualify it to burn by refraction, and even with water fitly figured, you may readily kindle fire. For though a round and hollow spherical phial of pure glass will transmit the sun-beams without making them burn, and consequently has not of itself the faculty I am going to name, but serves chiefly to terminate the water, that is to be poured into it, and give it its due figuration; yet by filling a spherical phial, I have taken pleasure so to unite the sun-beams, as, when frost and snow was about me, to make them burn; (and perhaps ice itself, if chosen free from bubbles, and conveniently shaped, may, as some incomplete trials make me hope, be made fit enough for that purpose.) And much more vigorous the accension would be, if two bare concave glasses of like shape, equal bigness, and truly ground, had their edges so joined by a close frame, that the cavity contained between the inside of the glasses and the frame may be filled with fair water; for by this means (the convex side of each glass being outermost) the whole instrument (one or two of which I have seen in a virtuoso's hands) will serve for a double convex glass, which may by this means be made far larger, and more efficacious, than other burning glasses of that figure, which consisting each of them of a single piece of solid glass, are wont to be far inferior in bigness to such hollow ones, as may be easily enough attained.

AND now I have named solid glass, give me leave to take hence a rise to add, that though glass stopples are made only by giving them an almost conical figure, and a superficies fitted by grinding, for an exquisite contact with the inside of the neck of a glass-bottle; yet this way of ordering glasses, which is ascribed not to mere philosophers, but men versed in optical and mechanical trades, produces stopples much surpassing all known before; not only in this, that neither aqua fortis, nor other corrosive liquors, work upon them, but also in their being able to keep in even the subtlest spirits so strictly, that I remember having once forgot some spirit of sal armoniack in a large bottle, which it did not near a quarter fill, when I long after (as I remember about seven years) came to that part of *England* again, I chanced to find this bottle in a place, where, being without an inscription, I knew not what the contained liquor was; and taking off the glass-stopple, to discover by the scent what it might be, upon smelling to that solid body, the adherent spirits operated strongly enough upon my nose and eyes to make me almost stagger, and with my curiosity had been more cautious.

WHAT I have further observed about the way of making, and the applications of this kind of glasses, belongs not to this place, where it would be fit to prosecute my former discourse, by shewing you, how much the chymical management of things may alter and improve them, were it not, that it would be improper to venture upon so copious a subject in one of the sections of an essay, where I shall therefore but point at it, without pretending to treat of it.

WE see that chymists can out of some fruits, that grow wild in the hedges, and are not edible, as also out of the lees of ale and beer, draw an inflammable spirit which, for many purposes (not medicinal) may be made use of for that of wine. We see that out of the dry body of hartshorn, as likewise out of the skull and bones of dead men, and other animals, which have been wont to be looked upon to be so devoid of moisture, that men proverbially say, as dry as a bone, chymists do ordinarily, to the wonder of the ignorant, draw store of spirit, and oil, and phlegm, as they likewise do from the driest woods. Some of them also, of the opacous body of lead mixed with sand, and a few grains perhaps of metalline pigment, can make in a few hours variety of amaufes, or metalline stones, which, by their transparency and lovely colours, do  
pleasingly



pleasingly emulate rubies, emeralds, and other native gems; about the emission of which, I may elsewhere acquaint you with some of my trials.

How unlikely effects may be sometimes produced by a slight spagyric preparation of things, may sufficiently appear by the Bolonian stone, from which (though one would not, upon the sight of it, expect any such matter, yet) being duly prepared by chymical calcination, it acquires that strange property of shining in the dark a while after it has been exposed to the sun, for which it is so justly admired by us, that have seen it, that it is judged unfit to be believed by many critics, that have not.

AND here let me take notice to you, *Pyrophilus*, that very slight circumstances in the management of a body may sometimes produce considerable and unlikely effects.

THAT salt, dissolved in water, is a powerful hinderer of the congelation of that liquor, is a matter of common observation; neither the sea-water, nor brine, being usually frozen with us by such frosts, as turn common water, and some liquors more indisposed than that is, into ice. And yet sea-salt, which being dissolved in water, keeps it from freezing, being outwardly applied to water, does so powerfully concur with snow or ice to make it freeze in artificial glaciations, and is so necessary to the effect, that the snow or ice, without the salt, would not ordinarily, here in our climate, produce in a seasonable time any ice at all, as I more than once purposely tried.

THERE is a certain powder, which by the proportion and mixture of nitre (whereof it chiefly consists) with other ingredients, obtains so odd a texture, that if putting it into a crucible, you should place that upon the coals, as is usually done in other fluxes, the powder would blow up, or take fire with violence enough, and perhaps not without danger; and yet, if instead of kindling this powder from the bottom upwards, you kindle it from the top downwards, there will be no danger in it, but it will make a powerful flux for the reduction of metalline powders mixed with it into a body.

## SECTION V.

In the fifth and last place I consider, that the generality of effects to be performed, being not produced by one single and unassisted production, either of nature or of art, but requiring the concurrence of more; he, that knows not the nature or properties of all the other bodies, wherewith that, on which the experiment proposed is actually, or may be usefully associated, or otherwise employed, can hardly discern all the effects the experiment may possibly concur to produce. For, whereas many inventions or operations consist, as it were, of distinct actions; a body, that seems useless to the main and ultimate effect, may usefully concur to the performance of some intermediate or subordinate part of the operation (by being requisite to which, it may be of no use to the experiment considered in the gross, though not to each distinct part of it.)

THOUGH spirit of wine will scarce (if at all) even in a very long time draw a red tincture out of the flowers of sulphur, yet, when they have been opened, by having been fluxed together with an equal weight of salt of tartar, we have found, that they will in a few minutes, and in a gentle heat, give, in thoroughly deflegmed spirit of wine, a tincture or solution as red as blood; which being freed from the superfluous menstruum, will afford us a balsam much finer than that vulgar one, which is wont to be made of the same flowers dissolved in oil of turpentine.

THAT such amalgams of gold and mercury, as goldsmiths are wont to gild silver with, cannot by ordinary ways be made to adhere either to iron or steel, is a thing so well known among gunsmiths, and such artificers as work upon iron, that when I in-

quired of several of them (as well Dutch as English) whether they could gild iron with water-gold (as they call that way of gilding) by the help of quicksilver, they judged it a thing not to be done: and yet I know a very ingenious tradesman, who was able to perform it, but not (that we may apply this experiment to our present purpose) without the assistance of another body, which was to perform one part before the amalgam could perform the other. The artificer's way was to coat (if I may so speak) the iron or steel to be gilt, with a coat of copper, to which purpose he used distilled liquors tempered with other ingredients, wherein the iron was to be immersed with great wariness and dexterity; for otherwise, not only the trial would not succeed, but oftentimes the iron would be spoiled. To obviate which inconveniencies, there occurred another way of casing the iron with copper, namely, by dissolving very good vitriol, that has copper in it (for it is not every vitriol, that is fit for the purpose) in warm water, till the liquor be satiated with vitriol, and immersing several times into this solution the iron, first scoured till it be bright, and suffering it each time to dry of itself; for this immersion being repeated often enough, there will precipitate upon the iron enough of the cupreous parts of the dissolved vitriol, to fill all its superficial pores with particles of copper. So that by this safe, cheap, and easy way, having, as it were, overlaid your iron with copper, you may afterwards gild it as copper with the above-mentioned amalgam, which will adhere to copper, not to iron.

BUT here we must not omit an observation very considerable to our present scope, namely, that though the several parts of an experiment or a process may in most cases, each of them be purely physical, or chymical, &c. yet in divers other cases, it may far more usefully be so ordered, that one part of it may be physical, (taking here that term in contradistinction to subordinate parts of learning) and several, or each of the rest may belong to other arts, as one may be chymical, and another statical, another mechanical, another hydrostatical, &c. and by such a concurrence of differing parts of knowledge to the same operation or production, I doubt not that many things may be performed, that have not yet been attempted, nor so much as thought of. For he that has skill but in one of these single parts of learning, must needs have his attempts as well as his knowledge much straitened, by confining himself to operate by such means and instruments as are within the compass of his own art; which, assisted by others, may bear a good part in the performance of divers considerable things, which it is by its self very insufficient to accomplish.

OF this we may take notice of some instances in the productions, that art and nature have presented us with already; for not only handicraft trades, as we have formerly noted, do many of them assist each other in their operations, but even those arts, that are counted ingenious, have sometimes need or use both of the service of the more mechanical trades, and of mutual assistance among themselves. The masters of capotricks know very well what would be the properties of spherical, cylindrical, and other specula; but to procure such specula, you must have recourse to the chymist, or the founder, whose part it is by artificial mixtures of metals and minerals, and by mechanical contrivances, to cast bodies, that give a more sincere and vivid reflection, than the single metals would do, and to give them withal that curious polish, for which the metallists and chymists are beholden to smiths, stone-cutters, watch-makers, or other handicrafts men.

ANOTHER eminent example to the same purpose may be taken from the consideration of organs used in churches. For to devise the rules of making them well, there is first requisite no small skill in the speculative part or theory of music: next he that would make the instrument well, must know how to choose wood proper for that purpose,  
(*mult*)



(most woods being unfit for it) how to season it, and how to discern, whether it be duly seasoned, and otherwise well conditioned. To excavate and fashion the pipes, and other parts of the instrument, that are made of this wood, there is use of the turner's and joiner's crafts. It is often needful also, that the organ-maker be skilled in the effects of metals, and perhaps their mixtures; and the ways of casting them, in order to the making of his pipes of a sonorous matter, and to the giving them a due shape, and other desirable qualifications. I might here borrow further instances from bells, lutes, harps, and other musical instruments; but I hasten to examples of another kind.

HE that has never so attentively considered the nature of salt-petre or of brimstone apart, shall never be able to make the considerablest uses of either of them, till he skillfully associate them to one another, and incorporate them into that wonderful body, called gun-powder, which will afford us an instance fit enough to explicate what we have been saying: for consisting of three differing ingredients, nitre, brimstone, and charcoal, though neither of these be sufficient, *in omni genere* (as they speak in the schools) yet each of them is very useful by being sufficient *in suo genere*, and really concurs to the effect produced by them all, as you may elsewhere find more particularly declared.

HE must remain ignorant of another considerable use of sulphur, that is unacquainted with some properties of common oil and calcined alabaster. For artists have a way of making moulds, wherein to cast off the impression of medals, and other works embossed on metals, which, though the effects of it seem strange to those, that know not how they are produced, they easily thus perform. They make about the embossed work, whose impressions they desire to have, a little border or ledge of clay, to hinder the melted sulphur to be poured on it from running over; then they lightly (but very carefully) with a pencil or feather anoint the metalline work with oil, to hinder the sulphur from adhering to it; then they melt good brimstone in any convenient pot (which they cover well to prevent its taking fire) and whilst it is hot, they pour it gently upon the embossed metal, all whose exances will make perfect impressions on the lower surface of the thus melted brimstone, which ought to be poured on in a considerable quantity, that the moulds thus made may prove the stronger. About the edge of this mould they make a little rim or border of clay as before; and lightly anointing both all the surface of the mould, and the inside of the clay with oil (which if it be too copious, is, as we have tried, apt to prejudice the accurateness of the impression) they pour in by degrees to the thickness of about a fourth of an inch of that mixture I formerly mentioned (in the eighth essay) to be made of recently calcined alabaster, stirred and incorporated with such a quantity of fair water, as may suffice to bring it to the consistence of the thicker sort of honey. And this mixture in about a quarter of an hour growing hard, and then being taken out of the mould (to which the oil hinders it from sticking) will, if the work have been dexterously done, and the mixture before affusion carefully freed from bubbles, perfectly exhibit the shape and dimensions of the work embossed upon the metalline pattern. And by this way in a few minutes have we sometimes cast off a coin, a medal, and sometimes too a whole landscape, without any trouble, and not without some delight.

AND here, *Pyrophilus*, let me perform what I lately intimated an intention of, by now taking notice to you in this fifth section of this essay (of what I had not long since occasion to observe in a former part of it) that you may oftentimes find such particular bodies conducive to the main effect of an operation or experiment, by performing some subordinate part or office in it, as yet may seem nothing at all of kin to the ultimate effect promised by the perfected experiment.

THAT aqua fortis, that so greedily corrodes and devours silver and brass, should eminently conduce to the real silvering over of the latter metal by the former, is that, which few goldsmiths, or even chymists would judge probable. And yet this fretting liquor performs a principal part in that ingenious way of silvering over brass and copper, which is more applauded than known. For first, aqua fortis serves very well to make clean such embossed or otherwise uneven pieces of metal, whose inequality hinders us from being able to cleanse their little cavities with tripoli, or those other powders commonly used to scour brass: whereas if such bodies be lightly washed over with aqua fortis, and immediately thrown into fair water, the foulness may be fretted off, and the work not disfigured. And this is esteemed the best way of scouring such metalline pieces of work by the best maker of mathematical instruments, that I have met with. And I the rather mention it to you, *Pyrophilus*, because that though it be not always requisite to our experiment of silvering (for many pieces of brazen work may well enough be made clean after the ordinary manner) yet divers trials have assured us, that the scouring of the brass and copper is necessary to the success of this experiment; probably, because any grease or filth remaining upon the surface of the metal is sufficient to keep out those little parts of dissolved silver, which ought to lodge themselves so thick in the pores of the metal, as to seem one continued silvered body.

THE remaining part of this operation may be thus performed. The metal to be wrought upon being made very clean, you must dissolve good silver (the finer the better) in aqua fortis in a broad bottomed vessel of glass, or at least of glazed earth; and having, over a chafing-dish of coals, or with some such heat, evaporated away all the aqua fortis, you must upon the remaining dry calx pour of water five or six times its quantity, or as much as will be needful perfectly to dissolve it. This water with the like heat must be forced away as the former menstruum, and the like quantity of fresh water must be poured on, and evaporated quite away the second time, and, if need be, the third time, toward the latter end making the fire so strong, as to leave a perfectly dry calx; which, if your silver has been good, will be of a good white, and will by these operations be competently freed from the stinking and fretting spirits of the aqua fortis. Of this calx you must take one part, and about as much (in quantity, not in weight) of common salt, and as much of crystals of tartar (or at least powder of good white tartar) as of either of the former ingredients; which, like this, ought to be finely beaten; and these three powders being exquisitely mixed, you must plunge the scoured brass, to be silvered over, into fair water; and then taking up as often as need requires, with your wet fingers, some of the newly mentioned mixture, you must rub it on well, till you find every little cavity of the metal sufficiently silvered over; remembering, that if you would have it richly done, you must rub in more of the powder. And last of all, you must wash well your silvered metal in fair water, and rub it very well and hard with a dry cloth, that it may appear smooth and bright. And this way of silvering, though it be presently and cheaply performed without quicksilver, yet may be made to last some years, as experience has partly informed me, and may be easily renewed, when the silvering begins to decay or wear off.

AND here, *Pyrophilus*, it will not be improper to give you this advertisement, that we ought not to conclude, as we are very prone to do, that such an use is not to be expected, or endeavoured to be obtained from such a thing, because we see the like use to be made of things, that are thought to be of a quite differing nature from that we consider, or perhaps quite contrary to it: for in many cases, as there are more ways than one, or even than a few, to bring to pass a thing proposed; so among the various instruments, that may be employed the same purpose, some may exceedingly differ be-



tween themselves as to other qualities, and yet agree in that, which is requisite and sufficient for the performance of the thing designed. As though, for instance, resin and sal armoniack be differing in colour, smell, taste, weight, hardness, &c. though the one be a vegetable concrete juice, the other an aggregate of urinous, fuliginous, and marine salts; the one readily dissoluble in water, the other not dissoluble in that liquor, but in oil; and though there be I know not how many other differences between them; yet either of them single may be, and is, usefully employed for the tinning of brass and copper-vessels, each of them being endowed with a fitness to make tin stick to those metals, as I elsewhere more particularly declare. Thus, though water, sand, and tin, are bodies in other respects very unlike, yet the two latter are found fit to make hour-glasses, as well as the first; though that alone, as is presumed, were for many ages employed by the ancients for that purpose.

To the foregoing advertisement I shall annex another, that may seem very differing from it, but yet is no less true; namely, that we are not always to suppose, that because a natural body has such an use on some occasions, the same body cannot on other occasions be employed to uses, that seem of a quite differing, and perhaps of an opposite nature.

THIS I conceive may be done principally by these two ways. First, by the differing constitutions of the several bodies the same agent works upon; as when the heat of the sun melts wax and hardens clay; and the same spirit of vinegar, which on filings of copper will by digestion obtain an abominable taste, will upon filings of lead acquire, by the same way, a very great sweetness: and spirit of salt, that will dissolve copper and iron, as aqua fortis also does, will yet precipitate silver dissolved in that menstruum. And to this first way I shall subjoin the second, which is, that such a parcel of matter, as is wont to be considered as one and the same body, may contain in it parts of very differing natures, upon whose account its operations may be diversified. Thus when we calcine some unripe minerals with nitre, the inflammable parts of the nitre do burn up and dissipate into smoke the volatile and combustible parts of the mineral; but by virtue of the remaining alkali of the nitre, several other parts of the mineral are made far more fixed and capable of enduring the fire, than they were before. So sulphur has in it some parts, that make it more readily inflammable than even nitre or oil; and yet it abounds with acid and vitriolate particles, that are not inflammable themselves, and much resist the accension of flame in divers other bodies. And accordingly, though in matches used in tinder-boxes to take fire readily, the kindled brimstone acts upon the shivers of wood, whose ends were crusted over with it, as an ordinary flame; yet the same burning body, by virtue of its acid parts, works in another capacity, than that of a common flame upon some metals, especially iron, and likewise upon the leaves of red roses, which its fumes turn white.

I COULD, if it were needful, propose in this place sundry other instances of the differing actions of the differing parts of a body, and could likewise subjoin other cases, than I have yet mentioned, wherein bodies may be applied to uses, that many would be unapt to expect from them: but judging it more convenient to reserve those for other places, especially in the last essay, I shall conclude this with the two following advertisements.

THE first is, that I have in all this discourse purposely forbore to treat of the medicinal uses of things, because my scope in the volume, whereof this essay is a part, obliged me so to do. But yet I am sensible, and would have you so too, that hereby I have forbore to employ a multitude of particulars, that would have much enriched this treatise. For there is a great number of bodies, both natural and factitious, that being employed

as medicines for human bodies, have there very various and sometimes seemingly repugnant operations, many of which would serve to illustrate and confirm sundry passages of this essay. Thus rhubarb, whether taken in substance or infusion, does by virtue of its differing parts, first purge, and then bind. Spirit of wine taken inwardly exceedingly heats the body; whereas outwardly it is employed to appease the heat caused by some hot humours and inflammations. Mercury taken inwardly, crude as it is, has often, though not always, proved an effectual and harmless medicine in worms, and some other distempers, even to children and women in labour: but the same mercury rarefied into fumes (which yet may be condensed again into running mercury) and in that form taken into the body, does too often cause vehement and dangerous commotions in the juices of the body, as excessive salivations, fluxes, &c. declare. And he that shall attentively consider the various operations of that one mineral antimony, and the not only differing, but oftentimes contrary effects, that it produces, according to the complexions and dispositions of the taker's body, and the preparation of the mineral itself, will not, I presume, stick to allow me, that the medicinal uses of things, if I had not thought fit to decline them in this essay, might have much increased the number of instances it contains; the effects of others bodies upon those of men being no less proper instances of nature's ways of working, than the changes they produce, when they work only upon one another.

THE second advertisement, wherewith I shall conclude this essay, is, that though what I have hitherto discoursed, hath almost solely related to the neglected uses of particular natural bodies; yet I would not have you thence take occasion to imagine, that there are not other natural things, whereof divers uses may be made, that men have hitherto either ignored, or overseen. By other natural things I mean the differing states of matter, or of bodies (such as rarity and density, fluidity and firmness, putrefaction and fermentation, may seem to be) as also the more operative qualities, such as heat, cold, gravity, &c. the laws of local motion among the parts of matter, and the present fabrick of the universe, and especially that of our terrestrial globe and its effluvia; to which might be added other things in nature, that are not properly bodies in the usual sense of that word, but may be called things corporeal, as they belong to bodies, and entirely depend on them. In favour of this advertisement it were easy for me to suggest to you such a multitude of particulars, that reserving some few for the last essay, I here purposely forbear to mention any at all, to avoid being enticed or engaged to enter upon a subject that could not be otherwise than very lamely handled, without enormously swelling an essay, that does already exceed its just dimensions.



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T R A C T S.

O F

A Discovery of the ADMIRABLE RAREFACTION of the AIR.

New Observations about the DURATION of the SPRING of the AIR.

New Experiments touching the CONDENSATION of the AIR by mere COLD ; and its COMPRESSION without mechanical Engines.

The admirably DIFFERING EXTENSION of the same Quantity of AIR rarefied and compressed.

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A D V E R T I S E M E N T.

THE Author of the following papers supposeth his readers to have learned, either from the books he hath published, or from what hath been borrowed thence by other writers, the structure and more familiar uses of a pneumatical engine of his, mentioned by several authors under the name of *Machina Boyleana* ; with whose description therefore those are desired to acquaint themselves, that shall think it worth the while to understand, as well as read, the following papers ; about which it might be further taken notice of, that the first of them was indeed written to a learned friend, though his name be not now annexed (for certain reasons) ; presently after which the three others were thought fit to be subjoined. As for the omitting of the compliments and forms, usual at the close of epistles, the author did it, as well to spare the reader, as himself ; who hopes he may be excused, if the transitions from one discourse to another, and even the stile and method of them, be not so smooth and regular, in regard the ensuing writings were traced, when he was afflicted with a great fit of sicknels, that kept him from so much as once reading over himself, what he had indited.

A D I S.

A

## D I S C O V E R Y

OF THE ADMIRABLE

## R A R E F A C T I O N O F A I R,

(EVEN WITHOUT HEAT)

I M P A R T E D

In a LETTER to a FRIEND.

DO not imagine, Sir, that I did at all wonder to see you yesternight so much admire, to hear me talk with so much seeming extravagancy about the rarefaction and condensation of the air; for I confess, that I did deliver something on that occasion, that might easily, at first sight, appear so near impossible, as to be utterly improbable.

AND though you were pleased, even on such an occasion, to express a very favourable opinion of my veracity, yet thinking it fit, that such an obligation should not divert, but engage me, to endeavour to justify you to yourself, by confirming what I said to you; I have already sought and found among papers, many years since laid aside, some that will enable me to make good more, than what the diffidence of my memory allowed me to say in the very boldest part of my yesternight's discourse. For now that I luckily find not only the originals of the relations, whereof this paper contains copies, but that my engine is in good order; I am so far qualified to countenance a discourse, wherein I kept somewhat within compass, that though it will perhaps cost me much pains and trouble, to make *extempore* experiments fully equal to the inclosed; yet if any just doubt should require it, I presume, I can make ocular proof of, at least, as much as I last night told you.

AND now it is time, after having contrary to my custom, raised in you a high expectation, that I endeavour in some measure to answer it, which I hope I shall the more easily do, because the agreement you have often had occasion to observe between the relations registered in my adversaria, and the phenomena of the experiments they describe,



scribe, will, I presume, make it needless to persuade you, that the ensuing trials, being transcribed thence, may be safely credited. Wherefore I shall proceed to annex them, as soon as I have presumed a few historical lines, by way of introduction to them.

It is now many years since, that having a desire to reduce the air to a degree of rarefaction, that appeared to be considerable, upon surer grounds than slight conjectures, I attempted to do it by the help of heat, and particularly by that of an aeolipile, which I have mentioned in another tract\*: but finding, that the diligent *Mersennus* had, if there be no mistake in his account, been able to rarefy air that way, full as much, or more than I could, I betook me to try, whether I could not, by the spring of the air (without heat) procure a greater expansion of it? I found (as I have long since elsewhere† related) that in the pneumatical engine, which has been since called *Machina Boyliana*, I could encrease the expansion of air, till the body attained to about one hundred fifty-two times its former and usual dimensions. But this expansion, though it were above twice as great as the utmost procured by *Mersennus*, did not yet satisfy me, but put me (according to what I there intimate) upon another contrivance, which though put in practice eight or nine years ago (as the date of one of the trials may inform you) had the relation of its successes laid aside among those of others, made in the same engine, which yet lie by me unpublished. So that I may now proceed to give you the transcripts of the trials themselves, as they were hastily and inelegantly, but very faithfully, set down among my Pneumatical Collections. And this I am ready to do, as soon as I have intimated to you, that in that noble collection of experiments, that has about two years since appeared in publick, as the first-fruits of the justly famous Florentine Academy, I find, that those virtuosi had, according to their sagacity, so advanced the extent of the air, as without the help of heat to bring the dilatation to exceed one hundred seventy-three times its former dimensions; and that, which made their improvement the more considerable, and consequently the more worthy of them, is, that they procured this great rarefaction, as well as I had done mine, by the air's own spring; and had surpassed, without the help of my engine, what I was then at first able to do by the conveniencies that it afforded me. Whereupon, remembering what I had performed in that kind several years before, I sought among my papers for the trials I had then made, and found those notes, whereof, I now, at length, think it high time to give you the promised copies in the following terms.

# EXPERIMENT I.

WE took a round glass-egg (as they call them) of clear metal furnished with a pipe, or shank, of some inches in length; this we filled with water, and conveyed both it and a phial with water in it, into a receiver of a convenient size, and by pumping the air out of it, we made the bubbles both in the egg and the phial to disclose themselves in great numbers; so as to make the liquor in the glass-egg seem to boil, and to make all that was in the shank really to run over. When we thought the water was sufficiently freed from air, which it was not quickly brought to be, we took out the glasses and filled up the pipe of the egg with water taken out of the phial, and inverted it into more of the same water, in such manner, that the egg was quite full, shank and all, excepting a small bubble of air, that we purposely left to gain the top of the egg; where, the glass being transparent, with a pair of compasses we measured as accurately as we could, and found it to be a tenth, and less than two centesims of an inch. Then putting the glasses

\* New Physico-Mechanical Experiments, Exper. VI.

† Ibid.

El. 12.  
Propof.  
ult.

again into a receiver, we set the pump at work, and the little bubble, after a while, began to expand itself, which when it had once done, it did at each suck strangely increase, till at length it drove all the water out of the round part of the glass. And lest it might be objected, that it was only the subsiding of the water upon the withdrawing of the outward air, that before kept it up to the top of the glass, we caused the pumping to be so continued, till the expanded air had several times driven the water in the pipe of the egg, a pretty way beneath the level of the external and surrounding water in the other glass. This done, we let in the air by degrees, with a design to observe, what bubble we should find at the top of the egg, when the water should be again driven up into its cavity. But the expanded air had forced over so much water, that there remained not enough to fill the globulous part of the egg: wherefore we tried the experiment again, and when we had proceeded thus far, we compared the above-mentioned diameter of the small bubble, with that of the spherical part of the glass, which we took with a pair of calliper compasses: and though we found it to be somewhat more than 20 times as great, yet being willing rather to disavour than flatter the experiment, we supposed the two diameters to be as 1 to 20, and consequently, since, as *Euclid* demonstrates, the proportion between spheres is triplicate to that of the diameters, and in our case, the cube of the lesser diameter being one, is also but one, the cube of 20, the greater diameter, must be 8000; and so the air appears to have, by expanding itself, acquired a place 8000 times as big as it possessed before. Nor was it overseen by us, that the globulous part of such glasses as we used is scarce ever made spherical. But not only I, but Dr. *Wallis*, who was pleased to assist at the experiment, concluded, that the cavity of the shank, which the expanded air drove the water from, but which we did not compute, would make abundant compensation for the two above-mentioned particulars. After this, for further satisfaction, we took water, laboriously freed from air, and putting it into the same glass-egg, we inverted it as before, but left not any bubble in it. This we did, that in case we could make the water subside, the experiment might prevent a suspicion, that some air latent in the water might increase the bubble that was formerly left in it; having then exhausted the receiver as much as before, and if we mistook not, more, the water in the egg did not all subside; but at length, with obstinate pumping, a bubble disclosed itself, and drove all the water clear out of the round part of the glass; and though by reason of some small leaks, that we could not find or stop, we were not able, as before, to make the expanded air depress the water in the shank, beneath the surface of the external water, yet we wanted very little of it; and then out of weariness giving over, we found, that when the water was impelled up again into the egg, there was at the top of it a bubble, whose diameter we measured as faithfully as we could, and found it to be the diameter of the globular part of the glass, as 1 to 14; so that, though the little bubble had been a perfect sphere, yet spheres being, as was lately noted, in triplicate proportions to their diameters, the bubble when expanded, must have been 2744 times as big as the bubble unexpanded. But Dr. *Wallis*, who will be allowed to be a very competent judge in these matters, observing (what we all took notice of) the great thinness of the bubble, positively and constantly affirmed, that he could not estimate it to be at most any bigger than the third part of a perfect sphere of that diameter; by which estimate the expansion of the bubble must have reached to 8232 times its natural dimensions.

N. B. By letting in water into the receiver, as much as it would admit, we found, that by reason of some secret leak, we had not been able so to exhaust it, but that there remained some air.



EXPERIMENT II.

A SMALL and almost inconspicuous bubble expanded itself, when the ambient air was June 2, pretty well exhausted, to more than 10,000 times its former extent. The manner thus: <sup>1662.</sup> we took a small bolt-head, blown by a lamp, which contained in all about 80 grains of water, and inverting the small neck into a jar of water, it was included in the receiver, and the ambient air being exhausted, store of bubbles rose out of the water, and expanding itself, quickly drove all the water out of the bolt-head. Then re-admitting the outward air, the bolt-head was presently almost filled, and all the expanded air shrunk into a bubble, little bigger than a small pin's head; then taking the bolt-head out of the water, and inverting it, that the bubble might get out at the neck, we carefully filled it up with the water that had been freed from air, and then inverting it as before into the jar with water, we again included it, and after some exsuctions found, that there was gotten out of the water into the neck a very conspicuous bubble, which, upon the admitting of the air, shrunk almost into an invisible one, and ascended into the head of the glass. Then again exhauſting the receiver very well, we found it expand itself, so as to fill all the capacity of the bolt-head, and to drive out almost all the water. And upon the re-admitting of the air, it again shrunk into a bubble, whose diameter (according to our best estimate) was not bigger than one two-and-twentieth part of the diameter of the head of the above-mentioned glass; so that to fill the whole cavity of the head only, it expanded itself 10648 times: but because it filled likewise the greatest part of the neck, we found by weighing the water that filled that part, and the water that filled the head, that the capacity of that part of the neck was almost a third of the capacity of the head, being as 141 to 481: if therefore 481, the capacity of the head, contained it 10648 times; 141, the capacity of the neck, must contain it  $3121 \frac{1}{3}$  times; so that in all, the small bubble of air was expanded to above 13769 times its former bulk.

THE diameter of the small bubble retracted was  $\frac{1}{17}$  of an inch.

THE diameter of the outside of the head of the glass was  $\frac{3}{8}$  of an inch.

THE water that filled the head only weighed  $60 \frac{1}{2}$  grains.

THE water that filled the head, and as much of the neck as the air had before expanded itself into, weighed  $78 \frac{1}{8}$  grains; so that that part of the neck weighed  $17 \frac{1}{8}$  grains.

THE bolt-head itself weighed 15 grains.

I MIGHT have set down this second experiment unaccompanied either with the first, or with that I am going to subjoin; because the expansion produced by neither of them was, at least by measure, so vast, as that produced by the trial newly mentioned: but this was so stupendous, that I thought it not so fit to present it to you by itself alone, but rather accompanied with other experiments, the least prosperous of which produced a dilatation of air sufficient for my present purpose, and such as may not a little confirm, that what is recited in the second experiment, was neither a lucky chance, or mistake. And that may be enough for my present purpose; for as for the little abatements, that some will perhaps think fit to be made upon the score of the unequal thickness of glass or some such circumstances, they are not considerable enough to deserve to be now solicitously debated, nor to hinder the expansion, that must be granted from proving what they are alledged for: wherefore I will proceed to what follows.

EXPERIMENT III.

WE tried this experiment again, and found a small bubble, much about  $\frac{1}{12}$  of an inch in diameter, filled not only the ball at the end of the bolt-head (which was  $1 \frac{1}{2}$  of an inch

inch in diameter) but the whole neck, which contained near as much water as the head, and beat down the surface of the water within the pipe, much below that of the water without the pipe.

THESE experiments already found among my old papers will, I hope, without seeking for more, suffice to manifest, that the expansion, which the air may be reduced to without heat, is indeed admirable; for if we make an estimate of it but according to the experiment, which had the most moderate success, it appeared, that one space possessed, though not adequately filled, by a portion of air, may have its air extended to at least 2744 spaces equal to it; I say, at least, because very probably it was above twice as great: and if we make our estimate according to the most prosperous of our trials, we must allow the air to be rarefiable at least 13000 times; I say again at least, because I am not sure, that in that trial it was reduced (not fully, though perhaps very near) to the uttermost degree of rarefaction attainable in our engine: so that I presume you will now grant, that I spoke warily and much within compass, when I mentioned but an expansion from one to a thousand.

AND now having performed the promise I made you, it remains only, that I take notice of the request that you made me, about communicating these experiments to the curious. But this desire of yours is opposed by no small inconveniencies, that would resist my compliance with it. For it would oblige me, by tearing out these papers, to dismember a collection long ago in making, and wherein they were placed to be much otherwise disposed of, and not only make a great gap in it, but strip or deprive it of some things, that were the likeliest to recommend it. Besides that these appearing before the rest are odd enough to make these seem far less uncommon, than perhaps otherwise they would. Yet all this notwithstanding, I find it uneasy to refuse what you, and those friends that concur with you on this occasion, desire, that if after having once more perused these papers, you persist in the same earnestness you expressed yesterday, when you had not yet seen them, I shall not refuse you the disposal of them, both for the reason now given, and because I have been informed as well by you as by other means, that the rarefaction of the air is at present the subject, that busies the disquisitions of several eminent virtuosi, both domestick and foreign, to whom I pay so much respect, that I shall think it a happiness, if it may be acceptable to them, not only because it will be seasonable, but because, that though the engine, that most of the attempts were made in, has not been thought altogether barren, yet these trials will probably pass for one of the least inconsiderable productions of it: and these two services I hope this short writing may do several ingenious readers; the one, that it will invite and accustom them to take notice of, and consider the great subtlety of nature, and the scarce imaginable smallness of those aerial instruments that she employs even about visible operations: the other, that these relations will excite the more curious and piercing wits to debate, and I hope help them to solve the two problems here proposed to them; what figures and motions may be assigned to the particles of the air, to explicate it's so wonderful rarefiableness, and that perhaps without quite losing its durable spring, and how the air comes to be rarefiable so many times more without heat, than hitherto we have found it to be by heat. To which might be added, as a third, what might be reasonably conjectured about that part of the cavity of an exactly closed glass, where, though the eye discovers no visible substance harboured in it, it appears not, that the common air does adequately fill so much as the ten thousandth part?



NEW OBSERVATIONS  
 ABOUT THE  
 DURATION OF THE SPRING  
 OF  
 EXPANDED AIR,

(Subjoined by way of APPENDIX to the foregoing EPISTLE.)

**F**ORASMUCH as reviewing the former paper about the Rarefaction of the air, I took notice in the close of it of an expression (viz. and that perhaps without quite losing its durable spring) which, I fear, may, to some readers, seem to need explication; it will not be improper on this occasion to subjoin something by way of appendix about it.

FIRST then, the reason why, in this short intimation, I thought fit to employ the diffident term *perhaps*, was, because I had not (nor yet have) been taught by trial, whether and how far the utmost expansion of the air actually produced in my engine, or otherwise procurable, and its retaining a sensible spring, are consistent. I express myself thus, to insinuate, that I thought of other instruments and methods, whereby the dilatation of the air may not improbably be measured and promoted; as by making the Torricellian experiment in a glass with a very capacious head or globulous part, and applying the aerial particles, that will ascend out of the subsiding mercury together with a bubble of other air, if it be needful, to the use we have been speaking. Something also may be done to some purpose, with very fine and large fish-bladders; but I shall not insist on these or the other expedients that came into my thoughts, contenting myself to have intimated, and thereby acknowledged, that there may be other means besides the Machina Boyliana, to bring air to a very great expansion. But whether any of them will surpass what has been actually attained in that engine, time must declare; till when, we shall be content to make use of the experiments it has already actually furnished us with.

WHEREFORE to come to the second or other remaining part of it; whereas in the mentioning of the spring of the expanded air, I employed the attribute of durable, you may easily gather the reason from what I am now going to annex.

I HAD

I HAD observed, not without some wonder, in the enquirers into the nature of the air, that they have not, that I know of, so much as attempted to discover, whether the air, either in the utmost or in the intermediate degrees we can bring it to, does retain a constant and durable elasticity?

For, first, men have not determined whether a portion of our common air being exactly shut up in an hermetically sealed glass or some other exactly closed vessel, will constantly and uniformly, for a moderate time at least, retain the degree of elasticity it had when it was shut up; and whether it will not sometimes vary its pressure, as we see, that the atmospherical (though I think upon peculiar grounds) is, by the help of our baroscopes, observed to do? Next, it does not appear, whether included air, in case it retain an uniform elasticity for a moderate time, will retain it for a very long one. Nay, whether it would not at length come not to have a weaker spring, but perhaps to have no sensible spring at all, as we see it happen in sword-blades and divers other springy bodies, which, after having stood too long bent, will continue so, and lose their former power of self-restitution, as they call it.

THIRDLY, men have not yet determined any thing about the degrees of the air's elasticity, whether the durability and uniformity, or varying of its strength, may not depend upon the differing degree it had when it was first shut up.

FOURTHLY, much less have we yet attempted to discover, whether the spring of an inclosed portion of air may be sometimes weakened, and sometimes strengthened by the changes, as to gravity, of the outward atmospherical air, the new and full moon? To which I might add divers other external accidents, which, as yet, we scarce suspect. And to these I might add some other doubts and enquiries that may not be impertinently suggested, but here would, I fear, pass for a digression.

WHEREFORE I shall proceed to tell you, that having taken notice of it, as an omission among the inquirers into the nature of the air, in whose negligence I was too long a sharer, that we have not, that I know of, so much as attempted to discover this itself; whether the air, either in the utmost or in the intermediate degrees of rarefaction, we might bring it to, would for a considerably long time retain its elasticity, or at least some determinate degree of it, or lose it by determinate and regular decrements, I thought fit to make some trials about this matter, but cannot brag of the success of my intentions; having been hindered either by want of instruments, or by removes, or by sickness, or by unlucky accidents, or by one unwelcome thing or other, from accomplishing what I had chiefly designed, and partly also made some progress in it; but yet to give you some hints, as well as some occasion to more prosperous experiments, I shall not stick to annex, what I readily call to mind about my attempts on that occasion.

I REMEMBER then, that when I first began to try something in order to my design, being destitute of fit accommodations, I was fain to content myself, by causing a good bubble of glass, with a stem to be so blown at the flame of a lamp, that whilst the ball was yet exceeding hot, and consequently contained none but highly rarefied air, the stem was very nimbly clapped into the flame of a candle that was purposely kept ready at hand; so that being slender, it was in a trice sealed up, and the air within remained as much expanded as the great heat it had been exposed to had brought it to be. This bubble many months after I inverted into a basin of water, and having broken off the seal under the surface of it, the liquor was violently impelled into the cavity, but yet was not able to fill it, a considerable part being defended from the further ascension of the water by the spring of the remaining air, which, for all the long stretch it had been put



to, had not lost any thing of its spring that we could take notice of. But this was a trial in which I could by no means acquiesce; and therefore when I was a little more befriended by opportunity, I tried another way, partly to give a somewhat pleasing surprize to unaccustomed beholders, and partly, because though it could not shew all that I desired, yet it might plainly shew, that the air, even at a very considerable extension, would hold out for a considerable time. Wherefore leaving a very small proportion of air in the folds of a fine limber bladder, whose neck was very closely tied, I caused it to be, by the help of the *Machina Boyliana*, so expanded, that at length it so dilated itself, as to seem to fill the whole bladder, and reduce it to the extent it had just before it was emptied; and the bladder, by a peculiar contrivance, was so included in another vessel, that being protected from all intrusion of the outward air, it maintained its plump and tumid figure, and in that unwrinkled state I shewed it many months since to some virtuosi, now here in *London*, after it had continued so, if I mistake not, near two years. Since the writing of this, I did, at length, find the newly-mentioned vessel, and shewed it to some curious spectators, who, with me, took notice, that the included bladder, instead of being wrinkled or shrunk, appeared to be plump and full, as well blown bladders are wont to be. So that many months, perhaps a dozen, may be added to the freshly mentioned duration of the expanded air.

BUT this way satisfying me neither as to some of the particulars I desired my attempts should discover, I devised a little instrument, whose contrivance, though it seemed very simple, promised, and for some time gave me a far more accurate account of what I expected. The instrument, if you desire it, I can easily shew you, having lately been forced to make a new one, which is now by me; but it may suffice to tell you, that it is so framed that it is fit to discover, besides divers other things, whether, and how long air brought to the greatest expansion I could conveniently reduce it to in my engine, will retain its spring; and by what degrees or stages and periods of time, the decrement, if any be, is made? But of the issue of the trial made in it, I can give you but a very imperfect account, in regard, that, though I made it about three years ago, yet having left the instrument in a place, where it is so lodged, that I cannot have it without returning thither, till I see it again myself, I dare not venture to judge of the success of the experiment; only this I remember, that I took no notice of any observable diminution in the air's elasticity, though it were pressed, and, as it were, clogged with a weight, that one would wonder how it could, when it was so highly rarefied, support for one minute\*.

\* See the  
Postscript.

THERE is also another way that I contrived, wherein the air in a little portable instrument, which I can shew you, being expanded, as one may guess, to five or six hundred times (perhaps a thousand times) its wonted extent, has not only for a long time preserved its spring, but satisfies me also about one of my chief queries, which was, whether the air, very much dilated without heat, would be considerably sensible of external heat? which it plainly appears to be in this instrument, where, notwithstanding the great rarity it has already attained and seems likely to preserve, the heat of one's hand applied to the outside of the vessel has a quick and very manifest operation; and upon the withdrawing of it, the sensible air quickly returns to its former dimensions, as well as temper; so that one may employ it as a kind of weather-glass, and perhaps make some discoveries by long comparing it therewith.

BUT hitherto I have been doing what I do not love to do, and very rarely have done, when I mention my own experiments, that is, I have not punctually specified any determinate quantities and proportions of the things spoken of; but one of my former trials I have newly found out registered in a loose note, and therefore the quantities being annexed,

ed, I hope it may both give some countenance to what I have been saying, and give some, though not an entire satisfaction about the thing itself.

March 18 A GLASS, as cylindrical as we could get it blown at our lamp, and having a long stem coming out at the unsealed end, was quite filled with water and inverted into water placed at the bottom of a large pipe sealed at one end, and of three or four feet in length. This external pipe, so called for distinction sake, was exhausted, till the air that disclosed itself in the water of the internal pipe, had drawn out the water in the cylindrical pipe as low as the upper part of the stem; at which great expansion of the air, the external pipe being speedily and securely closed by a certain contrivance, the air, thus rarefied, was kept sometimes in my own chamber, that was warmer, sometimes in an under room; and after it had been kept from first to last about eleven weeks or three months, if I mis-remember not, without any other remarkable variation than that in the cold room the water ascended, as I guessed, about an eighth, or near a fourth at that part of the internal pipe; where the lower end of the cylinder gradually lessened itself into the slender stem. Yesterday I invited Dr. *Wallis* to be present at the breaking of the glass, and to favour me with his assistance for the better estimating the expansion of the air upon the breaking of the closed apex. The water was but leisurely (because of the slenderness of the orifice that was made for the air to get into it) impelled up into the formerly deserted cavity of the cylinder, which it filled all, save a little bubble, which was exceeding shallow. We made use of our eyes at a fit distance, and of compasses both ordinary and calliper, to obtain these measures. The cylindrical part of the internal pipe was three inches in length, and three fifths of an inch, or less, in diameter on the outside. The bubble was two tenths in diameter, and about two centesms in depth; from all which, by the doctor's calculation, the bubble, to the space it possessed unexpanded, was as one to one thousand three hundred and fifty.





## NEW EXPERIMENTS

TOUCHING THE

CONDENSATION OF THE AIR

BY MERE COLD,

AND

Its COMPRESSION without MECHANICAL ENGINES.

BECAUSE it is as truly as commonly said, that *contraria juxta se posita magis elucescunt*, and because what I am now going to interpose is little less than necessary to be premised, to clear the way to what follows, and to connect the past writing to that which is to ensue; it will not be improper to add something in this place touching the condensation and compression of the air.

AND here I cannot but a little wonder, that among so many that have had occasion to consider the nature of cold, and the condensation of the air by it, I have not yet met with any that have had the curiosity to measure that condensation; wherefore I long since attempted to do it, as I have related in another discourse; but not having that by me at present, and remembering in general that I did it in winter, when it may be objected that the air, being already præ-affected with the coldness of the season, was not capable of being so considerably contracted by an additional cold, as it would be at a time of year when it is wont to be in a state of greater laxity; I thought fit to make the experiment about the beginning of Autumn, without tying myself to make it with the same circumstances that I had done before: the event of this trial I find registered as follows.

AFTER the midst of *September*, on a sun-shiny day, and about noon (which circumstances we made choice of, that the air might be the more rare and expanded) we took a bolt-head or round phial furnished with a long stem, and placed in a frame purposely provided, so that the stem was perpendicular to the horizon, and the globulous part was supported by such a vessel, that thorough a hole, purposely made at its middle, the shank reached downwards till the orifice of it was a little immersed beneath the surface of a glass full of water, that was placed at the bottom of the frame. This done, we took a good proportion of ice, and having beaten it in a mortar, and mixed with it a due quantity of bay-salt, we not only laid it round about the lower part of the ball, but the vessel contiguous to that part, being purposely made with turned-up brims, we were enabled to heap up the frigorific mixture, so as to bury the whole globulous part of the

glafs in it, and cover the very top of it therewith to a considerable thicknefs; upon which occafion, the air within being exceedingly refrigerated, the water, into which the fhank terminated, was made to afcend fomewhat falt along the cavity of the fhank, till we perceived it would reach no higher, but after a while began to fubfide again; which nick of time being carefully watched, we made a mark at the higheft ftation of the water, and then taking out the bolt-head we filled it with water, making allowance for that fmall part of the ftem, which was immerfed at the beginning of the operation. This water we weighed, and found it amount to nineteen ounces and fix drachms, then weighing as much water, as fufficed to fill the fhank up to the mark newly mentioned, we found that to be one ounce and three drachms, by which number the former being divided, the quotient was fourteen drachms four elevenths, fo that the proportion of the two quantities of water being as eleven to one hundred fifty-eight, the fpace, into which the air was condensed by refrigeration, was, to the fpace it poffeffed in its former ftate of laxity, as one hundred forty-feven to one hundred fifty-eight, and confequently the greateft condensation that fuch a time of the year and in fuch weather, fo high a refrigeration could bring the air to, made it lofe but  $\frac{11}{158}$  of its former extent.

N.B. FIRST, the ftem of the glafs ought to be of a considerable length, left by the great contraction made of the air in the ball by its high refrigeration, the water fhould afcend into the cavity of the ball itfelf, and thereby become exceeding difficult to be meafured.

SECONDLY, if one would be nice, one may take notice, that the height to which the water afcended in the ftem was about two feet; which cylinder of water, by its weight or tendency downward, might fomewhat hinder the liquor from afcending quite fo high as it would, and confequently keep the condensation of the air from appearing fully fo great as it was, but fo light a cylinder as that of the fufpended water would fcarce be very confiderable.

THIRDLY, when the water was afcended near as high in the fhank as it would rife, there was obferved in it an odd kind of fubfultus, or rifing and falling alternately, almoft like the mercury in the Torricellian experiment, before the mercury comes to fettle after its firft fubfidence. [But the confideration of this phænomenon belongs not to this place; for which reafon I infift not on this, and forbear mentioning fome others.]

FOURTHLY, that though it appears not by this experiment, whether the cold thus produced is equal to that of frofty weather in winter, and confequently capable of contracting the air as much as that feafon is wont to do; yet by preceding trials made with fit instruments, I had found, that by fuch an application of ice and falt as we had made in the late experiment, a greater degree of cold, and that in a warmer feafon, might be produced than had been found neceffary to make frofty weather in winter. The way of experimenting, for brevity fake, I omit, but if you please you may command it.

BUT it is not chiefly to acquaint you with the condensation that nature ufes to make of air, that I have been entertaining you with thefe memorials; for that which makes it very pertinent to my prefent purpofe, is, that it will fhew you, that as to the condensation or compreffion of air that I am to recite, though cold were employed about it, yet it was not really produced by cold, which could not contract the air to fo much as half that degree you will find it was reduced to by our operation, prefently to be mentioned; wherein the frigorifick mixture did not primarily or immediately comprefs the included air, but only fo affected the water that was fhut up within the fame vefel, as to make it fwell, and confequently crowd the aerial particles into lefs room; wherefore it now remains that we proceed to the experiment itfelf, a fhort account of which be pleafed to take in the enfuing tranfcript.



[To convince some strangers, we took a new glass bolt-head, with a neck not long, and filled it so far with common water, that being hermetically sealed, the liquor reached within three inches of the top, as near as we could guess by measuring it with a ruler, and making an estimate of the sharp end, made so for the conveniency of sealing up the glass, which sharp end we guessed to be about a quarter of an inch in length, then applying snow and salt to the lower part of the bolt head, we readily drove out the water further and further into the neck, till at length it was got up to the basis of the sharp and conical end, where the glass was sealed, and then just as I was looking upon it, the glass flew with a noise about my ears, being broke into many pieces, which argued the compression of the air to have been very great. And Doctor Wallis, who was present, and measured it from time to time, desired me to register the experiment, with his estimate of the compression, which was, that the air was reduced into the fortieth part of its former extension.]

I know so great a condensation of air will seem strange to those that have taken notice, that some of the best mathematicians of our age that have made use of wind-guns, and other forcible engines to crowd the air into as narrow room as possibly they could, confess themselves not to have been able, with all their strength and industry, to force the air into less than the fifteenth part of its usual extent; and besides, that this was done in countries, where the air may \* well be supposed more lax and rare than in England. I confess I saw no trials made with wind-guns, that convinced me that the condensation was so great as that newly specified (about which *Merfennus* himself somewhat hesitates, seeming to doubt, whether the air were indeed restrained into a fifteenth, or but into one eighth part of its former room). And he that hath observed and considered, as I have done, that in wind-fountains, as they call them, of glass, the air will seem to be notably compressed, whilst, indeed, we could not find it compressed into much less than its third part, will be the less unapt to be diffident of the great things that are said of the compression of the air; but because experience has informed us, that our English air may in peculiar instruments be forcibly crowded into a tenth, twelfth, or perhaps a fifteenth part of its former extent, I am content to take it for granted, what is related about the compression of the air, into the fifteenth part of its usual dimensions; and yet our experiment will be a considerabler instance of the great compressibility, if I may so speak of the air; for, according to the estimate delivered in the foregoing narrative, our compression, which was without mechanical instruments or engines, reduced the air into the fortieth part of the space it had lately possessed; and how great a force is requisite, when the air is once considerably condensed, to surmount, though but a little, its great resistance to further condensation, may be gathered from the observations about the gradual renitency of the air to compression, which we many years since made with mercury, and afterwards published in another treatise; but though upon the recited grounds that great compression of the air produced by our experiment may, as I was saying, seem very strange, yet it would not seem incredible, if I should here borrow those experiments and observations from my already published history, and some unpublished papers about cold, that would countenance what I have been delivering, and especially if I should stay to communicate to you the way I not unsuccessfully made use of, to estimate by weight the great force of the expansion of water upon its freezing. But since an account of this contrivance is not here necessary, and would require more leisure than I can spare at this time, it remains only, that by way of corollaries from what has been hitherto delivered in this and the two precedent writings, we rather point at than discourse of some observations that it suggested to us in the ensuing paper.

\* See *Merfen. Phæn. Pneum. Prop. 32.*

OF THE ADMIRABLY  
DIFFERING EXTENSION  
OF THE  
SAME QUANTITY OF AIR,  
RAREFIED and COMPRESSED.

**H**AVING already declared, that what I pretend to in the close is but to set down some observations that result from or are suggested by what hath been already delivered, I presume I need not trouble you or myself, with any other preface to what follows:

**T**HAT then, which seems first worth taking notice of, is the differing alterations that the air is subjected to by cold and heat; for whereas we could not find in this our climate, that cold would reduce the air into near the twentieth part of its former extension by condensation, heat would advance it to near seventy times its usual laxity by rarefaction.

**N**EXT, we may observe, that by engines and other artificial instruments, the air may be two or three times as much compressed as nature is wont to condense it by cold, even in frosty weather; and so on the other side, the air may by the intervention of art and instruments be much more rarefied and expanded, than it has been yet found to be by the bare application of external heat, though it were that of an intense fire itself.

**F**URTHERMORE, it may seem worth while to observe how much the utmost degree of rarefaction by heat, that experiment hath shewn us of the air falls short of the degree of expansion to which it has been advanced in our pneumatical engine, the proportion betwixt these two expansions being that of one to seventy or thereabout.

**B**UT, perhaps, it will not be necessary to conclude that the air is so much more rarefiable than compressible, as most readers will be prone to infer, by comparing the greatest compression and expansion of it that are mentioned in these experiments; since, if I mistake not, it ought to be considered that the air we made our trials with, upon the surface of the earth, was not (no more than is the air we commonly breathe) properly in a true natural consistence, as they speak; or, if you please, in a free and indifferent state, in reference to rarefaction and condensation, but was already highly compressed by the weight of the atmospherical pillar that leaned upon it, so that it had already a very strong renitency to further compression; whereas the air that was to be rarefied, had, by virtue of its spring (strongly bent by the weight of the incumbent air) a strong propensity or tendency to dilate itself; which difference I must content myself to have intimated, and leave you to consider whether and how much it may alter the case.

**F**OURTHLY, to some perhaps it will seem more fit to consider than easy to resolve, how, since the corpuscles of the air are acknowledged to be heavy, and those that remain must

See as to  
the utmost  
expansion  
by heat  
Mersen. in  
Cog. Phy.  
Mathem.



must be so wonderful thinly dispersed in the cavity of the receiver, they come to be supported and kept, as it were, swimming therein, and do not appear to subside by their own weight, the *Materia subtilis* (though the presence of that should be admitted) not appearing to have gravity wherewith to sustain them; and the vacuum (if that be supposed wherever the aerial particles are not) being too near a-kin to nothing to be able to oppose their descent; but though something may be suggested about the solution of this doubt, my haste obliges me to leave it as such.

FIFTHLY, I will not make it my business to make mention in this place of the wonder that may be justly excited in those, that when they look on one of our well exhausted receivers, attentively consider how small a proportion the common aerial corpuscles, which are very sparingly dispersed there, bear to the whole cavity of the vessel, which, before it was exhausted, was thought to be replenished with air alone. This, I say, I shall not solicitously observe, because I think I need not; for I little doubt the thing will be observed and laid hold of, both by the *Cartesians* and *Epicureans*, the former of which will endeavour thereby to establish the necessity of their *Materia subtilis*, to maintain the plenitude of the world, and the circle they attribute to moving bodies; and the latter will here triumphantly pretend to have a more illustrious instance than ever of their *vacuum coarctatum* within the world, since there is an impenetrable vessel out of which it is manifest, that an almost incredible proportion of aerial substance hath been manifestly made to issue; whereas it is no ways manifest to any of our senses, that any other body has got in to succeed in its room; wherefore leaving them to debate what it is that is contained in that far greatest part of the vessel, that the air pumped out of our receiver has deserted, I take notice,

SIXTHLY, that to conclude with what was the main drift of this and the foregoing papers, we are here invited to observe, with wonder, the stupendous mutability of the air, as to rarity and density, whereby the same quantity of air, being sometimes compressed, sometimes dilated, may change its dimensions to a degree that seems almost to transcend the power of nature and art, and by consequence might probably be rejected as incredible, if it were abruptly and nakedly proposed; and therefore it will be convenient to do, though very briefly, these two things.

FIRST, to consider, what we have upon experience delivered in our defence against the learned *Linus*, touching the condensation and rarefaction of the air, as it is exposed to a greater or smaller pressure, without the intervention of either external heat or elaborate engines. For from these experiments (that may be found in the lately mentioned defence \*) eminent mathematicians have inferred, that one can scarce safely put determinate limits to the stupendous rarity, which the upper part of the atmosphere, being almost totally uncompressed by incumbent particles of air, may be supposed to have by nature un-assisted by art.

AND this is the first of the two things I above desired to have taken notice of. But the other (which, though it be but the second, is much the more considerable) is to confer together the smallest extent; to which we have reduced it by condensation, and the greatest, to which we have advanced it by rarefaction, after having taken notice, that according to the least estimate of any recited in the foregoing experiments, the extension of the same air, is as 1 to 2744, or thereabouts; and if, instead of the moderate, we take the greatest expansion of the air, being (leaving out the odd hundreds to make the rounder number) as 13000 to 1, when the uncompressed air was highly rarefied, that

\* Chap. V. Whose Title is, Two New Experiments touching the Measure of the Force of the Spring of the Air compressed and dilated.

number being multiplied by 40, because of the fore-mentioned compression of the air, will amount to 520000, for the number of times, by which the air at one time exceeds the same portion of air at another time; which is a difference of expansion so great, that I hope it will keep you from thinking the title of the foregoing epistle, where the expansion of the air is called admirable, immodest, especially since I have forbore to mention what probable arguments might be offered to prove it at least possible that the industry of men, and perhaps our own, may find means to make both the condensation and rarefaction of the air to exceed the uttermost whereto we have yet been able to bring them.

## P O S T S C R I P T.

*Touching an Observation to be inserted above, (Page 503) immediately after the Mark \*.*

SINCE the writing of this, the author chanced to find one of the lately mentioned instruments of a considerable bigness, which was presumed to have miscarried; and comparing it with a memorial made, when it was first completed, to keep in memory the heights, dimensions, &c. of the inclosed mercury and air; we found, that in about ten weeks, there was not any considerable variation of them; and the little shrinking of the air, which was discoverable by an attentive eye, was not such but that it might be probably ascribed to the change of the weather to a far greater coldness, which might be supposed a little (and it did but very little) to weaken the spring of the included air, and consequently abate of its full resistance to the pressure of the mercury in the longer leg of the siphon.



A N

## O B S E R V A T I O N

O F A

## S P O T I N T H E S U N .

First Printed in the *Philosophical Transactions*, N<sup>o</sup> 74, p. 2216,  
for April the 27th, 1671.

Friday, April 27, 1660.

“ ABOUT eight of the clock in the morning there appeared a spot in the lower  
 “ limb of the sun, a little towards the south of its æquator, which was entered  
 “ about  $\frac{1}{40}$  of the diameter of the sun, itself being about  $\frac{1}{160}$ , in its shortest diameter,  
 “ of that of the sun; its longest about  $\frac{1}{40}$  of the same. It disappeared upon Wednesday  
 “ morning, May 9th, though we saw it the day before, about ten in the morning, to  
 “ be near about the same distance from the westward limb a little south of its æquator,  
 “ that it first appeared to be from the eastward limb, a little south also of its æquator.  
 “ It seemed to move faster in the middle of the sun than towards the limb. It  
 “ was a very dark spot almost of a quadrangular form, and was inclosed round  
 “ with a kind of dusky cloud, much in this form, and in this proportion to  
 “ the spot.



“ WE first observed this very same spot, both for figure, colour, and bulk, to be  
 “ re-entered the sun, May 25th, when it seemed to be in a part of the same line it had  
 “ formerly traced; and was entered about  $\frac{4}{33}$  of its diameter about seven of the clock in  
 “ the afternoon. At the same time there appeared another spot, which was just entered,  
 “ and appeared to be entered not above  $\frac{1}{132}$  part of the sun's diameter. It appeared to  
 “ be longest towards the north and south, and shortest towards the east and west.  
 “ There seemed to be dispersed about it divers small clouds here and there.”

[THESE observations were made, as the noble observer told us, with an excellent telescope, in the presence of divers curious and ingenious persons, one of whom was Mr. Hook. And discoursing of thoughts he had entertained touching the effects of such spots, he suggested this inquiry, whether they might not cause a considerable alteration both in the body of the sun itself, and in our air, and the bodies in it, upon their dissipation?] ]

A N

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A N  
E S S A Y  
ABOUT THE  
O R I G I N   A N D   V I R T U E S  
O F  
G E M S.

Wherein are propofed and hiftorically illuftrated fome Conjectures  
about the Confiftence of the Matter of PRECIOUS STONES, and  
the Subjects wherein their chiefeft Virtue refides.

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The P U B L I S H E R   t o   t h e   R E A D E R.

**T**HE philofophy and origin of gems, as well as their ufeulnefs and virtues, will, I am perfuaded, be found, upon the attentive perufal of this effay itfelf, fo rationally and warily delivered therein, that there will need nothing to be faid in the praife of the compofure thereof. I dare venture, notwithstanding the noble author's modefty, to prefent it to the moft critical tafte without hanging out a bufh to it.

ALL I have to fay in the publishing thereof fhall be the fame that was alledged by the Englifh interpreter of the learned *Steno's* Prodrumus to an intended difertation of his concerning folids naturally contained within folids, printed the laft year by *Mofes Pitt* in *Little Britain*; where, in the Englifh preface, occur paffages to this effect, *viz.*

“ THAT



“ THAT the honourable author of this essay, before he would see or hear any thing of that Prodrômus of *Steno*, did, upon occasion, solemnly declare to the author of that English version (who there protests that he speaks it *bona fide*) the sum and substance of what is deduced at large in this tract; the manuscript whereof the said interpreter then saw, and received it into his custody for publication; which sum was this; first, that the generality of transparent gems have been once liquid substances, and many of them, whilst they were either fluid, or at least soft, have been imbued with mineral tinctures that con-coagulated with them; whence he conceives, that divers of the real qualities and virtues of gems may be probably derived.

“ SECONDLY, as for the opacous gems and other medical stones, as blood-stones, jaspers, magnets, emery, &c. he esteems them to have, for the most part, been earth (perhaps in some cases very much diluted and soft) impregnated with the more copious proportion of fine metalline or other mineral juices or particles, all which were afterwards reduced into the form of stone by the supervenience (or the exalted action) of some already inexistant petrescent liquor, or petrific spirit, which he supposeth may sometimes ascend in the form of steams; from whence may be probably deduced, not only divers of the medical virtues of such stones, but some of their other qualities, as colour, weight, &c. and also explained how it may happen, what he hath (which he doubts not but others have done also) observed of stones of another kind, or marchasites, or even vegetable and animal substances that have been found inclosed in solid stones; forasmuch as these substances may easily be conceived to have been lodged in the earth, whilst it was but mineral earth or mud, and afterwards to have been, as it were, cased up by the supervenient petrific agents that pervaded it.

“ NOR are these petrescent liquors the only ones, to which he supposes that many fossils may owe their origin, since he thinks there may be both metatescent and minerallescent juices in the bowels of the earth, and that sometimes they may there exist and operate under the same spirits and steams.”

So far the preface to that translation; which is here repeated to do right to this noble author, in the matter of the theory relating to the origin both of precious and other stones. Which done, I shall keep the curious reader no longer from the contentment which he will doubtless find in the perusal of this essay.

## The P R E F A C E.

THAT the scarcity, the lustre, and preciousness of gems have made them in all ages to be reckoned among the finest and choicest of nature's productions, is generally granted. But whether the books that have been divulged of them be answerable to the nobleness of the subject, seems not to me so unquestionable. For, as for the origin of gems; to say with *Aristotle*, towards the close of his third book of *Meteors*, that a dry exhalation, ἐνὰ ἀναδυσίαις (whether) fiery or firing (ἐκπυρρῶσα) makes, among other fossils, the several kinds of unfusible stones; or to tell us, according to the more received doctrine, that gems are made of earth and water finely incorporated and hardened by cold; this, I say, is to put us off with too-remote and indefinite generalities, and to found an explication upon principles, which are partly precarious, and partly insufficient, and perhaps also untrue. And as to the history of gems; that has been so fabulously delivered, that, especially among the moderns, many learned men, philosophers and physicians, have, for the sake of so many improbable, and sometimes impossible virtues that have been ascribed to gems, been induced to deny them any virtues at all. It is true, that I am not altogether so severe, and that the esteem that I find made by learned men of the inquisitive Emperor *Rudolfus's* physician *Boetius De Boot*, makes me discriminate him and two or three modern authors, that in books professedly made on other subjects, have written incidentally of some gems, from such notoriously fabulous writers as *Mizaldus*, *Albertus Magnus* (if his name be not injured by the imputation of a spurious book) *Baptista Porta*, *Kirannides* (and some others that I forbear to name) from whose learning one would expect more wariness and judgment. But though, for reasons elsewhere mentioned, I do not unreservedly think that precious stones, especially opacous ones, can have no medical virtues at all; yet when I considered how difficult it was to assign any thing that is possible and intelligible (which I do not take a substantial form to be) whence their virtues may probably be derived, without giving some such account of the origin of gems themselves, as was not to be expected from the followers of the peripatetick, that is, the received philosophy; I could not but wish that something were attempted on that subject according to the principles of the Corpuscularian.

THESE things made me the less backward to comply with the curiosity of my friends, which put me upon the following discourse, wherein I was content to try, what, without ransacking the authors that had professedly written *de Gemmis*, the consideration of the subject to be treated of, my natural propensity to take notice of nature's productions, and the trial whereto these considerations and observations lead me, would suggest to my pen.

WHETHER my conjectures and ratiocinations be as new to others as to those I chiefly wrote for, it is not my part to determine; only I designed to suit my discourse to the phenomena of nature, without being solicitous with whom I disagreed or complied. And therefore, though it should happen that some conjectures of mine should, unknown to me, be coincident with the opinion of some classic writer about gems; yet, I presume, the whole subsequent hypothesis, and the arguments it is founded upon, will appear to have been suggested to me by the nature of the thing itself, and my way of considering it; not to mention, that sometimes one may meet with a good particular conjecture in an author that understands not the importance of it himself, and knows not

how



how to make use of it, but builds it on some such fabulous relation or erroneous principle, as is apt to discredit it with wary readers, unless they be such to whom its compliance with the opinions they have on better grounds already entertained, happen to recommend it. I know it may be thought strange that I have been so very sparing in the citation of those authors that have writ whole books about gems; but I have this to say for myself, that I had neither them, nor so much as my own papers about the origin of minerals at hand, when I writ the following essay. Which I was the less troubled at, upon two distinct accounts; the first, because I remembered that several passages that I had met with about the virtues of gems, cited out of divers of those authors, were such, as I should have much scrupled to vouch; some of them being such as I knew to be false; others that I shrewdly suspected not to be true; and others that appeared to me altogether incredible; and the second, because, to forbear transcribing what my friends might probably have met with in authors already, would best comply, both with their desires, which was to know my particular thoughts; and with my design, which was partly to see how far I could make out those thoughts, by my own arguments and observations, assisted only by some very few historical passages that I lighted on in writers not classic; and partly, to take this occasion to prosecute divers matters of fact relating to the subject I was treating of, which probably would otherwise have been quite lost. And I doubted not, but if this first draught of my conceptions were by my friends thought worthy of being enlarged, it would not be difficult for me, when I should come at my books and papers again, to enrich this tract with many histories borrowed from famous writers; if that should be thought necessary by persons that were possibly less diffident of me than of them. In short, I proposed this discourse but as a conjectural hypothesis, wherein I attempted to derive the origin of gems, and one of the main causes (I do not say the only cause) of their qualities and virtues, from principles less remote and more intelligible than those of the peripateticks; and having delivered divers observations and experiments of my own about the phenomena of gems, to explicate some of them by intelligible principles, and illustrate others by resembling things, that may be really observed in nature, or easily performed by art. Which way of handling my subject permitted me to hope, that, whether or no I should be thought a lucky conjecturer about the subject I attempted, I should, at least, in some measure, prove a benefactor to what is, perhaps, preferable even to lucky conjectures themselves, the natural and experimental history of such noble subjects as gems.

A N

E S S A Y

ABOUT THE

ORIGIN AND VIRTUES

O F

G E M S.

## S E C T I O N I.

**T**HOUGH it will not perchance prove very difficult to propose to you my conjecture about the causes of the virtues of precious stones; yet I fear it will not be easy for me to acquaint you fully with the grounds of it. For unless I should transcribe for you my whole discourse of the origin of minerals in general (of which you know stones make a part) I cannot well lay before you all the considerations by which I have been induced to take up the conjecture or hypothesis I am about to propound; and consequently, I cannot well comply with your curiosity about gems, without either omitting several things which might much countenance the following discourse, or proposing (without amply proving them) some things, that I confess seem not clear, nor some of them so much as probable, by their own light. But since you will have it so; I will, rather than disobey you, present you in one discourse several things concerning gems, whereof some belong to others of my little tracts about the origin of minerals from fluid, or at least soft bodies; though some indeed were more directly written concerning gems; notwithstanding that they were delivered not as an entire tract about that subject, but as corollaries that might be drawn from, and applications that might be made of, what had been in a more general way discoursed about the origination of stones and other minerals. And therefore presuming that you will suppose with me in this discourse some few particulars that, I think, I have elsewhere made probable, and might perhaps



perhaps do so from some of the phænomena mentioned in this writing itself, I would immediately address myself to the subject of it, if I did not think a previous admonition very requisite.

For, I must, at the very entrance of this discourse, desire you to take notice, that when I propose my conjectures about the virtues of gems, I do not suppose the truth of all, or so much as the tenth part of those wonderful properties that men have been pleased to ascribe to them. For not only some of the writers of natural magick, but men of note, who should be more cautious and sober, have delivered in their writings many things concerning gems, which are so unfit to be credited, and some of them perhaps so impossible to be true, that I hope the believers of them will, among the votaries to philosophy, be as great rarities, as gems themselves are among stones. And those that can admit such unlikely fables will be as much despised by the judicious as jewels can be prized by the rich.

For my part I never saw any great feats performed by those hard and costly stones (as diamonds, rubies, sapphires) that are wont to be worn in rings. But yet, because physicians have for so many ages thought fit to receive the fragments of precious stones into some of their most celebrated cordial compositions; because also divers eminent men of that profession, some of them famous writers, and some virtuosi of my own acquaintance have, by their writings, or by word of mouth, informed me of very considerable effects of some gems (especially crystal) upon their own particular observations; and lastly, because, that (as I shall shew anon) I find no impossibility, that at least some costly, and less hard (though indeed more valuable) gems, may have considerable operations upon human bodies, some few of which I have had opportunity to be convinced of; I will not indiscriminately reject all the medicinal virtues that tradition and the writers about precious stones have ascribed to those noble minerals; contenting myself to declare in a word, that suspecting most of them to be fabulous, my conjectures aim only at giving one of the causes of those virtues ascribed to gems, which experience warrants to be real and true.

HAVING thus explained in what sense my conjecture about the virtues of precious stones is to be understood; it follows, that I propose the conjecture or hypothesis itself; the substance of which may be comprized in these two particulars; first, that many of these gems and medical stones either were once fluid bodies, as the transparent ones; or in part made up of such substances as were once fluid; and secondly, that many of the real virtues of such stones may be probably derived from the mixture of metalline and other mineral substances, which (though unsuspectedly) are usually incorporated with them; and the greatness of the variety and efficacy of those virtues may be attributed to some happy concurrent circumstances of that commixture. The first of these heads relates properly to the origin of gems. The second, partly to that, and partly to the kinds and degrees of their virtues.

BUT that any gems, especially the hardest sorts of them, should have a later beginning than that of the earth itself, will probably be thought to relish of a paradox; and I doubt not, it will pass with many for a great one, that some of these hardest of solid bodies should have been once fluid ones or liquors; wherefore I shall endeavour to countenance this hypothesis by the following considerations.

1. AND first the diaphaneity of diamonds, rubies, sapphires, and many other gems, agrees very well with this conjecture, and thereby seems to favour it. For it is not so likely that bodies that were never fluid should have that arrangement of their constituent parts, that is requisite to transparency, as those that were once in a liquid form, during which it was easy for the beams of light to make themselves passages every way, and dis-

pose

pose the solid corpuscles after the manner requisite to the constitution of a transparent body. Therefore we see that silver in aqua fortis, or lead in spirit of vinegar, having by that solution had their particles reduced into a fluid form, those particles, though before opacous, are so disposed of as to make, not only a diaphanous solution, but, if one pleases, transparent crystals. And what chymists usually try with those metals, I have had the curiosity to try with several stones, which I may hereafter have occasion to name to you. But this argument I bring rather to confirm than evince my conjecture.

SECONDLY, the origin assigned to gems may be also countenanced by the external figuration of divers of them. For we plainly see that the corpuscles of nitre, allum, vitriol, and even common salt, being suffered to coagulate in the liquors they swim in before, will convene into crystals of curious and determinate shapes. And the like I have tried in several metalline bodies dissolved in several menstruums. But unless a concreting stone or other like body be either surrounded with, or in good part contiguous to a fluid, it is not easy to conceive how it should acquire a curious angular and determinate shape. For concretescent bodies, as I may so speak, if they have not room enough in an ambient fluid for the most congruous ranging of their parts, cannot cast themselves into fine and regular shapes, such as I shall presently show, that divers gems seem to affect; but the matter they consist of must conform to the figures of the cavity that contains it, and which in this case has not so much the nature of a womb as of a mold. And so we see, that saltpetre, and divers other salts, if the water they were dissolved in be much too far boiled away before they are suffered to shoot, will, if the liquor fill the glass, sometimes coagulate into a mass fashioned like the inside of the containing vessel, or if a pretty quantity of liquor remains after the coagulation, that part of the nitrous mass that was reduced to be concreted next the glass, will have the shape of the internal surface of it, whatever that be; but those crystals that are contiguous to the remaining liquor, having a fluid ambient to shoot in, will have those parts of their bodies that are contiguous to the liquor curiously formed into such prismatical shapes as are proper to nitre.

To apply this now to gems; that divers kinds of them have geometrical and determinate shapes, though it be not vulgarly observed, because we are wont to see them when they are cut, if not also set in rings and jewels; yet I have often had the opportunity to take notice of it, by having had the curiosity to look upon many of them rough as nature has produced them, and the good fortune to take divers of them out of their wombs. For I remember I have taken a good number of Indian granates out of a lump of heterogeneous matter, whose distinct cavities, like so many cells, contained stones, on some of whose surfaces you might see triangles, parallelograms, &c. And being once near the rock whence those stones are chiefly fetched, that are commonly called Bristol-stones, I remember I rid thither and procured a workman or two to dig me up a number of them, divers of which I found to be curiously and determinately shaped, much like some crystals of nitre that I have taken pleasure to compare with them. And the like figuration I have also observed in divers Cornish diamonds, and in a fair and large one, which one, that knew not what it was, found growing, with many lesser, in *Ireland*, and presented me. And to let you see that it is not only in these softer gems that this curious figuration is to be met with, I shall add, that I found among many stones I had, and took to be rubies (and those the jewellers will tell you are exceeding hard) a considerable number, whose shapes, though not the same with those of the Cornish and Irish stones, were yet fine and geometrical. And the like I have observed even in those hardest of bodies, diamonds themselves; of which remembering, that in my collection of minerals, I had a pretty large one that was rough, I perceived that the surface



face of it consisted of several triangular planes which were not exactly flat, but had, as it were, smaller triangles within them, that for the most part met at a point, and did seem to constitute, as it were, a very obtuse solid angle; encouraged by this, I examined several other rough diamonds, and found the most of them to have angular and determinate shapes, not unlike that newly mentioned. And having thereupon consulted an expert jeweller that was also a traveller, though he could not name to me the shapes of the uncut diamonds he had met with; yet he told me he generally found them to be shaped like that I shewed him; inasmuch, that such a shape was a mark, by which he usually judged a stone to be a right diamond, if he had not the opportunity to examine it by the hardness.

AND this I shall add in favour of the comparison I lately intimated betwixt the coagulation of petre and that of gems, that having once made an odd menstruum, wherein I was able to dissolve some precious stones, there shot in the liquor crystals pretty large, and so transparent and well-shaped, that they might well have passed for crystals of nitre; and yet, if I much misremember not, they were insipid. And I have divers times taken notice in such stones, as the Bristol diamonds, that though that part which may be looked upon as the upper part of the stone, were curiously shaped, having six smooth sides, which at the top were, as it were, cut off sloping, so as to make six triangles, that terminated like those of a pyramid in a vertex; yet that which may be looked upon as the root or lower part of the stone, was much less transparent (if not opacous) and devoid of any figuration; of which the reason seems to be, that this being the part whereby the stone adhered to its womb, it was sullied by the muddiness of it, and reduced to conform itself to whatever shape the contiguous part of the cavity chanced to be of; whereas the upper part of the stone was not only formed of the clearer part of the lapidescent juice before the waterish vehicle was exhaled, but had room and opportunity to shoot into the curious figure belonging to its nature. And this is much more conspicuous where many of these crystals grow, as it were, in clusters out of one mineral cake or lump; as I have seen not only in those soft but yet transparent concretions, which some of the later mineralists (for the ancients seem scarce to have known them) call fluores, and particularly in a very fine mineral lump, that I had once the honour to have shewed me by a great prince, and no less great a virtuoso, to whom it was then newly presented. For this mass consisted of two flat parallel cakes, that seem composed of a dirty kind of crystalline substance, and out of each cake there grew, towards the other, a great number of stones, some of which, by their cohesion, kept the two cakes together; and most of these stones, having each of them a little void space about it, wherein it had room to shoot regularly, were geometrically shaped, and, which looked very prettily, were coloured like a German amethyst. And I have myself a pretty large stone taken up here in *England* by a gentleman of my acquaintance, which consists, as it were, of four parts; the lowermost is a thin and broad flake of coarse stone, only adorned here and there with very minute glittering particles, as if they were (as probably they may be) of a metalline nature; over this is spread another thin white, but opacous bed, which is so inclosed between the first named bed and the two others, that, without defacing the stone, I cannot well examine it: the third consists of a congeries of minute crystals exceedingly thick set, which therefore look whitish, having little or no tincture of their own; and this part, no more than either of the former, is not much thicker than a barley-corn. The fourth and uppermost part, which yet seems in great part to be the same crystals, which, as they grow higher and spread, acquire a deeper colour, is made up of a great number of amethysts, some paler, and some highly tinted, which are of very differing figures and bignesses, according (as one may guess) as they had conveniency to shoot; these at one end of the stone

stone lying in a flat bed, as it were, and scarce exceeding a barley-corn in length; whereas those at the other end shoot up to a good height into figured crystals, some of them as big as the top of my little finger, and those are the most deeply coloured, being also of a good hardness, since I found that they would easily grave lines upon glass.

I REMEMBER also, that going to visit a famous quarry that was not very far from a spring, which had somewhat of a petrescent faculty in it, I caused divers solid pieces of rough and opacous stones to be broken, out of hope I had to find in them some finer juice coagulated into some finer substances; and accordingly I found, that in divers places, the solid and massy stone had cavities in it, within which, all about the sides, there grew concretions, which, by being transparent, like crystals, and very curiously shaped, seemed to have been some finer lapidescent juice, that by a kind of percolation through the substance that grosser stone was made of, had at length arrived at those cavities, and upon the evaporation of the superfluous and aqueous parts, or by their being soaked up by the neighbouring stone, had opportunity to shoot into these fine crystals, which were so numerous, as quite to overlay the sides of the cavities, as I can shew you in some large clusters of them that I brought from thence. And enquiring of an ancient digger, whether he had not sometimes met with greater quantity of them? he told me that he had, and presented me a great lump or mass made up of a numerous congeries of soft crystals (but nothing so colourless as these other newly mentioned) sticking to one another, but not any of them to any part of the rock; so that they seemed to have been hastily coagulated in some cleft or cavity, as it were, in a mold, where meeting and mingling before concretion with some loose particles of clay, the mass may thereby be discoloured.

OUR argument, drawn from the figuration of transparent stones, may be much strengthened by the coalition I have sometimes observed of two or more of such stones, and the congruity in the shape of some of them to the figures of those parts of the others that were contiguous to them, and seemed to have been formed after them. But though this phenomenon be considerable to the scope of my discourse, yet perceiving that I shall have occasion to insist on it hereafter, I shall not do it now.

THIRDLY, nor is it only the external figuration of these gems, but the internal texture that favours our hypothesis, some of them seeming much to imitate in their coagulation several of those substances, which I have observed to have once been fluid. That common salt may be made up of small saline particles, that by a convenient juxtaposition may be associated into great lumps, divers of which are cubically shaped, is an observation easy enough to be made. And that such coalitions of particles may constitute solid and considerably hard bodies, I have tried by breaking some of the larger cubes of sal gem, and the lumps of the isle of Mayo salt, whereof the first is fossil, the other marine, and both natural. I have likewise found by trial, that, though silver dissolved in aqua fortis appears usually to shoot, if it be taken notice of, into flat and exceeding thin flakes; yet it is very possible so to order the coagulation, that many of these thin plates shall, in their convention, have their flat sides so placed over one another, as to make up pretty large and thick crystals, whose very outsidings will be finely shaped, as being some peculiar kind of vitriol. Nor are these the only fluid bodies, which I have reduced to coagulate into conventions, of such a flaky texture; wherefore I began to suspect that divers transparent minerals may have the like; and in some diaphanous kinds of talk, whose outsidings were mathematically figured, I found encouragement to try, whether even some gems themselves, notwithstanding their hardness, might not have such an internal figuration. Nor was I deterred by considering that it is taken for granted, that gems are of an uniform texture, and that there must be a strange thinness in the plates, that make up transparent stones, since no such thing has been noted by the most curious eye,

but



but men have taken it for granted, that the texture of all gems is uniform, without any grain or fibres, no more than there is in gold. But as to the thinness of the plates, I remember, I have several times taken pleasure to hold a piece of good Muscovia glass against the light, when it was of such a thinness, that the spectators, though provoked to look with curious eyes, could scarce see the plate itself, and would by no means be brought to think that it was possible to split it, till I did actually do it; and sometimes I then subdivided it beyond even my own expectation. But to examine this conjecture, I took some stones that had geometrical figures on part of their surfaces, and which I had other grounds to think to have been once fluid substances, and having diligently surveyed some of them which seemed likeliest to give me satisfaction, I manifestly enough perceived, not only with my assisted, but with my naked eyes, divers parallel commissures, which seemed plainly to be made by the contiguous edges of little thin plates of stone, that appeared to lie one over another, almost like the leaves of a book that is a little opened.

I REMEMBER, that holding a large and rough grizolette (as artificers call hard gems of a blueish colour, brought them from *East-India*) against the light, and curiously observing it, I have sometimes discerned a grain, as they call it, in the stone, and was answered by a skilful artist that used to make seals of them, that such stones would usually split, according to the ductus of their grain. I will not urge, that in some other precious stones that were cut and polished, as particularly the hyacinth, and even the sapphire, by obverting them several ways to the light, I have been able to observe, as it were, commissures, which were so fine as not to hinder, or call in question the intireness of the stone, for the lapidary's purpose. This, I say, I forbear insisting on, because the phenomenon is far less considerable than what I have several times observed in New-English granates, wherein, especially when they are broken, the edges and commissures of the thin plates or flakes whereof they consisted, were very easily discernible. And, to try whether this observation would hold even in the hardest stones, I had recourse to a pretty big diamond unwrought, which being placed in a microscope, shewed me the commissures of the flakes I looked for, whose edges were not so exactly disposed into a plane, but that some of them were sensibly extant like little ridges, but broad at the top above the level of the rest. And these parallel flakes, together with their commissures, I could in a somewhat large diamond plainly enough discern, even with my unassisted eyes. And for further satisfaction, I went to a couple of persons, whereof the one was an eminent jeweller, and the other an artificer, whose trade was to cut and polish diamonds, and they both assured me upon their repeated and constant experience, and as a known thing in their art, that it was almost impossible (though not to break, yet) to split diamonds, or cleave them smoothly cross the grain, if I may so speak, but not very difficult to do it at one stroke with a steeled tool, when once they had found out from what part of the stone, and towards what part the splitting instrument was to be impelled; by which it is evident, that diamonds themselves have a grain, or a flaky con-texture, not unlike the fissility, as the schools call it, in wood; which you will easily grant to consist of assimilated water or juices; which having been once fluid bodies, were fit to have their particles so ranged or disposed, as to constitute a body far more easy to be cleft according to the ductus of the fibres or planes, than otherwise. And I remember, that having, as I thought, observed in a rough diamond, which I purposely examined, that the flakes, whose edges were terminated in one plane, were far enough from being parallel to those, whose edges composed another plane (I speak of physical planes of the same stone) I imagined, that if this diamond were to be cleft, it would not be smoothly split into two pieces, because the commissures did probably make angles in

the body of the stone; and accordingly I learned of the ancientest of these diamond-cutters, that sometimes he met with stones that eluded all his skill, and would by no means be split like others into two parts, but, before they were cleft quite through, would break in pieces; which was a defect in the stone he could not certainly foresee, but was fain to learn from the unwelcome event.

FOURTHLY, it seems not unprobable, that the colours of divers gems (for I do not say of all) are adventitious, and were imparted to them, either by some coloured mineral juice, or some tinging mineral exhalation, whilst the gem or medical stone was either in *solutis principiis*, or of a texture open enough to be penetrable by mineral fumes. Which argument's considerableness makes me hold it unfit to be lightly touched in this place; though I cannot discourse any thing fully of it in few words, because it not only suggests divers observations and other particulars, but requires also the mention of some of the chief of them; which therefore I shall now subjoin.

I. AND the first shall be, that many gems, not to say almost all of them, have been observed to be deprived of their colour, if having fallen, or been put into the fire they have lain too long there; insomuch, that I have found it affirmed upon the testimony of the learned and experienced *Boetius de Boot*, that all gems will lose their colour in the fire, except Bohemian granates. How far this may be true, I have not had opportunity thoroughly to examine. But I well remember, that having purposely exposed divers gems to the fire, though that were but moderate, and had a crucible interposed between it and them, some of them seemed to have their tincture much impaired, and others quite destroyed. But I must be so free as to admonish you, that if these trials be not warily made, they may easily impose upon us; especially if we do not consider the nature and cause of whiteness. For any diaphanous body, as far as I have yet observed, being divided into a multitude of very minute parts, and consequently acquiring a multitude of distinct superficies, which do briskly reflect the light every way outwards, will appear to have a white colour that will be more or less vivid, as the particles are more or less numerous, minute, and otherwise fitted to scatter the incident beams of light; as you may see by reducing to powder fine Venice-glass, which will be white; and even red ink, if so shaken or beaten as to be brought to a froth, consisting of many minute bubbles, will seem to have put on a whiteness. So that if by too hasty an ignition, or too hasty a cooling of the fired gems, they come to be flawed with innumerable little cracks, they may be thought to be made white, by having their tincture driven away, when their whiteness really proceeds from the multitude of those little flaws which are singly unperceived; and the rather, because the body may still retain its former shape or seeming intireness. To illustrate which, I have sometimes taken pleasure to heat a piece of crystal red hot in a crucible, and then quench it in cold water; for even when the parts did not fly or fall asunder, but the body retained its former shape, the multitude of little cracks that were by this operation produced in it, made it quite lose its transparency, and appear a white body. In making which experiment, the multitude of produced flaws may be pretty well discovered to the incredulous, if, as I have sometimes done, the ignited crystal be warily and dexterously quenched, not in water, but in a very deep solution of cochineal made with spirit of wine, in which operation, if it be well performed, but not otherwise, enough of the red particles of the solution will get into the cracks of the crystal to give it a pleasing colour.

\*Tincture  
of coral.

THE other trials that I have made about the reducing of whiteness or paleness in bodies, either transparent, or even semi-diaphanous only, belonging to another paper, I shall here forbear to mention them, having already said enough for my present purpose, which is not so much to affirm positively, that no proof at all can be drawn from the operation



operation of fire upon the colour of gems, as to make you cautious, what proofs drawn from thence you admit.

2. WHEREFORE declining to say any thing more about the first, I shall now proceed to the next circumstance, that belongs to our argument (which you may think to be more considerable than the former) namely, that the colours of several gems, when they are not destroyed by fire, will be altered thereby; which being a thing that happens to divers fossil pigments (of which some I employ to tinge glals) and other bodies confessedly mineral, argues a commixture of mineral substances in those stones, whose colour receives some of the alterations I speak of: which last words I add, because I would not impose upon you, by concealing, that there may be a great change of colour produced by the fire, without any alteration of the tinging parts as such. For by flaving the heated gem in very many parts, a degree of whiteness or paleness emerging thereupon may somewhat change the former colour. But this alteration being but a kind of dilution is not that, which I here mean. For I remember, I have taken Indian granates, and having in a crucible exposed them to the fire, I found they had exchanged their reddish-colour for a dark and dirty one, like that of iron that has been long kept in the air. And having taken some pieces of agate prettily enough adorned with waves of differing colours, and kept them a competent time (for they should not be kept too long) in the fire, I found, as I conjectured, that the greatest part of the agate seemed to be deprived of its tincture, being reduced to a pleasant whiteness. But in some places, where there were stains of a different kind from the rest, and where there ran little veins, that I guess to be of a metalline nature, there, I say, the colour was not destroyed, but changed, and the veins of pigment, thus coloured, acquired a deep redness, which they will retain, if let alone; though I was induced to think by some trials made on other pieces of Indian agate, that even these metalline tinctures were not so fixed, but, that a lasting fire would drive them away, and leave the stones purely white. Such a change of colour, as I lately mentioned in the veins of agate, is likewise found in those of some other stones, as also in some pebbles, amongst divers of which, that lost only their transparency by ignition and extinction in water, one or two acquired so much deeper a colour than it had before, that I thought it remarkable.

3. ANOTHER circumstance, that seems to favour our conjecture, may be this, that it has been observed not unfrequently, that near many of the places, where coloured gems are found, some mines or veins of metals are to be met with. And I think it not unlikely, that if search were skilfully made, many more discoveries would be made of veins either of metalline ore, or some other mineral, liquid or concreted, whence, by way of juices or fumes, the gems may be presumed to have received tinctures. But usually, where precious stones are found, men's industry and curiosity is too much confined to those rich minerals, and does not make them solicitous to look after inferior ones. Besides that in *East India*, whose countries are best for the most gems, they are wonderfully unskilful at digging mines; as I have gathered from the answers of some, who purposely went to visit the diamond mines, as they call them. To this may be also referred, that gems are several times found in the metalline veins themselves, or very near them; as I can shew you divers amethysts, that an ingenious gentleman of my acquaintance took himself out of a piece of ground abounding with the ores of iron and tin, the latter of which was there plentifully dug up. And in those colder countries, such as *Germany* and *England*, where hard gems are more unfrequent, those soft ones, that mineralists call fluores, are often to be found in, or near metalline veins, so finely tinted by mineral juices, that, were it not for their softness, they might pass, at least amongst most men, for emeralds, rubies, sapphires, &c. as I have been informed, not only by

some mineral writers of good credit, but also by eye-witnesses, and partly by my own observation.

4. THE fourth circumstance, which may be alledged to the same purpose with the three foregoing, is, that it seems possible, from some gems, by menstruums, to obtain tinctures, that seem rather extractions, than dissolutions, strictly so called. I will not urge the chymical processes, that may be met with in some authors to this effect; because some circumstances in the things, and in the writers, made me so far suspect those I could try (and those that required undiscovered menstruums, as they may be true, so, for aught I know, they may not) as to keep me from meddling with them. But I remember, I once made a menstruum (I say once, because its preparation is so subject to casualty, that I have often failed in it) which being poured upon well coloured granates, not only not calcined, but entire, was in no long time beautified with a high and lovely tincture, which was admired by very skilful persons, to whom I shewed it, because the menstruum was not more corrosive than white-wine; and which yet I therefore took to be a genuine tincture, partly because it was drawn in the cold, partly because the liquor would not tinge itself by standing, if no body were put in it; and partly because it drew a tincture from antimony of a very differing colour from this we speak of. Nor are granates the only gems, which I have made the liquor work on, in the cold.

5. To these four circumstances I shall add this fifth, that some gems, which jewellers affirm, without scruple, to be rubies, sapphires, &c. either are colourless, or have other colours, than those that are wont to belong to them. That famous goldsmith *Benvenuto Cellini*, in his little Italian tract of his own profession, admonishes his reader, that there are one kind of rubies, that are naturally white (and not made so by art) which he proves by the degrees of hardness peculiar to rubies. And the same author elsewhere tells us of beryls, topazes, and amethysts, that are white. And it seems, by what he says not far from that place, that the Italian jewellers did not look upon the tinctures of gems, as any thing near so essential to them, as they are commonly reputed, since they reckon topazes and sapphires, whereof one is blue and the other yellow, but both extremely hard in comparison of other gems than diamonds (and perhaps rubies) to be of the same species. The degree of hardness of rubies and sapphires is oftentimes so equal, that I knew an expert English jeweller, who for that only reason (for he knew not whence the difference of colour might proceed) took rubies and sapphires to be of the same kind of stone.

AND that gems, referred by lapidaries to the same kind, may be very differently tinged, is a truth, whereof I have seen notable instances in diamonds themselves; which I therefore prefer to other instances, because the extreme hardness of diamonds is such, as keeps jewellers from mistaking any other stone for a true diamond, if they are permitted to put them on their rapidly moved wheels employed to cut them. Now, of true diamonds I have seen some, that were yellowish; others, that were more yellow; and, among the rest, one that was so perfectly yellow, that I first took it for a fair topaz, though it were a diamond valued at near three pounds weight of gold. I have also seen diamonds, and those rough, as they came directly out of the *Indies*, and were soon after bought by traders in diamonds for such, which were either bluish or greenish. And I particularly contemplated one stone, which, if its shape and other things had not convinced me of the contrary, was so green, that I should have taken it for an emerald.

I REMEMBER I had once occasion to buy a considerable number of small rubies, divers of which were very curiously shaped; and coming to look upon the whole parcel more leisurely,



leisurely, than my haste would permit me when I bought it, I found in a great number of other stones, one, and but one, that was devoid of any colour; but in any other respects was so like the rest, as invited me to conclude, that it would have encreased their number, but that it was coagulated and hardened before the mineral pigment had tinged it of the same colour with the rest. In which guess I was confirmed, when having met with a gentleman, who had been in the chief places of the *East Indies*, where rubies are found, and particularly at the river of *Siam*, or *Pegu*, near which he lived a good while, and where he frequently saw rubies taken out of the bottom of the water, and sometimes took them out himself; I learned of him by enquiry, that he had there seen several stones, each of which was partly a ruby, and partly colourless: and sometimes in the same stone, there would be two portions of one sort, and the third, though lying betwixt them, of another; which has frequently obliged the jewellers considerably to lessen the bulk of such stones, by cutting off the untinged part. And, if my memory do not much deceive me, I saw, in a great and curious prince's cabinet, among other rarities, a ring, in which was set a stone of a moderate bigness, whereof only one half, or thereabouts, was well tinged, the other being colourless. In gems, that are less precious, and not so transparent, especially in agates, and in opacous gems, I could easily give a multitude of instances of the differently tinged parts of the same entire stone. And I usually wear in a ring a small sardonyx, that was once a great prince's, wherein there are three portions, one within another, the uppermost black, the middlemost of a kind of chestnut colour, the other of a blue, almost like a turquois; each of which portions is exactly of a fine oval figure, and each of the two uttermost is throughout of a very uniform breadth as well as colour, and exactly parallel to the other. But it would not be here so proper, as it will be hereafter, to multiply instances of opacous gems: wherefore (having mentioned only the sardonyx, because it is not always opacous) I shall add concerning transparent ones, that jewellers reckon among sapphires not only that sort of azure gems, which usually pass for such, but also another sort of stones, because of their sapphirine degree of hardness; though for their want of tincture, they call them white sapphires.

6. THE sixth and last circumstance belonging to the foregoing argument or consideration is this, that sometimes one may find gems, that are partly tinged and partly not; as if the tinging pigment mixing with one part of the matter, whereof the stone consisted, whilst it was liquid or soft, were not copious enough to diffuse itself to the whole, nor to give an equal intense colour to all that portion that it tinges. It is true, that in some cases, the diffusion may be stopped by the petrescent juices coagulating first in another part than that with which the tincture was mixed. And perhaps, in some other cases, the different colours may have belonged to differing portions of matter, coagulating upon or against each other, at different times, yet so as to seem one entire stone; as I may have hereafter occasion to declare. Yet since, which soever of these explications be admitted, it will, if it belong not to this place, at least, confirm our main hypothesis (of the origin of gems from fluid or soft materials) I shall return to what I was saying about gems, partly tinged, and partly colourless. And having only intimated upon the by, that in some hard semidiaphanous stones, European and East-Indian, I have observed a very unequal and irregular diffusion of the tincture; I shall add to the things, that may be gathered in favour of the proposed conjecture from some of the things before (as also since) related, these two particulars:

THE one, that I have (as I think I elsewhere mentioned) seen in *Italy*, among rarities, a large piece of crystal about the bigness of my two fists, whereof the pyramidal part was of a transparent green, the vertex being richly tinged like an emerald; but the further

further the colour spread from the vertex, the fainter and paler it grew ; so that, before it came near the base, it was quite spent, if I may so speak, leaving the bigger part of the stone transparent, but colourless, like ordinary crystal. And by this, perhaps, we may explain an expression of *Josephus Acosta*, where he says, that emeralds grow in stones like unto crystals ; and that he had seen them in the same stone fashioned like a vein ; and they seem, adds he, by little and little to thicken and refine. And in the same place, this learned author has a memorable observation, that may confirm both what I have just now related, and what we mentioned a little above, about colourless gems : I have seen, says he, some that were half white and half green ; others all white, and some green and very perfect. And this is the first particular I was to mention.

THE other is afforded me by the way I have used, and elsewhere described, of giving to pieces of rock crystal passably good tinctures by mineral fumes. And supposing them thus coloured pieces to be as intire stones, as the beholders have generally believed them, the instance will be pertinent to our purpose in spite of an objection. For though the colours thus given are not wont to pervade them very deep, and have their penetration assisted by no faint degree of heat ; yet it is to be considered on the other side, that these pieces of crystal had attained their full hardness, and after their colouration, are cut and polished like other crystals : whereas the gems, that our conjecture means, are supposed to have been tinged under ground, when they were yet fluid, or at least soft. That there are sometimes generated in the bowels of the earth, mineral exhalations capable of applying themselves to the stones they meet with there, I have, in another discourse, sufficiently declared. That also some hard and stony substances have been actually tinged with such mineral steams, I shall, in the subsequent part of this discourse, have occasion to take notice. And I remember too, that even in so hard a gem as a sapphire, I have observed the efficacy of these subterranean fumes ; having divers times seen one of those stones, wherein a fine seal was cut, which continued so oddly tinged, notwithstanding what had been taken off to reduce it to an exquisite shape, that having inquired of a skilful person of my acquaintance, by whom it had been engraven, he both assured me, that he had found it of the full hardness of a sapphire, and confessed to me, that the mineral fumes had so oddly tinged it, that in his opinion, it might, by the looks, pass rather for a Chalcedonian.

Of subterranean  
fumes, &c.

AND now, Sir, I fear I may need your pardon, for having been so prolix in discoursing of one of the particulars belonging to our argument ; to excuse which, I have no other apology to make, but that I hope what hath been delivered will scarce seem impertinent, and that I might easily have made it more tedious, if, to decline doing so, I had not purposely made some omissions.

HAVING then said thus much about our fourth consideration, I proceed now to add, in the fifth place, on the behalf of the hypothesis hitherto favoured, an argument, which I presume you will not think inconsiderable ; namely, that solid gems may include heterogeneous matter in them. Several instances of this sort, in opacous stones, I elsewhere recite upon my own observation ; but in transparent ones they are very great rarities ; and therefore it will not, I presume, be thought strange, if I mention but a few.

FIRST then, on this occasion I remember, that a very ingenious and qualified lady, who had accompanied her husband in an embassy to a great monarch, assured me, that she brought thence, among several rich presents and other rarities (some whereof she shewed me) a piece of crystal, in the midst of which there was a drop of water, which by its motion might be very easily observed, especially when the crystal was made to change its posture. And, if my memory deceive me not, I have, in some pieces of  
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rock-crystal, taken notice of things, that seem to argue, that somewhat or other was intercepted within the body of the stone.

A CURIOUS person, that traded much, and was very skilful, in Indian gems, particularly grizolettes, which he got from the Indies, and whereof he shewed me the largest I have yet seen, being asked by me, whether he had ever found in them any heterogeneous substance, which something, I had observed, made me suspect, that some of them might harbour, notwithstanding their hardness; he averred to me, that among divers rough ones, that were brought from the *Indies*, he had with wonder seen one, that was about the bigness of a philbert, in the solid substance whereof there was a cavity with a certain liquor in it; which, by changing the posture of the stone, might be made to move to and fro in the cavity: and when the drop was settled, it was of the bigness of a round pearl, that he shewed me, which wanted somewhat of a moderate size for a neck-lace. And when he had answered the questions I proposed him, to clear my doubts, he added, that this rarity made the stone, which was otherwise of a small value, prized at an hundred pounds. And I have myself seen a monstrous gem, if I may so call it, and little less a rarity than the former, that an acquaintance of mine had bought (as I afterwards learnt) from this relator; whose narrative about the grizolette I think the more credible, because that having had the curiosity to break a stone, that was brought as a rarity from the *East-Indies*, where gems are often harboured in such stones, I found in the solid substance of it (which was so hard as to strike fire, like a flint, and in its little flakes was at least semi-diaphanous) a cavity, wherein were coagulated very minute but polished and crystalline stones, which seemed to have their points inwards; which argued, that there had been some liquor, in which these glistering particles had shot, though in process of time the remaining and incoagulable part of it may have been imbibed by the ambient matter, if not have escaped thorough it, by virtue of some peculiar congruity of it with the pores of the stone. Which need not be thought impossible, since experience has assured us, that some solid stones, and even gems, may be, though slowly, penetrated, or have their texture altered by common water. Nor are these the only heterogeneous substances I found included in this stone.

AND if, as amber is reckoned among gems, and is sometimes of a greater hardness, than one would expect, so I could reckon it among true stones, it were easy for me to borrow thence a great confirmation of what I have been saying; and, however, it will afford me an illustration of it. For, not to mention many things, of what I elsewhere recite myself to have seen in amber, I have now by me a fine piece of clear and solid amber (presented me by a person no less extraordinary than it) in which is included a large intire fly, in shape and size much like a grass-hopper, but variously and curiously coloured, with his wings displayed.

To these observations I shall add only this; that I have had myself, and shewn to others, one of that sort of pale amethysts, that some call white amethysts; which had been cut, to be set in a ring, or turned into a seal, and was, like that sort of gems, so hard, that I could readily cut glass with it, and yet in the body of this stone there appeared to be a considerable number of things, that looked just as if they had been hairs, some of them lying parallel, and others inclining to one another; and having contemplated them, as well by day-light as candle-light, and in divers positions in reference to the light and the eye, some of them seemed at times to be of a lovely reddish colour, but reflecting the light, as if they were well filled either with air or water: but for the most part they did, as I was saying, seem to be hairs of a brownish colour, which made the stone not a little wondered at, even by curious and skilful men. I leave you to judge, whether it will be fit here to add, that I have sometimes suspected, that even

in diamonds themselves there may possibly be found intercepted, or mingled with a pure lapidescent substance, some particles of heterogeneous matter. And that in this suspicion I was somewhat confirmed, as by the odd clouds I had observed in an extraordinary diamond, and by some hydrostatical, and other observations I made about those stones (some of which I found heavier than either crystal or white marble); so by my having purposely demanded of an ancient cutter of diamonds of great practice and experience, whether he observed not a sensible difference of weight among diamonds of the same place: for to this he replied, that he had, especially in those that were cloudy or foul; inasmuch, that shewing me a diamond, that seemed to me to be about the bigness of two ordinary peas, or less, he affirmed, that he sometimes found in diamonds of that bigness about a carat (which is by common estimation four grains) difference in point of weight.

SIXTHLY, the last argument I shall employ to shew, that the matter of divers gems may have once been fluid, may be taken from the proofs you will meet with (in the following part of this tract) of the second member of our hypothesis. For if it shall appear, that several even of the transparent gems have metalline or other extraneous mineral bodies mingled with them, *per minima*, it will be very agreeable to reason to suppose, that such a mixture was made, when the mingled bodies were in a fluid form; since, beside that one may well ask, how else the metalline corpuscles came to be conveyed into such compact and hard bodies as gems, it is very easy to conceive, if our hypothesis be admitted, and very hard otherwise to apprehend, how among bodies, that differ *totò genere*, as metals and stones, there should be made mixtures so exquisite, as many of these appear to be, partly by the uniform coloration of the gem, and partly by the diaphaneity retained notwithstanding this dispersion of mineral pigments through the whole mass; and in many instances also by the curious figuration, that we have lately been discoursing of.

#### P O S T S C R I P T.

To all the foregoing circumstances, I can now add something, that I met with, since I thought to conclude with the last of them, and that tends highly to the confirmation of our hypothesis. In a tract, that makes part of a small book freshly published in French, principally to acquaint men with the ways of estimating gems, according to the rates of modern jewellers, the anonymous, but curious author, takes occasion, to give us, from the mouth, as he affirms, of the famous late travellers he conversed with in divers places, and whose relations are indeed the recentest I have seen in print, an account of the number, and names of the places, where diamonds and rubies are found in the *Indies*; adding some circumstances and particularities about the qualities of the soil in those places, that I have not elsewhere met with. This author, then, speaking of the first of those three diamond-mines, which he makes to be the only ones in the *East-Indies*, having told us, that the stones are there found, some in the ground, and some in the rock, subjoins, that those that are drawn from the rock, or the neighbouring parts, have ordinarily a good water; but for those, which are drawn out of the ground, their \* water partakes of the colour or soil, wherein they are found. So that if the earth be clean and somewhat sandy, the diamonds will be of a good water; but if it be fat or black, or of another colour, they will have some tincture of it. Nay, he

\* Que s'il y a quelque sable noir ou rouge parmi la terre, le Diamant aussi en aura quelque'un, p. 9.  
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immediately annexes, that if there be some black or red sand among the earth, the diamond will also have some grain of it. And elsewhere mentioning the second mine of diamonds, which the natives call gems, he admonishes his reader, that in this, as in the mine of *Visapour*, which is that formerly mentioned, the stones partake of the quality of the soil where they are found; so that if that be boggy or moist, the stone will incline to blackness, and if it be reddish, it will have an eye of that colour. Elsewhere he tells us, that of late years there were found in the kingdom of *Goleonda* store of diamonds, which were brought to the Nababe, or first minister of state, who forbade the making any further search after them, finding not one in the whole number to have a good water, all of them being black or yellow. But by the way, whereas this author affirms it as a clear truth, that as gold is the heaviest and most precious of metals, so diamonds are the hardest and heaviest of all stones, he must excuse me, if I declare, that what he asserts agrees not with my experience, who have tried the weight of an uncut diamond hydrostatically, having taken such a course to estimate its specific gravity, as I find not to have been yet taken by any other, and which you will easily grant to be more exact than any other of the known ways can be.

THE argument that hath detained us all this while, comprized so great a variety of matter, and may, I hope, perform so great a part of my task in this discourse, that though I shall not apologize for having dwelt so long upon it, yet I shall think myself obliged to make some amends for my past prolixity, by being succinct in the remaining part of this treatise; and therefore, having left off with an intimated promise to shew more fully, that divers gems contain metalline, or other mineral substance, in them, I should immediately connect those arguments to what hath been lately said, but that I think it altogether requisite, to make way for what is to follow, by first taking notice of a main objection that may be urged against the doctrine we have been proposing.

THIS is taken from the figuration of some gems, and especially the prismatical one of crystal, and seems the more fit to be urged against us, because we ourselves have, in the second of the above-recited arguments, given several instances of it. For it seems scarce possible, that so curious a shape should be so uniformly produced in such a multitude of crystals, great and small, unless there were some seminal and plastick power to fashion the matter after so regular and geometrical a manner.

BUT he that shall attentively consider, what I elsewhere say concerning the figuration of salts, and of metalline and other magisteries dissolved by, and concoagulated with salts, may be very much assisted to discover the invalidity of this objection. But yet, because I confess it is very specious, if not important, I am content here to consider it a little more particularly.

TO this plausible objection then I have two or three things to answer; first, that there is no absurdity to conceive, that, if there be a seminal and plastick power in mineral bodies, it may be harboured in liquid principles, as well as otherwise. For we see, that the seed of animals, which oftentimes, as in elephants, rhinoceroses, &c. produces hard and solid bones, teeth, and horns, is at first but a liquid substance; and the formative power in some trees and their fruits does convert the alimental juice into woods, shells, and other bodies very solid and ponderous.

BUT secondly, I elsewhere \* shew, that even in the figures of alum, vitriol, and other salts, that are so curiously and geometrically shaped, there is no necessity to fly to a distinct architectonick principle; but that those bodies themselves may receive their shapes from

\* See the Origin of Forms and Qualities, now published by the Author.

the coalition of such singly invifible corpuscles, as by the motion of the fluid, wherein they did swim, and by divers affiftant circumstances, are determined to ftick together rather in that manner, than in another. That this may be applied alfo to other bodies, I fhall need to fhew in this place, by no other inftance than that of the falt, that (in this or fome other paper) I formerly told you I made of common falt, only by the help of oil of fulphur, or of vitriol and water. For though it be manifefly a factitious body compounded of falt and fulphur, and fuch a body, that therein the fea-falt, whereof it was chiefly made, has had its own nature destroyed; yet, by reafon of the figure of the resultant corpuscles, and their fitnefs to convene, when difsolved in water, into curioufly fhaped bodies, this factitious falt, when I have rightly prepared it, did fundry times fhoot into long cryftals with points like diamonds, that did emulate native cryftal as well in the regularnefs of the fhape, as in the tranfparency of the fubftance. And to make it the more evident, that it was partly the figure, that happened to refult from the operation of the oil of vitriol upon the fea-falt, and partly other circumstances, that determined the fhape of the cryftals; I fhall add, that ufually, when the quality or proportion of the oil of vitriol was other than it fhould have been, or an error was committed in fome important circumftance or other of the operation, the faline concretions, though they did not fhoot at all like cubes, as the fea-falt, which they were made of, would alone have done; yet they did not fhoot any thing at all like rock-cryftal, as did thofe formerly mentioned; and for all this did, by reafon of the curious fhapes of the corpuscles they confifted of, fhoot into cryftals, for the moft part, finely figured; though fometimes of one fhape, and fometimes of another. And that you may not have any fufpicion, as if the regular figure, which fea-falt is naturally of, is any way neceffary to fuch figurations, I will add an experiment, that I devifed to fhew, that even out of a petrefcent juice fuch curioufly figured bodies may be made. I took then fome ftony ftiræ, elfewhere mentioned to have been found in caves or grottos, where petrefcent liquors coagulated before they have time to fall down; and having difsolved them in fpirit of verdigris, I put the clear folution to evaporate in a digeftive furnace, after the ordinary manner; by which means, though I made the experiment more than once, I had rather a coagulated mafs, than any thing like cryftals. Whereby you may learn the truth of what I was faying, that a concurrence of divers circumftances may be requifite to determine the figuration of confiftent bodies made out of fluid ones; fince here, for want of time for making occurfions enough for the particles to concrete in after the moft convenient manner, the experiment fucceeded not: wherefore it being agreeable to my notions, that fome forts of bodies may require a longer time to make fuch a convention in, than others, I allowed many days to another folution of ftiræ made in the fame menftruum; after which there fhoot, as I defired, about the fides and bottom of the glafs a number of diftinct cryftals, long, tranfparent, and curioufly fhaped, moft of which, I think, I can yet fhew you.

PERHAPS it will be faid, that the petrefcent juice, when broken, does oftentimes appear to abound, within, with ftiræ, or narrow ftreaks like thofe of antimony; and that I myfelf obferve fome gems to be made up of thin flakes or plates; which internal figuration feems to be much more difficult to be accounted for, without a plaftick form, than the external.

I WILL not reply to this, that, for aught I know, divers known falts would, when broken, appear to be geometrically figured, even in the leffer corpuscles, as well as they are evidently fo in their entire bulk, if we had eyes quick enough to difcern the fhapes of the minuter, as well as of the bigger bodies. And we have great inducements to think, that whether or no *Cartefius* do rightly make the invifible particles, of which

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the smallest visible grains of sea-salt are made up, to be long and rigid like sticks; the minute visible concretions, of which the bigger grains of salt consist, are, as well as themselves, of a cubical figure; I will not, I say, insist on this reply, but proceed to alledge, that there are divers bodies so luckily shaped, that upon a slow coalition, they will convene into a multitude of manifest concretions; some of which will consist of streaks, and other be made up of flakes; as in the sal armoniack commonly sold in the shops (for I speak not of the native, that is said to come from *Armenia*,) though it be avowedly a factitious body, you may often observe, upon breaking the bigger masses, great multitudes of streaks, like those we may usually observe in the broken striae of petrifying water. And I have more than once seen, and also made, artificial concretions (of whose preparation I elsewhere speak) some of which consisted of salts alone, and others of salts and minerals, as stones or antimony, which look very like talk, being white bodies made up of a multitude of very slender streaky particles lying longways one upon another, as in that mineral. And as I have taken out of earth many concretions, which, as they were for the most part outwardly shaped like rhombusses or lozenges, were composed of a multitude of flat and extremely thin plates; so I have sometimes taken pleasure to imitate such concretions by art. And though a solution of silver in putrified aqua fortis does usually afford only a great company of small, thin, and seemingly simple flakes, like scales of fish, because men have not any design like ours in procuring the concretion; yet having dissolved a good quantity of the metal together, and suffered it to shoot leisurely, and with due circumstances, I have obtained sundry crystals, which both were geometrically figured without, and consisted of a multitude of exceeding thin flakes orderly sticking to one another. And I remember, that whilst the objection, I am answering, was in my thoughts, I pitched upon a yet more pregnant experiment for the clearing of it. For considering how tin-glass, though a compact and ponderous body, does naturally consist of a multitude of shining polished flakes (which may be easily perceived and distinguished by breaking a lump of it into three or four pieces); I found by trial what I expected, that though a mass of this mineral were beaten to powder, yet if it were melted and suffered to cool of itself, the disposition of the component particles would determine them to stick to one another in broad and shining flakes, whereof many will be incumbent one upon another, and some cross to one another at various angles, according as the matter happened in its several portions to be diversely refrigerated. And some factitious bodies may afford us the like instances, as I have observed in some mixtures of copper, iron, and other minerals; and very conspicuously in good regulus martis stellatus, whose internal configuration may be found by breaking it; by which way I have observed with pleasure, that the regulus abounded with flat and shining flakes of an almost specular polish.

If it be urged, to confirm the former objection, that some lapidescent juices, even of those we mentioned in these discourses, do concrete, even whilst men are looking on; and yet our stony striae, above mentioned (which probably may be also hastily coagulated) have in some places a streaky, and in other places an angular configuration of parts; I answer first, that I have seen divers of that kind of concretions, which, as far as the eye took notice of, were made up of parts confusedly jumbled together. And next, that (to consider now those whose texture is more uniform) I have found by trials, that, if there be a due disposition in the component corpuscles of bodies to such configurations, they may be brought to concrete accordingly in a far shorter time, than almost any, that have not tried, would expect, not to say believe. Having sometimes, for curiosity's sake, warmed six or seven ounces of aqua fortis, glutted with fine silver, till the mixture was all brought into a transparent liquor; and having then put the clear, but

strong glass, that contained it, into cold water, that the menstruum might be the more hastily refrigerated, I observed, that when once the dissolved metal began to shoot, the coagulation into figured crystals proceeded so fast, that a naked eye could see the progress of it. And having sometimes put a quantity of salt and snow, or of some other strongly refrigerating mixture, into a convenient glass, and wetted the outside with a strong solution of sal armoniac, or some urinous spirit, though in less than a minute of an hour it would be coagulated; yet the salt, into which it shot, had usually a curious and determinate figure, according to the nature of the liquor that afforded it; as I have often shewn the curious.

PERHAPS you will say, that these instances are taken from saline bodies, which are, for the most part, disposed to convene in smooth surfaces, and angular shapes, and easy enough to be wrought on by the external cold; and it may yet seem strange to philosophers themselves, what in some cases must have happened, if our hypothesis be admitted, namely, that external circumstances and accidents, such as the figure of a mould or womb, the coldness of the ambient, &c. should visibly, and sometimes not a little, diversify even the internal figuration of close and solid minerals and gems, without excluding all those that are supposed to be of a quicker concretion.

WHEREFORE, to clear this difficulty, it may not be amiss to subjoin an experiment that I devised to shew, that if the corpuscles of a body be so shaped, as to be fitted by their coalition, to constitute smooth (and if I may so speak) glossy planes, though they be variously shuffled and decomposed, as to their pristine order, yet, if they be but a little kept in a state of fluidity, that they may the fitter place themselves, or be placed by other agents, they will presently be brought to convene into smooth and shining planes, and the situation of those planes, in reference to one another, will be more uniform and regular, than almost any one would expect in a concretion so hastily made; notwithstanding which, their internal contexture will be much diversified by circumstances, as particularly the figure of the vessel or mould, wherein the fluid matter concretes.

CONSIDERING then, that, according to what I noted already, if we break tin-glass (taken for the bismuth of the ancient mineralists) as it is wont to be sold in lumps in the shops, it will discover a great many smooth and bright planes, larger, or lesser, according to the bigness of the lump; which sometimes meet, and sometimes cross one another at very differing angles: considering this, I say, I thought it probable, that a body, that had already been melted, and was apt to convene into such planes, not only would do so upon another fusion, but might have the order and bigness of those planes diversified by the figure and capacity of the vessel I should think fit for my purpose. Wherefore, having beaten a sufficient quantity of it to powder, and, when it was well melted, cast it into a good pair of iron moulds, whose cavity was an inch in diameter, we had a bullet, which being warily broken, did, as we expected, seem to be, as it were, made up of a multitude of little shining planes, so shaped and placed, that they seemed orderly to decrease more and more, as they were further and further removed from the superficies of the globe; and they were so ranked, that they seemed to consist of a multitude of these rows of planes reaching every way, almost like so many radiuses of a sphere from the center or middle part to the circumference: whereas, if we melt tin-glass in a crucible, and let it cool there, the matter being taken out and broken, will appear indeed full of smooth planes, but (as was lately intimated) very irregularly and confusedly associated or placed.

I WILL not now stay to enquire, whether the orderly composition of the planes in our bullet (which some curious persons, that I shewed it to, looked on, as a not unpleasant sight)



fight) may be derived from this, that the matter was cooled first on the outside, by the contact of the cold iron mould, and the neighbourhood of the ambient air; and that the coagulation being once thus begun, the parts of the remaining fluid, as they happened to pass by this already cooled matter, with a motion which, by reason of their removal from the fire, was now slackened, they were easily fastened against the already stable parts (as may be illustrated by the concretion of dissolved nitre and alum, both about the injected sticks, and the grains, that first concrete against the sides of the vessel) and the refrigeration still reaching further inwards, till it came last of all to the middle of the globe, that being the remotest part from the refrigerating agents; the apposition was successively and orderly made, till the whole matter was concreted. But (as I was saying) I must not now stay to inquire, whether the figuration of our bullet may be explained after this or some such way; or whether we are not to take in some subtle or pervading matter, or some other catholic agent? For though such points may be well worth discussing, and we may possibly elsewhere say something of them; yet here it may suffice to say, that we have varied the foregoing trial, by casting bullets of some other bodies (and particularly the simple regulus of antimony) wherein it succeeded well enough, though the produced texture were not so uniform as in tin glass. And I also tried, that having cast melted sulphur itself into a globulous body of about five or six inches in diameter, and warily broken it, though one would think it an unlikely mineral to make any other, than a confused concretion, it presented me great fibres, almost like little straws, whose number, and, in great part, orderly situation, afforded me a much less unfit instance for my present purpose, than one would have lightly expected. But what I came from saying, may serve to make out what I propounded to myself; which having named already, I need not here repeat.

BUT one thing more there is, that may be pertinent on this occasion, namely, that I have broken divers marcasites of a peculiar sort, that were either of a roundish, or of an almost cylindrical figure, to observe their internal structure and qualifications; whereupon, I found in more than one of them (for I remember not, that I did in all) a great many rows of little planes or glistering corpuscles, reaching from the innermost to the external surface, and in those, that were somewhat cylindrically shaped on the outside, these ranks of gold-coloured particles in the several planes of the broken mineral seemed like semi-diameters issuing out from a row of physical points, conceived to be placed on an imaginary line, lying almost like the axis of a cylinder between the opposite ends (though I do not well remember, how near it reached to them); as if the cavities of the chalk or clay, wherein these marcasites were found, had made the soil like a mould, wherein the matter of the marcasite being detained, whilst it was in a fluid form, did afterwards concrete much after the manner, that the bullets of tin-glass, regulus, &c. did in our moulds. But the prosecution of this conjecture belongs to another discourse.

I SHALL therefore now proceed to a further answer to the formerly raised objection: therefore, as to the exquisite uniformity of shape, which is so admired in gems, and is thought to demonstrate their being formed by a seminal and geometrizing principle: though I have, in the second of the above-mentioned arguments, ascribed to them such curious figures, as argue their having been generated after the way proposed in our hypothesis; and though also I willingly allow their shapes to deserve from us a delightful wonder at the curiousness of nature's, or rather her author's, workmanship, yet, upon a more attentive surveying of them, I do not find the uniformity to be near so great as is wont to be imagined; but have rather met with such diversities, as agree well with our hypothesis about their figuration.

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IN several transparent gems, it seemed manifest enough to me (as I lately also noted) that the shape was, in great part, due to the figure of the womb, or mould, wherein the matter, whilst liquid or soft, happened to settle. In some other transparent and well figured gems of the same kind or denomination, and sometimes growing very near one another, by a diligent inspection I found a manifest and sometimes very considerable difference in their shapes, either as to the number, or the figures, or the bigness of the sides or planes, that made up the respective gems; or as to two, or all, of these; comparing these deviating particulars with what would have been in a stone of that kind or denomination, that were perfectly figured. This I had opportunity to take notice of, particularly in two sorts of stones; the first granates, of which I had a considerable number brought me out of *America* growing in one lump of matter; but in distinct parts of it, and without touching one another: among which I took notice of a manifest disparity of shape, and so I did in some African ones, that were presented me; as also in others, that were European, one of which, that was of an extraordinarily large size for a figured gem of a transparent kind (for it weighed above eleven drachms and a half) I considered with a particular attention, and found, that though it seemed to have been coagulated in a fluid medium, and to consist of twelve planes, at the concurrence of two or three of which it seemed to have been broken off from the womb or root; yet it was very far from the dodecahedron of geometricians. For, whereas that consists of twelve æquilateral and æquiangled pentagons, almost all the planes, that made up our granate, were quadrilateral and very different from what regularly they should have been, not only in magnitude, but in shape: for one of them seemed to have five sides, and of the rest, some were most of kin to a rhombus, others to a rhomboides; but the most were but little better figured, than those that the geometricians call the trapezia. And thus much for the first sort of gems, whose shapes I observed to be not regular. The second consists of those crystalline stones, which they call Cornish diamonds, and which are some of them much harder than the Bristol diamonds, or perhaps, than rock-crystal itself; it being easy to write upon glass with them. Of these stones having procured a good number (many of which I have yet by me) I took notice, by comparing them heedfully together, that though some of them were geometrically and curiously shaped like rock-crystal, having each six sides, whereof every two, that were opposite, were thoroughly like and equal enough to one another; and though the stone had a pyramidal termination, made up of several resembling and curiously figured planes, that terminated in a solid angle or apex; yet the greatest number, by much, of these titular diamonds was made up of stones, far from being so exactly and uniformly shaped, as those newly described. For though most of them had six long planes; yet oftentimes the opposite ones (besides that they were not so parallel to one another, as they should have been) were unlike and exceeding unequal; and those planes, that were to make up the apex, though apart, they were usually angular; yet being compared to one another, or to the regular patterns abovementioned, their figures, their bignesses, and their manner of concurring (which was sometimes not in a point or apex, but in a line) was so remote from being uniform, that this great diversity and irregularity agreed far better with our hypothesis, than with its rival. And yet in these stones, the want of room to coagulate freely in, could not with probability be pretended; for they seemed to have been formed separately in a fluid ambient, save at the bottom, where they were fastened to the rock, as appeared by an opacous root, if I may so call it, which still adhered to most of them. And, if I much misremember not, I have more than once in diamonds, newly brought from the *Indies*, and some of them very fair ones, observed a great want of uniformity in the areas of the superficial planes, or in their figures, or

both;



both; and sometimes too in the very number, as well as situation, of their solid angles or corners: about which I hope to recover some notes. And so I have done with the first part of my answer to the abovementioned objection; whereby it may appear, that there is no such regular and constant uniformity in the shapes of gems, but that their real likeness may be reconciled to our hypothesis.

But now in the second part of my answer, I shall endeavour to shew, that the figuration of gems may not only consist with our conjectures, but confirm them. For I have, more than once taken notice in the Cornish diamonds I have been mentioning, that sometimes a small stone of the same kind has made up, as it were, one body with a greater; so as that the lesser stone did not only adhere closely to the other, but was, if I may so speak, set or bedded in it. So that when the separation was made, there remained in the greater stone a cavity, whose figure did curiously answer that of as much of the smaller stone, as chanced to be harboured there. And, as sometimes I observed, that there was such an adnascency (if you will pardon the word) of a lesser stone to a much greater; so at other times, I met with the like of a greater to a much lesser, with a cavity in the lesser, answerable to that part of the greater that had been lodged in it. Which, for aught I know, allows us with high probability to conjecture, that the stone, to which the other grew, was first formed and hardened; since it retained its own shape, and that, whilst this remained adherent to the rock or soil, some more liquor, either, that came afterwards by chance into the same cavity, or (in case it were there before) that was less disposed to be coagulated by fastening itself against the solid body, that was already concreted: upon which account these two diamonds must stick close together, and yet be but contiguous, and a cavity, such as I freshly mentioned, must be left in the last concreted gem. Which may be illustrated by putting into a strong solution of pure nitre, or rock-alum, some little sticks of wood or any solid body, that may be kept steadily in the posture; for you will see many coagulations begin to be made against them, and the crystals thus concreted will necessarily have their figures incomplete, and have in them cavities correspondent to those parts of the stick, whereto the saline corpuscles fastened themselves. To which I shall only add, that though I have given instances of the adnascency of figured stones only in Cornish diamonds, yet they are not the only transparent minerals, wherein I have been able to observe it. And particularly I remember, that I observed among some minerals, left by a goldsmith to his widow, a fine transparent and neatly figured stone, which seemed to be pure crystal, but was coagulated about a kind of branching wire, whereof a good part was inclosed by the stone, that seemed to grow out of a piece of ore, that looked like silver-ore, and which the woman, that was a curious person, upon the strict enquiry that I made, affirmed to be, together with the above-mentioned branch, good silver, produced by nature in that form (which I thought the more credible, because of the odd and almost hair-like shape, wherein I have seen silver-ore to have, as it were, grown); which will excellently agree with the resemblance, I was just now proposing, betwixt the coagulation of dissolved salts and the liquid matter of gems, about stable bodies partly immersed in those fluids.

THE very many circumstances belonging to our first argument, and the last answered objection, have so long detained us, that I doubt, you now think it more than time I should advance to, and dispatch the second of those grand considerations, whereon I at first intimated our hypothesis was founded; and this is built upon the weight of some gems, which being greater than that which seems to belong to them, as hard and transparent stones, I think we may probably derive it from metalline or mineral mixtures.

I QUESTION not, but as you will think this allegation new, so you will be apt to question, how I come to know the truth of what I here deliver; since, though gems are wont to be estimated by lapidaries, as they weigh such or such a number of carrats, or of grains, yet they compare only the weight of this and that stone of the same kind in reference to one another, as the greater and lesser weight argues the greater or lesser bulk, without looking after, or knowing how to discover the specifick gravity of several gems, which depends not on the greater or lesser bulk; as (if you know it not already) you will gather from what I am now going to relate.

CONSIDERING then with myself, that for my purpose, it was requisite to have a gem as free as I could get from the metalline mixtures, that I suspected many precious stones to have; and remembering, that rock-crystal, as it is by mineralists reckoned among gems, so it is hard enough, as I tried, both to cut glass, and to strike fire; and that its having so great a transparency, and its being devoid of colour, makes it exceeding likely to be free from adventitious mixtures; I pitched upon it as the standard, whereby to make a probable estimate of the weight of gems; and having hydrostatically, and with a tender balance examined the weight of it, first in the air, and then in water, I found its weight to be to that of water, of equal bulk, as two and almost two thirds to one: which, by the way, shews us, how groundlessly many learned men, as well ancient as modern, make crystal to be but ice extraordinarily hardened by a long and vehement cold; whereas ice is bulk for bulk lighter than water (and therefore swims upon it) and (to add that objection against the vulgar error) *Madagascar* and other countries in the torrid zone abound with crystal.

HAVING thus found the ponderousness of crystal in reference to water; when I met with a coloured gem, whose specifick gravity I guessed to be sensibly greater, I sometimes gave myself the trouble (for a trouble it is) to weigh them in the air and in the water, and so discover, whether I conjectured aright. And if its specifick gravity did much exceed that of crystal, I thought it a probable argument, that there might be some metalline or mineral corpuscles mingled with the stony ones of the gems, and that also it may probably derive its tincture thence. I will not tell you, that I then found many sorts of transparent stones much heavier than crystal: for, besides that the trials were troublesome enough to make, I chanced to fall upon them in a place, where I had not any store and variety of gems to examine. But one instance among those that occurred to me, I shall here set down, because, being so notable, it may suffice to shew, that, as to some gems at least, my opinion of their having an adventitious gravity, and consequently ingredient, is very probable. I had some American granates, which I had a great and peculiar reason to believe had been once liquid bodies, and therefore thought them the more worthy to be examined; and finding their colour to be so deep, that they were almost opacous, and judging by my hand, that they were much heavier than pieces of crystal of the same bulk would be, I weighed them in a pair of nice scales in the air, and in the water, and found them, as I expected, to be almost four times as heavy as water of the same bulk, and consequently heavier by about a third part than pieces of crystal, equalling them in bigness, would be. Whence so great an accession of ponderousness proceeded, I shall tell you, when I come to my next argument; to which I shall advance, as soon as I have noted, that though, when coloured gems have a greater gravity than crystal, it is a probable argument, that they have some metalline pigment, or other mineral substance, mingled with them; yet, if such gems have no such surplussage of weight, it will not follow, that their colour cannot proceed from any mineral tincture; since it is not unreasonable to conceive, that a mineral substance may be present in a liquor (such as the lapidescent juice) that we suppose gems



to be made of, even when it adds no manifest weight to the body that harbours it; since I have observed (what is odd) that a mineral water, which by its taste, its effects, and the colour it would strike, appeared to be richly impregnated with iron, being carefully by me examined hydrostatically, did appear very little, if at all, sensibly heavier than common water.

THE third and last argument, I shall now make use of, is taken from hence; that out of divers medicinal stones, and even out of some fine gems, real and corporeal metals, or other mineral substances, may be extracted.

OF this argument I shall at present say the less, because the further prosecution of it will be more proper in the second part of this discourse, where I shall be obliged to handle it, with reference to opacous gems, in which its force will best appear. And therefore I shall desire you to take notice, when you arrive at that part of the subsequent discourse, of those particulars, that may serve to strengthen the newly proposed argument: and if it be objected, that the bodies, there treated of, are opacous stones, not gems, I have these things to answer.

FIRST, that divers stones, that are reckoned amongst precious ones, are opacous too; as the turquoise, the onyx, the sardonyx, &c. not to mention divers others, as catseyes, opales, &c. which are as it were semi-opacous. Besides I much question, whether diaphaneity be absolutely necessary to the essence, though it be to the beauty, of those precious stones wherein it is usually found. And I might here make it probable by discourse, that transparency and opacity oftentimes depend but upon the manner of the pigment's dispersion thorough the stony matter of the gem, and the convenient or inconvenient situation of the pores, in reference to the beams of light. But waving this speculative argument, I shall rather take notice, that several precious stones, and even diamonds themselves, have sometimes great clouds, which make them in those parts almost (if not quite) opacous, without being thereby hindered from being true diamonds or gems, of this or that kind, to which their hardness, colour, &c. makes them appertain: and not to mention cornelians, agates, and some other stones, that we may observe to be (as the tinging corpuscles happen to be, in a due or an over great proportion, mixed with the petrescent matter, and to be uniformly or inconveniently mingled with it) some of them transparent, and some of them semi-diaphanous; I have seen worn in a ring a sardonyx itself, that was transparent, as unlikely a gem as that is to be so. And as for granates, though you know, that both of them are diaphanous, yet I have had some figured ones, that seemed quite opacous; and I have others by me of several countries (whereof one very remarkable for its large size and geometrical shape) that art in some places diaphanous, but, as to the main bulk of their bodies, appear at least almost as dark as ordinary stones.

I FURTHER add, that I little doubt, but that experiments, not unlike those I shall hereafter tell you, I tried to obtain mineral or metalline substances from load-stones, native cinnabar, blood-stones, &c. might succeed in several other of the more ponderous gems, if it were not, that the glassy nature, or exceeding compactness of many of them, makes the mineral corpuscles, that are harboured in the stony and insoluble parts, to be inaccessible to our common menstruums. And when the metalline and mineral ingredient is very abundant, and the tincture of the stony parts not so very close, I question not, but even from transparent gems the adventitious ingredient may, in part at least, be dissolved. And to satisfy you about this matter, I shall now inform you, that having by the ponderousness of the lately mentioned kind of granates been induced to conclude them impregnated with somewhat metalline, and for that reason to think it fit to try, whether I could separate it from them, or otherwise discover it in them; I kept

some of them (in a crucible) for a competent time in the fire, and found, that they had exchanged their colour, or one not unlike that of unbrightened iron; and having reduced them to very fine powder, and digested some acid menstrooms, and particularly rectified spirit of salt upon them, they afforded me a rich tincture: encouraged by which, I hoped, that, without their being previously burnt, they would in aqua regis afford a tincture, and accordingly I obtained from crude granates (only reduced to very fine powder) a rich solution, which, though in colour it somewhat emulated a solution of gold, yet partly by the colour of the burned granates, and partly by the taste of this solution, I supposed, that another metal was likelier than gold to be the predominant mineral; and having gently evaporated part of that menstruum, I obtained from some of the rest certain crystals, whose shape, by reason of their smallness and disorderly coagulation, I could not well determine; and touching with the tip of my little finger the uncoagulated portion of the liquor, this part of a drop being put to a great many drops of the infusion of gall, did so immediately turn it into a substance, that seemed full as black, if not blacker than ink, as you would, I think, have been somewhat surprized to behold.

WHICH trial I made to examine the conjectures I had, that one mineral (for perhaps it was not the only, that helped to constitute these granates, was of a martial nature; which, if it were, I supposed it would, like other bodies that participate of iron, afford with galls an inky colour. I tried also with a parcel of small and red transparent stones, which some guessed to be granates, others, more probably, rubies, that being finely powdered, they would in an appropriated menstruum (made extraordinary strong) give a colour like that of dissolved gold. And that there were really some parts of the gem dissolved in the menstruum, appeared not only by the above-mentioned colour, but by these two indications; the one, that having put some of this liquor to some of the same solution of galls I just now spoke of, it produced indeed, at the very first, a dark colour, but not near so black as that of the granates, and in a trice let fall a copious precipitate, that was almost white: the other, that I was able to precipitate from it, by an urinous spirit, a reddish substance, which being suffered to dry in air, seemed to grow into bodies, in shape not unlike moss, and here and there small mushrooms, all of them prettily coloured. And from certain granates, that were in some places opacous, as well as in others diaphanous, I obtained a solution, from whence the superfluous liquor being abstracted, the residue, which was deeply coloured, did in the cold afford me a kind of saline concretions, which yet were not large enough to enable one to determine their figure.

AND on this occasion, I hold it not unfit to intimate, that perhaps, if men had curiosity enough to make trials, there would be other transparent minerals found capable of being wrought on by appropriated menstrooms. For I do not think, that every seemingly glassy contexture of a mineral makes it unfit to be wrought on: for though the clear spar, which in most of our western lead mines in *England* is found next to the metalline veins, be at least semi-diaphanous, and be of so glassy a contexture, that it usually breaks into smooth and glossy superficies, and looks like a talk, and also, for the most part, is made up of, and presently reducible into geometrically figured bodies, shaped like rhombusses or rhomboides; yet some other trials, that I have made with this spar, inducing me to suspect, that it was not indeed a talk, but a body of a much more open texture, I found I could dissolve it in several liquors, and particularly in good spirit of salt, which would presently work upon it, even whilst it was in lumps, and that without the assistance of heat; which observation may perhaps give some encouragement to such a curiosity as yours.



BUT by what I have said of the usefulness of menstrooms, I would not have you think, that they are the only instruments, wherewith something metalline may be obtained from some gems: for in another paper of mine (to which such trials more properly belong) you may find an account of some attempts of that kind of fusions and appropriated additaments. And however such trials may succeed with you, that aim at separating from a gem a metalline or mineral body of a determinate species; I can teach you an easy way, whereby I have (by the help of fusion) more than once manifested in the general, that there may be substances, partaking of a metalline nature, in some kinds even of transparent gems. And partly by the same way, and partly by some others, I have been able to determine probably enough, in some cases, that the mineral substance is predominant in it.

AND here, before I dismiss the first part of our essay, I think; I may possibly somewhat illustrate our hypothesis, if I briefly mention to you an experiment, I remember I once made to that purpose. And it was this: I reduced to powder some of those stiræ, that I have often spoken of, of water petrified, as it were, spontaneously. I also considered with myself, that I had found spirit of verdigris (which I make without the tedious preparations, that *Basilus* and others prescribe, by barely distilling, without additaments, good French verdigris, and rectifying the obtained liquor) I had, I say, found this menstruum to be, not only, as I elsewhere observe, a good solvent for many bodies, but also to be distillable from many of them, without leaving near so much of itself behind, as other saline solvents are wont to do: considering this, I say, I dissolved the stony stiræ in this liquor; and having suffered some of it to evaporate away, and put the rest into a cool place, I obtained, as I expected, store of small, but finely figured and transparent crystals, that shot much after the fashion of those of the purer sort of nitre. With some part also of the stony solution I mixed, in a convenient proportion, a high coloured solution of copper, made likewise in spirit of verdigris; and these two solutions being made with the same menstruum, and warily enough put together, did not precipitate one another, but afforded me, upon the evaporation of the superfluous moisture, among divers crystals, that were transparent and colourless, some that were richly adorned with a greenish blue tincture of the dissolved metal. What trials I made by this way, little varied, to imitate nature, by associating into transparent bodies stony and metalline substances, I cannot now give you a full account of; since I neither have by me the notes I set down about those trials, nor think it fit to make this first part of our discourse more prolix, than I now perceive it to be already.

## SECTION II.

*Containing a conjecture about the causes of the virtues of gems.*

WHAT has been hitherto delivered in the first part of our discourse, will, I suppose, make it allowable for me to be more succinct in the second. I shall now, therefore, proceed to those other considerations, which, being assisted by what has been already said, may, I hope, suffice, to keep our conjecture about the cause of the virtues of gems from seeming unreasonable.

AND my first observation shall be, that not only there is in the earth a great number and variety of minerals, already known by particular names; but probably there are very many others, that are not yet known to us.

THE former part of this proposition will not be doubted by those, that consider, how great a multitude of metalline ores, marchasites of several sorts, antimonies, tinned glaſs, fluores, talks of various kinds, spars, sulphurs, salts, bitumens, &c. are mentioned partly by chymists, and other mineralists, and partly by those that have given us accounts of museums and other collections of natural rarities; insomuch, that of only one kind of fossils, the diligence of some modern writers hath reckoned up between two hundred and two hundred and fifty; besides animals stones, as lapis bezoar, lapis manati, oculus crancri, lapis porcinus, &c.

AND as for the second part of our proposition or observation, you will scarce deny it, though you consider with me but these two things.

THE first is the small and inconsiderable proportion, that the perpendicular depth, that the generality of mines bears to the semi-diameter of the earth, reckoned to be above 3500 miles; so that, though our globe were inhabited by some hundreds of millions of men more than now it is, and they had curiosity enough to dig mines every where, and consequently there were millions of inquisitive and laborious men, more than really there are, their spades and pickaxes would, except here and there, penetrate so little a way into the earth, that a vast multitude of fossils might, by lying deeper in the bowels of it, continue undiscovered.

AND to this first observation I shall subjoin this second; that, as far as I have observed, almost every region affords minerals of its own, differing from those, that are taken notice of in other regions. And in particular countries, as in some shires of *England*, a curious and heedful eye may, I doubt not, observe several, that are not taken notice of by the inhabitants themselves; especially, if well-made borers were diligently and skilfully employed to pierce the ground, and bring up samples of divers fossils, that lie hidden under it. But having elsewhere discoursed of this matter, I shall here only tell you, in general, that in some parts of *England*, where I had more opportunity than in others, to exercise some curiosity about minerals, I met sometimes, in a small compass of ground, with a much greater variety than I expected, and several of them undescribed, that I know of, by any writer; of which sort I have received divers others from several parts, both of the old world and the new.

IN the next place, I consider, that nature has furnished the earth with menſtruums, and other liquors of several sorts, and endowed it with divers qualities. This I have already manifested in the discourse of subterranean menſtruums, whereto I shall therefore refer you; only taking notice in this place, that whereas water is abundantly to be met with under ground, and, for the most part, very copiously in mines, by which it is capable to be variously impregnated; this liquor itself, especially being thus altered, may, in some cases, act the part of no despicable menſtrum, and, on some occasions, otherwise concur to the production of mineral bodies.

I FURTHER observe, that the subterranean liquors, upon one account or other (for we need not now particularly determine it) are qualified to work, either as corrosive menſtruums; or as other solvents, upon many of the medicinal earths, and other minerals they meet with under ground: which minerals, having never been exposed to our fires, have their texture more open, and their parts more soluble, than those that have been melted by the violent heats of our furnaces.

AND that even common water will suffice to dissolve and impregnate itself, both with the saline, and, oftentimes, with metalline parts that it meets with in its passage, is obvious enough in the differing tastes, and other qualities of liquors, that all pass for common water, whereof some is found better, and some worse than others, to brew, some



to wash linen, some to dye scarlet, or other determinate colours; some to temper steel, and some for other uses.

BUT others, unquestionably more eminent instances, are given us by the mineral springs, whether thermæ or acidulæ, as authors distinguish those that are actually hot (as at *Bath*) and those that are saline, and, for the most part, sourish (like those at *Tunbridge*, and the *Yorkshire Spa*); of which two sorts, good store are enumerated by physicians and geographers; and of which a far greater number would be discovered, if men wanted neither skill nor diligence. And here I shall desire you to take notice, that, though common water do the most readily dissolve the salts, more properly so called, though not altogether pure, it meets with in the bowels of the earth, as we see it happens in those salt-springs that come not from the sea; yet there are also many other subterranean bodies, which, upon the score of their abounding with saline particles, will be dissolved by water, though they be of a compounded nature, and contain very differing substances; as it is plain in those waters of *Hungary*, and other regions, which, by the evaporation of their superfluous moisture, will yield vitriol, a mineral not only compounded, but decomposed, as containing in it a saline, a sulphureous, a metalline, and an earthy part (which, itself, I have found to be none of the simplest bodies); every one of which may be made distinctly to appear.

LASTLY, I consider, that the petrific juice of spirit coming to be in a sufficient proportion mingled with these impregnated waters, so as to coagulate them, and concoagulate with them; from their coalition may result those precious stones, that we call transparent gems. For it is certain, that bodies, that were a while before in the form of waters, may coagulate into stony striae, of whose odorousness and reducibleness into lime, I have already given an account in my discourses of lapidescent juices; of which you may command a sight. And that even diamonds themselves, the hardest of gems, were once fluid substances, the first part of this discourse has, I hope, evinced.

To which I shall now add, that procuring some petrified bodies to be brought me from a place in *England*, which I could not be admitted to, I found, that the petrific juice or spirit, that abounded in the earth of that spot of ground, was so penetrating; and so operative, that it made some of the vegetable substances, that were found in it, in their pristine shape, and, for aught I could perceive, bigness, hard enough to cut glass, as well as grave on iron. And it was among these rarities (if I much misremember not) that I picked up a (moderately) transparent body (which, I think, I have yet by me) that, by the shape, and other circumstances, I judged to have been a diaphanous gum, belonging to one of the pieces of petrified wood that had been brought me, and was hardened to a degree, that made it capable of scratching glass.

AND now to bring home these things to my present subject, I conceive, that some, at least, of the real virtues of divers gems may be derived from this, that whilst they were in a fluid form, or, at least, not yet hardened, the petrescent substance was mingled with some mineral solution or tincture, or with some other impregnated liquor; and that these were afterwards concoagulated, or united and hardened into one gem, as a diamond, a sapphire, a granate, an onyx, a blood-stone, &c. And as divers of the virtues of gems may be, in a general way, deduced from the commixture of these mineral corpuscles; so the greatness of those virtues, and the variety of those properties, in particular, may be ascribed to the peculiar nature of the impregnating liquors, to the diversity of them, and to the greater and lesser proportions wherein they are mixed with the petrescent juice.

To.

To render this conjecture (for I propose it as no other) thus summarily and briefly expressed, the more probable, it will be fit to recall to mind the arguments, whereby we have already shewn, both that gems were once fluid or soft bodies, and that divers of them were not simple concretions of a petrescent liquor, but consisted also of other mineral adventitious corpuscles: which may appear, partly by the separableness of such substances from some gems (as we exemplified in granates) partly by the specific gravity of others, and partly by the differing tinctures (whereof one, at least, may well be supposed adventitious) to be met with in gems of the same species, as rubies, sapphires, granates, and even the hardest stones, that we yet know of, diamonds themselves; of which (as is before noted) I have seen some yellow, and that to a great degree, some of other colours, but not so vivid; and some green, almost like emeralds.

Now since there may be in gems, and, in some of them, abundantly such adventitious corpuscles; and since there is cause to think, that some may be endowed with divers properties, and medicinal virtues; since also there is a great difference among these impregnating particles, and, probably, of a greater variety of them than is known to us; since, lastly, divers gems are not sparingly, but richly impregnated with these ennobling corpuscles, I see no sufficient reason, why some of the virtues of divers gems are not more likely to proceed thence, than from those unintelligible and precarious substantial forms to which they are wont to be referred.

BUT because there are some difficulties, that the objections of others, or my own thoughts, have suggested against our hypothesis; though I neither have time, nor do think it very necessary, to discourse amply of them; yet to clear the way for what I am afterwards to represent, I shall (though I can but briefly do it) say something to each, that may, perhaps, appear no insufficient answer; especially after I have declared, as I here do once for all, that I speak of the true and medical virtues that belong to gems; and that, as to those magical, and other extravagant properties, that either notoriously fabulous, or other credulous writers have made bold to deliver, I am so far from pretending to afford them an explication, that I do not allow them the least degree of assent.

THIS premised, let us consider the chief difficulties themselves; among which, I doubt not, but it will be objected, that it is not credible, that the mineral substances, wherewith our hypothesis would have gems to be impregnated, should have any medical operation at all on the human body, in regard that they are so locked up, that they can communicate nothing to it, especially being indigestible and unconquerable by so small a heat, as that of the stomach and other parts of the body.

BUT to this specious objection I have several things to return by way of answer. And first of all, had there yet never been any actual trial made, whereby to know, whether a gem be capable of having any medical virtues, I confess I should find probability enough in the objection to suspend my judgment, till experience should determine the question. But since upon the very credible testimony of eminent physicians and patients themselves of my own acquaintance, I find much less cause to disbelieve, than to assent to some matters of fact about the operations of gems; and since such matters of fact do strongly argue, in the general, that a precious stone may have medical virtues; I think the objection, as it is proposed in general, is sufficiently enervated by such particular instances, and ought not to keep us from believing upon experience the possibility of the thing denied; especially since there are other things besides, that may be alledged in favour of our hypothesis.

FOR it may be considered in the next place, that vigorous load-stones emit copious and very plentifully effluvia; and yet, besides that ordinary magnets are usually a very hard  
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sort of stones, I have met with some load-stones much harder than ordinary ones, and possibly than divers gems. And it is farther considerable, that there are load-stones (some of which I can shew you) which do not only work upon iron, and other magnetical bodies, but have a manifest and inconvenient operation upon human bodies, by being worn in men's pockets, or long held in their hands; as those, that have resented such operations themselves, and observed them in others, have complained to me; which I might confirm by some analogous observations, if I had time to relate them.

BUT now I proceed to observe, that among transparent pebbles, some of which, you know, are, by being barely well cut and set, made to counterfeit diamonds, I have found several, that may be brought, in a trice, to emit copious, and even strongly-scented steams. And if you allow the opinion of the generality of modern philosophers, who ascribe electrical attractions to the effluvia of bodies excited by rubbing, you will, I presume, allow me to infer, that very light alterations may suffice to procure expirations, even from transparent gems: many of which are electrical, and so are the hardest of them, diamonds themselves; one of which I keep by me, that, upon a little friction, attracts vigorously enough to be wondered at by the spectators.

AND as to that part of the objection I am answering, which contends, that gems are not to be digested or conquered by the heat of the stomach; I will not stay to examine, whether, and how far, the digestion of things in the stomach be to be ascribed to heat, contenting myself to say at present, that, to make the objection valid, it should be first proved, that such bodies cannot have any operation upon the human body as pass thorough it, without any sensible change of bulk, figure, &c. as gems, that are swallowed down, are supposed to do. For we know, that some chymists make bullets of the regulus of antimony (which we also have made, and observed something odd about them) *pillulae perpetuae*, because, when they have performed their operation in the body, and have been ejected with the excrements, they are by some more thrifty, than cleanly persons, washed, and employed again and again to the former purposes. Nor do we know, what analogy there may be between some juices in the body, and some of the mineral substances that impregnate gems with their virtues.

FOR, though the *oculus mundi* be reckoned, by classic authors, among the rare gems (as indeed good ones may be justly accounted rarities) yet, if one of the best sort be but a while kept in common water, it will, as experience assures me, receive an alteration obvious to the eye. I might here alledge the concurrent authority of many, and the common practice of most physicians, who, in their publick dispensatories, as well as private prescriptions, ordain the fragments of precious stones to be taken inwardly, upon the score of the cordial, and other virtues they ascribe to them. But I shall rather make use of less questioned arguments, and, without insisting on the manifest operation, that the juices of the body have not only on the chalybeate preparations, where the metal is presumed to be opened, but upon crude steel itself; or urging the examples of Lazarus *vitri-vorax*, or the devourers of stones, as being rare *idorisuxorales*; I shall proceed to acquaint you, that with a faint liquor, distilled from a vegetable substance, as temperately qualified, and as plentifully eaten as bread, I have obtained, and that without heat, from divers hard bodies, and amongst them, from a transparent sort of gems, a manifest tincture. And whether some juices of the body, assisted by the natural heat of it, may not, in reference to some gems, serve for extracting menstruums, though it may well be more than either I, or the objectors, certainly know, yet the instance, I come from alledging, favours our hypothesis more than theirs.

AND even the natural heat of a human stomach, nay, perhaps, the outward parts of the body, may be able, though not to digest precious stones, yet to solicit some of their

their virtues; since I am sure, it makes a sensible alteration in the hardest sort of them. For I have a diamond, whose electrical faculty may be excited not only by rubbing, but, without it, by a languid degree of adventitious heat. And I have had in my keeping a diamond, which, by water made a little more than lukewarm, I could bring to shine in the dark.

*Object.* If it be further alledged, that, though some virtues may be conceded to gems upon the account of the minerals that impregnate them, yet it will be no way likely, that their virtues should be so various and great, as even the modefter sort of authors pretend: if this, I say, be alledged, I shall readily acknowledge, that I do not think others, or myself, obliged to believe all the strange things, that even some learned writers do sometimes ascribe to gems: and if any man will think, that some of them are fabulous, and more of them hyperbolical, he may sooner find me his associate, than his adversary in that point. For the rarity of transparent gems, their lustre, and the great value, which their scarceness, and men's folly sets upon them, imboldens some to say, and inclines others to believe, that such rare and noble productions of nature must be endowed with proportionable, and consequently, with extraordinary qualities.

But this being freely granted, I answer to the objection; first, that it is not improbable, that there may be in the earth a much greater variety of minerals dissoluble by the subterranean menstruums, and capable of concoagulation with petrescent juices, than authors have yet taken notice of: to which conjecture divers subterranean productions, that I have met with, do strongly incline me. And from the number and various mixtures of these may proceed, not only a great variety of operative particles in precious stones, but a high degree of energy in some of them.

AND next I consider, that the efficacy of those mineral tinctures or solutions that are already known to us, and may be concoagulated with the petrescent juice, may be reasonably presumed to be much greater in some gems, whereof they became ingredients, whilst they were (as chymists speak) *in solutis principiis*, than may be expected in our shops, or laboratories, from the vulgar solutions of the same metals or minerals, after they have, by vehement fires, been reduced into gold, or silver, or lead, or antimony, &c. For whereas, in these vehement fusions, requisite to bring metalline or other ores into such substances, the volatile and spirituous parts are wont to be driven away, and the remaining body becomes more hard and compact, and has his virtues, as it were, locked up; in the state of fluidity those subtle and efficacious parts are preserved, and united to the other ingredients of the gems, whence some emanations of them may be easily enough drawn out; as in the instance I not long since mentioned, of the easy eduction of strongly-scented streams from pebbles so hard, that I found them more disposed to strike fire, than flints themselves, that are used in guns. And from the greater or less plenty, and natural activity of the impregnating particles in this or that gem, may, probably, be deduced the difference in colour of some, and in virtue of other stones of the same denomination: of which we have, in a learned writer or two, eminent examples given us of the great virtue of some, and the inefficacy of other, that experience has discovered, among those stones that go under the title of lapis nephriticus. For, though they be not properly transparent gems, yet the analogy betwixt them, and those that are, seems sufficient to warrant the mentioning of them on this occasion.

AND here we may subjoin two things, in favour of both the foregoing answers: the first, that for aught we know, the petrescent juices themselves may have all that is requisite to make them such, and yet have distinct natures, and be endowed with peculiar qualities, abstracting from those which they acquire upon the score of their coalitions  
with

See *Unce-  
rus de ne-  
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with adventitious liquors. This I cannot stay to make probable by the differences I have observed in petrescent fluids, and therefore I hasten to the second.

THE next thing which I would represent, is, that having observed petrific liquors, or spirits, to pervade and give a high degree of hardness to bodies that chanced to lie within their reach, though one would have thought them sufficiently indisposed to receive such an induration; I see no absurdity in supposing, that, sometimes, such a liquor may invade, permeate, and subdue transparent minerals, abounding in saline, sulphureous, and bituminous particles; which consequently being duly excited, may be made to emit their more subtle and more active parts. And as I have cause to think, that subterranean fires and menstrooms do divers times make various compositions and decompositions in the earth (as it were not hard for me to shew, if I had leisure); so it is not impossible but that the spirit we have been speaking of, supervening, may mingle itself with such bodies, and petrify them, together with itself, into gems. On which occasion, I remember, that I have had salt, made by nature in the bowels of the earth, just like that which chymists compound by art on the surface of it. And I have sometimes made, by an easy operation, and a moderate degree of fire, a certain composition of volatile particles of salt and sulphurs (some of which I have yet by me) which, after distillation, did, in a fluid medium, shoot into crystals transparent, and more curiously figured, than I have seen divers natural gems to be. So that, if either beneath, or upon the surface of the earth, such kind of substance happen to be pervaded and subdued, by a clear petrifying liquor; we may well presume, that the resulting concretions may be indued with qualities, as well uncommon for the kind, as considerable for the degree.

*Object.* If it be yet objected, that it is very unlikely that gems should part with any effluvia or portions of themselves, since they lose not of their weight, and some of them are very little heavier than crystal itself, and consequently are not like to have much adventitious substance to part with; I might leave the answering of one part of the objection to physicians and chymists, who teach, that the antimonial glass and cup imbue wine and other liquors with a strong emetic quality, without any sensible loss of weight. But having elsewhere spoken of those things, I shall rather here demand, whether the objectors have tried the truth of what their argument supposes by any way sufficiently accurate? For I much doubt, that that has neither been attempted, nor would be found easy to be performed. And till due trial be made, let me represent, that though they will not allow common water to be a menstruum fit to draw any thing rich, from such a body as mercury, which is wont to mock the chymists aqua fortis and aqua regis; yet both *Helmont* and others inform us, that mercury kept for a day or two in common water, or boiled a while in it, though it be taken out without any sensible diminution of weight, or bulk, will have imbued a considerable quantity of water with a virtue of killing worms; for which purpose it is much used, and often with good success, in a great hospital in *London*, as the chief physician of it (a very judicious and experienced man) has more than once informed me.

AND as for the lightness that is objected against some gems, besides that it may safely be granted, that, *ceteris paribus*, such may have fewer or more languid virtues than others of the same kind; it may also be answered, that the adventitious substance that impregnates the petrescent juice, may be of so small specifick gravity, as not to make the gem at all heavier in specie than crystal itself. For this (as we have formerly observed) being about two times and a half heavier than common water of the same bulk, I have hydrostatically found, that divers salts and some other mineral substances are of less specifick gravity; and consequently, if they were concoagulated with the petrescent juice, that hardens into crystal, need not increase the ponderousness of it, and yet may

imbue it with considerable virtues; nor is it necessary (to add that *in transitu* on this occasion) that, not to alter even the colourlessness of crystal, or the colour of another gem, the adventitious substance should be purely saline; for I have divers times made bodies, which though transparent and colourless like crystal, and sometimes curiously and regularly figured, were yet of a compounded nature, and particularly abounded with an easily separable and strongly-scented sulphur. But to give yet a farther and more direct answer to the objection; I shall add, that though when a gem has much more specific gravity than crystal, or will suffer an adventitious mineral to be separated from it, it is a very probable argument that the petrescent juice is that body compounded with an adventitious substance; yet it will not necessarily follow, that, when neither of these signs appear, the gem is quite devoid of any such substance. For (according to what I elsewhere declare) the petrescent liquor it mainly consists of, may be impregnated, not with the grosser substance, but with the finer and more spirituous part of the mineral, without having the specific gravity sensibly encreased. Of which I remember I shewed a notable instance to some curious persons, at a mineral spring, which many were then drinking of by the advice of learned physicians for several diseases. For, though this water, both by its inky taste, by its blacking the excrements of those that drank it, and by other signs appeared to participate richly enough of iron; yet the ferruginous particles it abounded with, were so light and spirituous, that not only they would, as I tried, be easily lost, if the liquor were kept too negligently stopped; but when I came, whilst the spirits were yet there (it being but newly taken from the spring itself) to examine it hydrostatically with very good scales and much diligence, I convinced the virtuosi that assisted, that this ferruginous water was very little, if at all heavier in specie than other water, which was brought as common water to be compared with it, and examined with the same scales, and after the same manner.

AND now, if you recall to mind what I have elsewhere said, partly of the atmospheres of solid bodies, and partly of the great efficacy of effluvioms; I hope you will not think it absurd to conjecture, both, that some precious stones may have medical virtues, and, that divers of these may be ascribed to the mineral substances, whereof they participate or consist; and especially to those which are best fitted to exert their powers, by the copious effluxions of their more agile and subtle parts.

AND by this time it may be seasonable to tell you, that though what I have hitherto discoursed do chiefly belong to transparent gems; yet divers of the things already delivered may, with no great alteration, be applied to opacous gems; of which I shall speak much more briefly, not only for the reason just now given, but because, if we have shewn, as I hope we have, that even diaphanous gems may be endowed with virtues by the mineral substances they contain, or are in part made up of; the arguments will hold more strongly as to opacous gems; both because these are, for the most part, much less hard than the others, and because it is far more easy to shew, by their specific gravity, and the compoundedness of divers of them, that the dark ones, than it is that the clear ones may partly, and sometimes plentifully consist of mineral substances embodied with and hardened by petrescent juices or petrific spirits.

IN favour of this doctrine, I shall endeavour, in the first place, to shew that what has been delivered is possible; and afterwards set down some particulars to make it very probable.

THE first part of my task might be easily performed, or, perhaps, would be needless, if I were sure you had no need to be told of any thing I have written about lapidescent juices. But for greater security I shall, in this place, briefly intimate, that among the kinds of those liquors I have observed a sort that is of so fine a substance, and yet of so petrifying



petrifying a virtue, that it will penetrate and petrify bodies of very differing kinds, and yet scarce, if at all, visibly encrease their bulk, or change their shape or colour. To which purpose, I remember, that I have seen divers animal and vegetable substances so petrified, as scarce at all to be taken notice of, by their appearance to have been altered by the operation of the petrescent liquor. I have, with pleasure, seen a thin cream-cheese turned into stone, where the size, shape, and colour, even of the wrinkles, and the blueish mold (which, it seems, it began to have, when the liquor invaded it) were so well preserved, that an hungry man would not have scrupled to have fallen upon it for a good bit. And as for the hardness that this petrescent juice can give to the body that it penetrates, I shall now only remind you of what I lately told you; that I have had, and, I think, yet have, in another place, a pretty quantity of wood petrified in *England*, which retaining its former figure, and grain, and scarce at all visibly increased in bulk, was so very hard, that I could make impressions with it upon iron and glass itself, and make it strike fire like an excellent flint. To which I shall here add, that the stony parts did not suffer the wood which they had penetrated to be reduced in the fire, either to ashes or charcoal. And I have by me a lump of mineral substances, wherein a petrescent liquor that fills the large intervals between them, is transparent enough and harder than most stones, as far as we could guess by some trial of it made by a skilful engraver of gems.

AND to these instances might be added many others, if it did not by these few sufficiently appear, that petrifick agents may insinuate themselves into the pores of various bodies, and turn them into stone, without otherwise destroying their pristine nature, or so much as their former figure.

WHEREFORE, having in general shewn our hypothesis to be possible, we may now descend to four or five particular arguments, that it is hoped may help to render it very probable. And these I shall fetch, partly from the great specifick gravity of divers opacous and medicinal stones; partly from the fitness of our hypothesis to render a reason of divers phænomena relating thereunto, some of them scarce at all, and others much less probably to be accounted for without it; partly from the metalline substances to be manifestly separated or obtained from the stones we are treating of; and partly from the nature of the bodies whereof medicinal stones seem to be compounded.

*Arg. I.* THAT the specifick gravity of divers opacous stones, whereunto medicinal properties are ascribed, is very considerable, is a truth, which if those that have written of such concretions had been versed in hydrostaticks, and had had the curiosity to examine them that way, they might have easily discovered, as will quickly appear by particular examples; before the mention whereof, it will be fit for me to take notice to you, that considering with myself, that white marble is generally allowed to be a pure and solid stone, and, upon the score of its whiteness, is likelier than most others to be free from mineral mixtures; I thought I might at least as well pitch upon that as on any other, for the standard of the specifick gravity of opacous stones, as they are merely such. And accordingly having weighed a piece of white marble in air and water, I found it to be in weight to an equal bulk of that liquor very near  $2\frac{7}{10}$  to 1, or, that the proportion with very little error may be the better remembered, as two and seven tenths to one. And to make trial in a stone uncoloured, but, because harder, supposed to be of a closer texture, we examined a fine white pebble, which we found to be to an equal magnitude of water, as two and above six tenths to one. This being determined, it was not difficult for me to think, both that divers bodies that commonly passed for mere stones, are more ponderous than white marble of the same bulk; and that if there were any such great surplusage of specifick weight, as I guessed, many will be

found to have above that of marble, it might proceed from some metalline body, though not visibly, yet really, and perhaps plentifully mingled with the petrescent matter of these stones. The latter part of this conjecture will hereafter be confirmed in the third argument; which makes it unnecessary for me to give you now of the former more than a few instances: which I shall soon dispatch, by telling you, that I quickly found by weighing the following minerals, first in the air, and then in the water, that a blood-stone (bought at the druggist's) was in weight to water of the same bulk at  $5 \frac{7}{10}$  to 1; the load-stone I then tried (for all are not equally heavy in specie) as 4 and  $\frac{2}{3}$ , to 1; lapis calaminaris, used for rheums in the eyes, and to turn copper into brals, as  $4 \frac{7}{10}$  to 1; lapis tutiæ, as they call it, which is also much employed in rheumatick eyes, as very near 5 to 1.

BUT here I must advertise you, that I have not found the proportion of each of these bodies and water to be any thing near constantly the same, but sometimes to differ very much in particular stones of the same kind; which agrees very well with our hypothesis. For, according to that, those particular stones that happen to partake more plentifully of mineral substances, heavier in specie than stone, as such needs to be, ought to be more ponderous than others of the same kind, that are not so qualified; I said, heavier in specie than a stone, as such need to be, because there are substances that are reckoned among minerals, and are capable of endowing the stony matter wherewith they are coagulated with medical virtues, and yet those substances may make the stone, or aggregate whereof they are made, not to be heavier, but lighter in specie. From jet, which in some parts of *Europe* being found in quarries of mines, is indeed a fossil, which is wont to be reckoned among stones, and by many worn as a gem, I obtained no inconsiderable proportion of oil; and having weighed choice jet itself in water, I found it to be, bulk for bulk, to that liquor, but as  $\frac{2}{3}$  to 1. And there are some other fossils, hard as stone and polishable as marble, from which I have, by distillation, obtained two kinds of oil, whereof one was lighter than common water; which shews, that even bituminous and light substances may be ingredients of a stone; and that salts, which are most of them less heavy in specie than white marble, may plentifully concur to the making up of stones, I shall have occasion to manifest at the close of this discourse, by those stones whereof we in *England* use to make vitriol. The foregoing reflection I have here touched upon, because I would intimate to you, that stones that are lighter in specie than white marble, may be compounded of fossils, whence they may derive peculiar qualities, at the same time, when I tell you, that in my opinion such stones as are considerably more heavy in specie than marble, may afford us a strong presumption of their owing their gravity to the mixture of metalline or mineral substances. And this may suffice for our first argument.

*Arg. II.* THE next shall be taken from the consideration of some phenomena (relating to medicinal stones) which agree very well with our hypothesis, and will scarcely be very well explicated without it.

AND, first, as to transparent gems themselves, I have learned by inquiry of travellers that have visited those parts of the *East-Indies*, where they grow, that sometimes one sort of gems, sometimes another, and sometimes also diamonds themselves are found included, in the rocks where they are digged for, or in the midst of hard loose stones, which must be broken in pieces, to take out the diamond or other inclosed gem; which phenomenon will be hard to be accounted for, unless by our hypothesis; according to which it may rationally be supposed, that the gem was first formed either in earth, or some other soft and easily permeable substance, which being afterwards pervaded by some petrifick juice or spirit, was turned into rock or loose stones, according as the earth and other ambient



bient matter chanced to be an entire and coherent mass, or divided into clods and other portions. And I remember, that the governor of an American colony, having sent me, among other rarities dugged up in his country, an odd kind of mineral that seemed more ponderous than at first sight it promised, I had the curiosity to break it, and found in it, here and there, several gems, which by their figuration and some other circumstances, were concluded to have been formed there, before the ambient mineral had obtained the nature it then appeared to be of. And in opacous stones, it may hence happen, that a great lump of medicinal earth may be invaded and petrified after the newly mentioned manner; so that it may not be thought incredible that some of these medicinal stones should be very large in comparison of others: as I remember that an ingenious physician told me of a spleen-stone, as they call them, in the hands of an acquaintance of his (where I might have seen it, if my occasions had permitted) amounting to about four-score pounds weight. And on this occasion, I also remember, that even in a medicinal stone much harder and heavier than marble, and whereof I have seen lumps far greater than I could lift; I remember, I say, that having had the curiosity to cause a pretty big piece, violently broken off from the mass whereto it belonged, to be sawn asunder, that I might consider the internal textures, as far as it was visible; I found several empty cavities of differing sizes and figures in the solid substance of the stone (which I think I have not yet lost); which seems to argue, that this compact and ponderous body was made of a stony nature, by the supervening of some petrescent liquor or spirit upon porous earth, or some other consistent substance. For if it had been a mere liquor wherein those cavities must have been so many aerial bubbles, it is not like that some of them should have such irregular shapes, and that all should have continued, without emerging to the top.

SECONDLY, our hypothesis will also help to render a reason of what seems exceeding difficult to be explicated; namely, how some gems, that seem to be entire stones, are in part of one colour, and in that, which is contiguous to it, of a quite differing; of which sort we have the sardonyx, and some other opacous gems. And I have observed the like, though very rarely, in diaphanous ones. For, according to our hypothesis, it may be said, that a portion of matter, imbued with one of the tinctures of the parti-coloured gem, was first formed, and afterwards, some petrescent juice, endowed with another colour, came to settle contiguously to it, and so by accretion made up one stone with it. I might illustrate this by telling you, that though fire do make a far greater agitation of bodies melted by it, than need be supposed in cold petrescent liquors, yet I have found in making artificial gems, that by some mischance or error in the operation, the mineral pigment has richly tinged one part of the transparent mass, without at all imparting that colour to the very next part to it; so that if I should shew one of those I have yet by me, you would judge it to consist of two differing gems subtly glewed or fastened together, unless you should in vain try, as others have done, to discover by the eye or otherwise, some naked commissure, which may keep those so differently coloured bodies from making up one entire mass.

BUT let us leave these artificial gems, and add to what I was saying about our natural ones, that the union of parts in these resulting stones (if I may so call them) I was speaking of before, might be the more perfect, if the supervening matter found not the first formed stone to have attained to its full induration; though, for aught I know, even in this case, the apposition may be so close, and the two matters so near of kin, that both may pass for one stone, and be polished both together, without any blemishing discontinuity of surface at those parts, where one would expect commissures. For I have by me a lump, wherein there plainly appear stones of colours very different from each other,  
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that were once distinct and incoherent; but by some petrescent liquor have had all their intervals so exquisitely filled up, that neither the touch, nor the artificers tool, the lump being now sawn asunder, discovered any commissures; but the whole mass bears an uniform polish, and is harder than divers gems that are worn in rings, readily enough striking fire with a steel. And to confirm this the more, I shall add, that in a place where a prying person of my acquaintance lighted on this portion of petrified matter, he found not only other lumps, but divers loose stones, that seemed altogether of the same nature with those, that by the supervention of the petrescent liquor were united into stony masses. I have also had a curious agate so formed, that it seemed highly probable that the opacous parts of its matter had been some thin, but not altogether contiguous beds of fine clay, or earth, lying almost parallel to each other (but not to the horizon) which by some petrescent liquor that chanced to settle there, was reduced to coagulate with it into a partly opacous and partly diaphanous stone. And of such clays or mineral earths, I have sometimes with pleasure observed more than one or two, which, though distinct, and perhaps of differing colours, were so very thin, that the thickness of them all did scarce exceed an inch, nor did they always lie flat or horizontally, but in differing postures, both in reference to the horizon, and one another, and now and then the exterior ones did successively almost surround the interior; and of these thin couches or layers of earth, I remember, I have observed a considerable number within a very small compass of ground. I must not in this place stay to shew how probable it is, that much after the same way may be explicated the production of divers other gems besides agates, as chalcidians and jaspers, which are for the most part opacous, but oftentimes have some parts that are not so. But I am content, before I go further, to mind you, on this occasion, of what I elsewhere deliver, that by purposely calcining, without breaking, some of these stones, whose greater part was diaphanous, I found, that the transparent parts turned white; and that some of the thin layers or couches of mineral earth had retained their colour, as well as position, and had it much heightened; so that one of these layers, after calcination, was of a very rich and permanent red. And this difference of colours I observed not only in layers, but in the specks and irregularly shaped clouds, if I may so call them, of other colours, as greenish, blueish, &c. I might here add, that I have found shining marchasites, not only in other solid stones, but in marbles, as also flints themselves, inclosed in great masses of marble, and likewise wood; in strong stones employed to build a wall, and shells, at least as was judged by their shapes and sizes; in a great mass of stone that I met with almost on the top of a hill remote from the sea, together with divers other such phenomena, which I think may probably be accounted for by our hypothesis, and scarce without it. But being willing to dispatch this discourse, and unwilling to intrench upon the discourse of the effects of the petrescent juice, to which the consideration of these and divers other phenomena, to be met with about the generation of stones and petrified bodies, especially in wombs or molds, more properly belongs: I shall in this place only point back to one observation, and answer one objection; because both of them are pertinent to our present discourse.

THE observation is this: that even in transparent gems, and which is more, of the self-same species, I have sometimes taken notice of such an aggeneration or accretion of stones, to one another, as argues their having been produced at several times. For proof of this, I need no more than refer you to what I have not long since related about those Cornish diamonds, wherein, sometimes, a lesser stone, though geometrically shaped, was found in good part inclosed in a greater, as well as in part also extant above it. Whence I argued, that the production of this aggregate of two crystalline bodies was

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not made all at once, but successively, and that the lesser was first formed, which I shall now confirm by this consideration. That if the greater stone had been first hardened, the matter of the lesser must only have exteriorly stuck to it, and been, as it were, imbossed upon it; but could not have made itself, in the substance of the greater, a bed or mold, especially of such a geometrical figure as itself had not yet received.

And though this successive generation of the parts of seemingly entire gems, may appear to you somewhat new and strange; yet, that its fitness and requisiteness to explain the foregoing phenomena, and others to be hereafter mentioned, may the more recommend it to you; I shall add, that, perhaps, you may be assisted to conceive, if not invited to admit it, by a mechanical illustration. For we see, in divers chymical solutions, as of salts, and other bodies, that there are certain stages, or periods, of coagulation; so that, when such a quantity of the superfluous moisture is exhaled, especially upon any considerable refrigeration or other favourable circumstance, those particles that are most disposed to coagulation, will convene, and shoot into crystals; after which, no more will do so, till a farther and more considerable evaporation of the water or other menstruum, be made; upon which will ensue a new crystallization of the parts. And I can shew you the productions of a metalline but uncommon solution, that I so made in an appropriated liquor; that the first shooting afforded me a layer, or bed of curiously figured crystals, and the following another layer of fine crystalline bodies, that have fastened themselves to the former, but differ notably from them, both in shape and posture. And in this experiment, the dissolved body was but one, as the menstruum but one; but if there be a diversity of nature in the liquors that make up a menstruum, or in the bodies that are dissolved in it, some of the corpuscles may convene either apart with those of the same nature, or mingle with those of a differing nature, but yet at the same time; and so make up crystals of a compounded nature; and some of them may convene with homogeneous particles, but at differing times; and so miss of such uniformity as might else appear in their concretions. Which may be illustrated by what I have elsewhere related concerning the crystallizations of salt-petre and sea-salt, dissolved together in ordinary water; where, most commonly, grains of salt of resulting figures are produced; and also a considerable part of the sea-salt coagulates in the form of imperfect cubes, about the bottom, before the nitrous corpuscles shoot into crystals of their own almost prismatical shape. And I might further add, that it matters not, whether the superfluous water be wasted by exhalation, or by being drained by a body fit to soak it up; as we have had occasion to observe in accelerating the crystallization of some bodies, where I was not willing to employ the heat of the fire, by placing underneath the solution, dried earth, or some other porous and soaking body.

With some analogy to such instances as these, we may conceive, that where there are petrescent liquors, mingled with common water, there may, by divers accidents, and particularly an hot summer, a sufficient discharge be made of the superfluous moisture, to make the more disposed parts of the petrescent liquor to coagulate; and afterwards the coagulation may be suspended, either by the supervening of a colder season, as winter; or even in summer itself, by a plentiful rain, or the effect of it, a land-flood, which might check the progress of coalitions by over much diluting the liquor, that might else have turned into stone. Not to mention, that trial hath assured me, that there are bodies, and those of very differing kinds, which will, in tract of time, especially if their coalition be furthered by cold weather, coagulate, after they have long remained in a fluid form, though the water, or other menstruum, by being inclosed in stopped glasses, be kept from wasting. And since the earth harbours differing kinds of these liquors, as I have elsewhere shewn, and divers of them may be copiously impregnated,

nated, some of them with one sort of mineral, and some with another; we may conceive, that they may have distinct periods for their respective coalitions, and yet may stick close to one another; in regard that, though in our chymical crystallizations the artists are wont to take out of the vessel what shoots the first time, before they make a fresh exhalation of the water for a new crystallization, and by this means have the coagulated bodies that they obtain at one time more uniformly shaped; yet in the hollow receptacles that the earth affords to petrescent liquors, the vessels continuing the same from first to last, the uniformity of the bodies produced by coalitions, made at several times, must be less regular; and the manifest accretions or aggregates of coalescent bodies must, in all likelihood, be more frequent. And accordingly having suffered the exhaling of some liquors to be continued in the same vessel, I had coalitions of very differing bodies at the bottom.

WHAT I was not long since saying, makes me remember, that, in order to a satisfaction, which the event gave me, of the conjectures I had about the successive concretions of some solid fire-stones, that were not suspected to be other than entire and uniform masses, I caused two or three, that I thought likely, and of very different sizes and shapes, and brought from distant places, to be warily broken; which trial gave me the pleasure of observing, that the internal texture of the least of these minerals, which was almost spherical, was very differing from that of the more internal part of the substance of the stone; and that in the other and greatest mineral, there was a little globulous stone that manifestly was not of the same piece with the invironing mass, differing from it not only in texture, but here and there by a discernible commissure; though in most places, their adhesion was so strict, that we could not make any separation of the two minerals by the help of this commissure. The greatest part of this double fire-stone I keep by me, and shall say nothing of what I further observed in it, having mentioned what I said already but upon the by.

I MIGHT add, that in some circumstances, even in close vessels, and therefore without any manifest exhalation of the water, or other menstruum, and, sometimes, where the dissolved body was homogeneous, I have, in process of time, had coagulations, where the last formed crystals seemed plainly to have been generated by way of accretion to the first.

*Difficulty.* Having now done with my observation, I shall endeavour to clear a grand difficulty, which, I foresee, may be objected against our hypothesis; namely, that these aggenerations, if I may so call them, of medicinal and other stones, are sometimes found in places where there are no petrifying springs, and perhaps no springs or other waters at all; nay, little or nothing but quarries or other masses of stone.

BUT to this I answer, first, that if we admit of the relations that I elsewhere mention out of approved authors, concerning men and beasts turned into stone by a petrifying spirit that suddenly invaded them, it will not be absolutely necessary that there should be any petrescent springs, or other-like water, to produce such minerals as we are now discoursing of.

SECONDLY, for aught has yet been shewn to the contrary, we may suppose, that rain-water does sometimes bring along with it such petrifying particles as may serve our turn. In confirmation whereof, I shall add, that having, of a learned and judicious person, enquired after divers particulars relating to a famous bath, by him visited in *Hungary*, whose water abounds very much with petrescent particles, over which there is very high building erected, I learned by his answers, among other remarkable things, that to the roof, or upper part of this tall structure, there were fastened many long stony concretions (like those wont to be employed to adorn grottoes) which he affirmed to be, from



time to time generated there; not, as I at first suspected, by the dashing up of any drops of water (which he averred could not reach any thing near so high) but by the copious petrific steams that being there checked in their ascent, did, according to their natural propensity, coagulate into stone. Whether this relation may warrant me to guess, that, in some places, stones may be generated without the help either of rain, or springs, by the ascent of petrific particles, in the form of exhalations, from some lower parts of the earth; which exhalations, suffering the lighter steams that accompanied them to exhale, may operate upon some disposed materials that they find in their way, and turn them into stone; whether, I say, this narrative may well suggest this conjecture, I shall not now stay to examine, though the earthy and sometimes sulphureous sediments that have been observed at the bottom of rain-waters, suffered to settle in clean vessels, may seem to favour it; and though also I might illustrate it by what I observed in a bottle of distilled liquor, whereof no part would naturally ascend in a dry form; for having kept this phial well stopped in a safe and quiet place for a year or two, I observed, that the ascending steams had quite pervaded the cork, and had formed, at the top of it, numerous whitish striae, slender, but of a length that surprized me.

THIRDLY, there is no necessity that in all soils where petrific waters are to be met with, there should be petrifying springs, at least above ground. For I have caused to be digged store of figured and transparent stones in a certain earth that lay upon the upper part of a rock, and seemed to be a very dry soil; perhaps you will allow me to tell you, that I have, by pouring a solution of stony striae, made with spirit of verdigris, on a convenient quantity of bolus Armenus, and suffering the soft mixture to remain in a glass in the open air, till the superfluous moisture was exhaled; I have, I say, by this means, imitated in a little, what I have been now relating, and found small, but untinged and figured crystals dispersed through the little cavities of the red earth. But it will be more considerable to our present purpose to add, that the fairest and hardest petrifying wood that I ever had, or tried, was taken up by an ingenious person I employed in a plot of sandy ground, where he could not find any petrifying, or so much as any other spring. To which I know not whether I should add, that supposing the ground to have been once moistened with a lapidescent liquor, whether brought thither by springs or any other way; one may, in our hypothesis, well enough account for this difficult phenomenon, that now and then, not only in the surface of the ground, and perhaps upon rocks themselves, there are found aggregates of figured stones that seem to grow upwards, as it were, from a root; which much puzzle men to know how they came there, and may incline them to their opinion, who ascribe vegetations to stones. But to this may be answered, that many of the concretions we are speaking of may have been formed in wombs, that lay, though not deep, yet under ground, or in shallow cavities in the surface of it; and that after their formation, the looser earth that surrounded them, may have been washed off, by rains blown off by winds, or otherwise removed, leaving behind them these stones that adhered firmly to a solid body. Besides, if I had time, I think it were possible for me to shew, that stony concretions might be produced by the mechanical action of the air, upon the stony particles that successively apply themselves to the matter, that first begins to coagulate, when they are ready to be forsaken by the moisture that accompanied those particles, and was necessary to their due application to the casual rudiments (which pass for roots) in imitation whereof, I have, more than once, obtained both from saline and stony solutions dry tufts of prettily figured, and diaphanous or white, but very slender striae, if I may so call them, that seemed to grow out of the solid glass, and made men wonder how they came thither, no water, or other liquor appearing near them.

FOURTHLY, It may very well happen that the petrescent liquor may be so mingled and diluted with ordinary water, as not to be distinguished from it by the generality of men, nor to be capable of disclosing itself by its effects, till either by the copious exhalation of the common water, or by some peculiar advantages it has to operate upon bodies, it has opportunity to discover itself. On which occasion I shall add, that there is a lake in the north of *Ireland*, wherein I could never hear but that fishes lived as well as in other lakes, and yet there are some rocks near the bottom of it, to which there fasten themselves divers masses and other pieces of a finely figured substance, and transparent as crystal; of which an eminent person, the chief owner of the lake, presented me with some, and promised me more. Now, if we suppose that either by springs of petrescent water, or by rains, or by subterranean steams, or otherwise, waters resting in any hollow place, though upon the top of rocks and mountains, shall be sufficiently impregnated with petrifick particles; and that afterwards, in process of time, the merely aqueous parts shall be, by degrees, by the heat of the sun, the soaking of the grounds, the winds, or the continual action of the air, brought to exhale away in the form of vapours, the petrifick particles, which are not so volatile, will turn the soil beneath them, and on the sides of them, as far as the sphere of their activity reaches, into stone harder or softer, of this or that kind, according to the particular nature of the petrescent liquors, and the structure and other dispositions of the soil they invade; in which soil, if there chance to be lodged bodies heterogeneous to it, whether vegetable substances, as roots, pieces of wood, gums, &c. of the whole bodies of animals, as toads, frogs, serpents, fishes, &c. or their parts, as shells, bones, &c. or minerals of an open texture, as boles, unripe ores; or else gems or stones of another kind already formed; any of these things, or any other that shall chance to be lodged there, must be found either petrified, or inclosed in stone, when this changed and hardened soil shall come to be broken up. Nor is at all necessary, that this petrification of the extraneous bodies, and of the soil or bed, be made at once; for it may well be made successively at several times, according as some parts of the petrescent juice happen to be more copious and penetrant, and consequently more fit to be soaked in further than other. For, as the porousness happens to be greater in one part of the soil, than in another; or, as the texture and disposition of particular bodies, lodged in the earth, gives advantage to the petrifick particles to work on some of them, sooner, or in a differing manner, than in others; so the induration of the pervaded matters may be very unequally made in point of time, as well as in other circumstances. So that (to omit many other things explicable by it) we may, from what hath been already delivered, conceive how it may happen that medical stones of very differing colours, consistencies, and operations (of which I have several by me, that I had from the same mineral mass) may be generated, and seem entire bodies, though (as in some that I found) the difference is great, that so one part of the medical stone is dark, heavy, and opacous, and the other much lighter, transparent, and quite otherwise coloured. And upon the same principle may be explained, what I lately mentioned to you about the finding of diamonds inclosed in loose stones and even in rocks; of which we have credible testimony, which seems not more strange to me than a stone, which I have by me, which being a kind of pebble, contains in it a perfectly shaped serpent, coiled up, but without a head, which appears to have been formed before the stone, in regard, that in the upper and lower parts of the solid stone there are cavities left, which together make up one cavity, just of the size and shape of the contained body; to which, as it was easy for the matter of the stone, whilst it was yet a soft body, to accommodate itself exactly; so it is scarce conceivable, how, if the pebble had been first formed, the inclosed animal, if it were one, or the matter whereof the seeming animal afterwards



afterwards was formed, should not only get in, but find a cavity so curiously shaped, and so fitted to its bulk. And that this variety was produced at several times, might be further argued from this, that the seeming serpent is plainly of another and clearer kind of stone, than that of the mould that encompasses it; and of the mould itself, one part, contiguous to the included body, is whitish, and abounds in shining grains or flakes; in both which, it differs from the other and far greater part. And now it will be time to hasten to the

FIFTH consideration, which is, that, for aught we know, in those very places, where now there is nothing to be seen but loose stones, and perhaps beds of stones themselves, that in those very places, I say, there may in times past have been petrescent liquors, whether stagnant or running. For, I elsewhere \* shew (to another purpose) that earthquakes, inundations of seas and rivers, sinkings of ground, encroachments of the land on the water, fiery eruptions, and other such accidents (some related by authentick authors, and others happening in our own times, in places, some of which I had the curiosity to see) have, among other odd effects, been able to dry or choak up pools and lakes, and to stop and quite divert the course, not only of springs, but of rivers, so as to leave no footsteps of them, where they plentifully flowed before. Upon the score of which transpositions of notable quantities of terrestrial matter, and other great changes of the structure and disposition of the soil in divers places, it may well be suspected, that the stony wombs or moulds, wherein the above-mentioned bodies were found, were heretofore, at some time or other, of a muddy or earthy nature, and were receptacles of petrescent liquors, which, at several times, turned the whole mass of the soil into stone, before the springs, or other waters, containing the petrific liquors or spirits, were quite consumed, or had their course altogether diverted. But though I could say much more to confirm and apply this, and the preceding considerations, yet having spent so much of my time already, I shall not only leave all that unsaid, but, to make some amends for having staid so long in clearing this difficulty, I shall do little more than name the two remaining arguments.

*Arg. III.* It agrees very well with what we were formerly saying (in the first argument) about the great specifick gravity of such, as the newly-mentioned stones, in comparison of that of white marble, or transparent pebbles, that it should be possible, out of those minerals, to extract some of that substance, whether metalline, or of kin to it; upon whose account, I told you, I supposed them to be so ponderous. And accordingly we have, by appropriated menstruums, obtained from the fore-mentioned bodies, and not from those only, solutions or tinctures, which, besides that, by their colour or taste, they discover themselves, did, upon their being dropped upon a solution of galls, or some other convenient liquor, or upon their being examined by other proper ways, produce such changes of colour, or such determinate phaenomena, as argued them to abound with metalline, or mineral particles, which, for the most part of them, I observed to be of a vitriolate nature; so I found, that the solution of a blood-stone, which tasted very rough upon the tongue, would, with the infusion of galls, make an inky mixture; and the like would also be made with load-stone, emery, marchasites, &c. opened with corrosive menstruums. But the solution of lapis calaminaris, which was of a golden colour, did not operate like the rest on the infusion of galls; but yet by its taste, as well as colour, sufficiently discovered itself to have copiously impregnated the menstruum. And now the mention of lapis calaminaris minds me, to take thence an instance of what I lately intimated, that there may be other ways, besides that of dis-

\* In an examen of an experiment urged for the magnetism of the earth.

solutions in proper menstruums, to shew, that some medicinal stones participate of metalline and mineral substances. For it is by melting lapis calamarinus with copper, and keeping them together for a competent while in fusion, that brasis is made; wherein the red colour of the copper is changed into a golden one, and the absolute weight (for I speak not of the specifick gravity) considerably increased. Nor is this the only mineral stone, from which I have, by a way quite differing from those I have yet mentioned, namely, with running mercury, obtained a metalline substance. And though native cinabar, used by eminent physicians both inwardly and outwardly, be looked upon by the vulgar as only a red stone; yet it is known, in the quicksilver mines of *Friuli*, and some other places where it abounds, that it is a mercurial ore, whence, by vehement fires, they distill running mercury, which we, by moderate ones, have sometimes done.

BUT here perhaps it may not be improper to tell you, that though before any admonition given men of the expediency of examining stones hydrostatically, I could not receive from others, yet I made against myself the following objection, that there are some stones, to which useful qualities are ascribed, which are either not at all heavier in specie, than is requisite for a stone, as such, to be; or so little heavier, that it is no way likely, that metals, or any such ponderous minerals, should contribute either to their productions, or their virtues.

IN answer whereunto, I thought it may be said in the first place, that our hypothesis does no way oblige us to deny, that there may be such stones. For though it ascribes the virtues of most gems, and metalline stones, to the metalline and ponderous mineral substances they partake of, yet the concession agrees very well with our doctrine; which (as will in the fourth argument be more manifested) speaks in general, when it teaches, that the virtues of stones may, in many cases, depend upon their consisting not of a pure petrescent substance, but a substance impregnated with other minerals, which, though most commonly they prove specifically heavier than the petrescent matter, as such, without being the less, but rather, in some cases, the more operative and communicative of their virtues; yet, in divers stony concretions, the adventitious ingredients may be specifically lighter than the genuine matter of the stone; as may be easily gathered from some passages of the foregoing discourse. For, not here to urge, that divers bodies, that pass for stones, do abound in particles of salt, which may be much less heavy than pure stone of the like bulk, I have observed, that some other hard fossils abound with a kind of bitumen, which, when by distillation brought to an oil, is much less heavy than a stone of the same bulk. And, as I remember, I have had some portions of such oil, that would swim even upon common water; and, lest this should be ascribed to the subtilization, the bitumen received from the fire, I will add, that having hydrostatically weighed a piece of good asphaltum, we found it to be to water of the same bulk, but as 1, and somewhat less than  $\frac{5}{7}$  to 1. Which was within a tenth of the proportion to water of a stony, though a bituminous fossil, commonly called in *England* Scots-coal. And because sulphur, as well as bitumen, is very apt (and indeed, more apt, than before trial I expected) by even a moderate heat, or attrition to diffuse its steams (usually rank-scented enough) I shall add, that there are variety of hard stones, which abound in sulphur: (witness, that in some places they obtain their common brimstone by sublimation thence) and yet having weighed a roll of brimstone in air and water, I found it to be but a fraction scarce worth mentioning above double its weight to the liquor; which shews it to be much lighter in specie than crystal itself.

AN improvement of this first answer may furnish me with the second. For hence we may argue, that it is not impossible, that the principal virtue of a light medical stone should



should be due to some mixture of a metalline, or the like ponderous substance; since, if some of the ingredients, that are plentifully mixed with the true stony matter, be of the lighter sort, though there be also some metalline, or other heavy mineral particles mingled with the same matter, yet the specific levity of the one, in comparison of this matter, may compensate the specifick gravity of the other; and they may all compose a stone, either less, or not more ponderous than white marble. On which occasion, I remember, not only that I found a blackish East-Indian flint, and likewise a black English one, to have to water not full the proportion of  $2 \frac{2}{5}$  to one, but that one of the first pieces of black marble, that I examined hydrostatically, was found, notwithstanding the darkness of its colour, to be to water of the same bulk, scarce any thing more than  $2 \frac{2}{5}$  to 1, which, you may remember, was the proportion I found between white marble and water, unless we should say, that this blackness of colour proceeded, not so much from any gross bituminous matter, imbedded with that of the stone, but from some mineral smoke that had pervaded it. And this puts me in mind of speaking something in this place about what might properly enough have been discoursed of long ago.

WHEREFORE I shall subjoin, in the third place, that it seems not impossible, that the matter, which medical stones are made of, may, before it comes to be hardened, derive various colours, and be imbued with virtues by subterranean exhalations, and other steams. This, I fear, you will think somewhat strange, and therefore I shall briefly endeavour to confirm it by the mention of two or three particulars.

THAT then many places of the lower part of the earth emit copious exhalations into the upper, and even into the air itself, I presume you will grant, and I have elsewhere proved it. That also such subterranean steams will easily mingle with liquors, and imbue them with their own qualities, may be inferred from the experiment of mixing the gas (as the Helmontians call it) or the scarce coagulable fumes of kindled and extinguished brimstone, with wine, which is thereby long preserved. And I have elsewhere mentioned, how I have incorporated this smoke with other liquors, wherein I observed its operations to be notable.

THAT beneath the surface of the earth there may be sulphureous, and other steams, that may be plentifully mixed with water, and there, in likelihood, with lapidescent liquors, I have also manifested in another \* discourse.

THAT quicksilver may be in part resolved into fumes by less fires than many of those that burn under ground, will be readily acknowledged by chymists and gilders, and is obvious in the fumigations employed in the cure of the lues venerea. And that mercury may in the bowels of the earth be so disguised, and well mixed with stony matter, as to suffer the whole concretion to pass for stone, may be observed in some kind of native cinnabar.

THAT sal armoniac, of which, in some places, there is to be dug up store, will with a moderate fire be made to ascend in form of exhalations, is vulgarly known, as to the factitious salt of that name; and I have found it to hold in the native. That common sal armoniac, sulphur, mercury, and tin, will be sublimed into a gold-like substance, that participates of most, if not of all the ingredients, may appear by the account I have elsewhere given of the way, I used, in making aurum musicum: and that even gold itself, the heaviest and fixedest of the bodies we know, may, by no great proportion of additament, and that with but a moderate fire, be made to ascend in the form of fumes, or even of flame, I have several times tried, by ways elsewhere delivered. And that

\* Of subterraneous steams.

mineral exhalations may be met with in the bowels of the earth, is witnessed by the relations of divers credible persons, conversant about minerals, that affirm themselves to testify what they write upon their own observation, to which some things, that I had seen myself, did the more incline me to give credit. And this copious ascension of mineral fumes, and even of metalline ones, may be much confirmed, not only by what is written by professed chymists, but by the learned and curious *Johannes Kentmannus*, who, in the useful catalogue of the *Misnian* fossils he had collected, amongst the pyritæ or fire-stones, reckons one, whose title is, *Punicoſus, & ab exhalatione ardenti nigro colore tinctus*; and another whose inscription is, *Coloris argenti, qui ab exhalatione viroſa colore cinereo eſt tinctus*. The ſame may be further confirmed by what I have ſomewhere met with, as related in *terminis* by the learned *Cabeus*, that he found in the territory of *Modena*.

To bring this home to our purpoſe, ſince there are mineral exhalations of very differing kinds, diſperſed in divers places under ground, and ſince there are ſeveral volatile minerals, as aſſenic, orpiment, ſandarach, &c. that are very aſtively hurtful; there may be others endowed with medical qualities, and the exhalations of ſuch minerals, either alone, or mixed with petreſcent liquors, pervading duly-diſpoſed earths, and boluſſes, and other fluid, ſoft, or open ſubſtances, before their induration, may endow them with medicinal and other qualities.

NAY, when I recall to mind the old phænomena, that I have partly obſerved, and partly received from credible teſtimony, about the coalitions, mixtures, tinctures, and the emanations, as it were, of thoſe tinctures in metalline, ſtony, and other ſoſſile concretions; I dare not peremptorily deny, but that even after ſubterranean bodies have obtained a conſiderable degree of induration, and perhaps great enough to make them paſs for ſtony ones, there may be ſubterranean ſteamſ ſubtle enough to penetrate, tinge, and otherwiſe impregnate them. Which you would think the leſs impoſſible, if you reflect upon what I juſt now related out of *Kentman*; and eſpecially, if I had time to add here, what I remember I eſewhere delivered about my trials to tinge native cryſtal with differing colours, by the fumes of volatile minerals. And that a very ſmall proportion of a metalline ſubſtance, reſolved into minute particles, may ſuffice to impart a tincture to a greater quantity of other matter duly diſpoſed, may appear, by thoſe factitious gems, wherein, with three or four grains of a ſkilfully calcined metal, or ſome ſuch mineral pigment, we may give the colour of a natural gem to a whole ounce, or more, of vitrified matter. And I remember, that in ſubtiler fluids, I have made the inſtance by vaſt odds more conſpicuous, having tinged with one grain, or leſs, of a prepared metal, as gold or copper, as much ſucceſſively generated phlegm, as, if it could have been all preſerved, would have amounted to a bulky lump of deeply-coloured matter.

BUT your allowing the heſtancy I have expreſſed in this laſt paragraph is not neceſſary to my preſent purpoſe; wherefore I ſhall not borrow any thing to countenance it from another paper, but paſs on to what remains.

*Arg. IV.* THE laſt thing that I ſhall repreſent, to ſhew that the virtues of opacous gems, and medicinal ſtones, may be more eaſily, than thoſe of tranſparent ones, accounted for in our hypotheſis, is this, that the main ingredients, whereof many ſuch opacous ſtones conſiſt, were complete mineral bodies before they became ſtones; ſome of them having been medicinal boluſſes, or the like earths; ſome earths abounding with metalline or mineral juices; ſome, ores of metals, or minerals of kin to metals; and ſome, in ſine bodies of other ſorts, or natures, differing from theſe and one another. For all theſe ſeveral kinds of ſoſſils may, by the ſupervening and pervation of petriſc ſpirits, be turned into ſtone; and conſequently retain many of the virtues, they were

indowed



indowed with by the mineral corpuscles, that had copiously, either under the form of liquors, or exhalations, impregnated them, whilst they were yet earths, or other bodies of a more open or penetrable texture.

I MIGHT illustrate this by the way I elsewhere mention, whereby I made such mixtures, even of stony and metalline ingredients, that, notwithstanding their coalition, were transparent; though you will grant that to be more difficult, than to compound such concretions when one is allowed to make them opacous.

BUT here I must obviate an objection, which I foresee may be made against our present fourth argument, unto which, even what I have been now saying, may afford a rise. For since it seems by our doctrine, that gems may be but magisteries, and consequently but such compositions, as, though made in the bowels of the earth, might be made or imitated by human skill, it may seem very improbable to many, that bodies so near of kin to artificial ones should be endowed with such peculiar, and, some of them, with such strange virtues, as are ascribed to divers gems, and are thought to be capable of flowing only from certain substantial forms, and those very noble ones too.

To this I might reply, that I admit not any such imaginary beings as the Peripatetic forms, which, I fear, they will never be able to demonstrate. But to avoid unnecessary disputes, I will rather answer in short, that such compositions, as are called artificial, may, for all that, be endowed with great virtues, and such as are called specifick; witness the virtues of many chymical preparations, even of those that are used by physicians of all sorts. And lest you should think, I need to fly to chymistry, of which some learned men are pleased to have a great distaste, I will name a couple of instances out of *Galen* himself; the one is the athes of cray-fish, to which, notwithstanding the destruction that has been made of the pristine body by fire, he gives a greater commendation against the as strange, as fatal poison infused by the biting of a mad dog, than he does either to the fish itself unburned, or to any medicine of nature's own providing; and I hope, you will grant a virtue of that kind and degree to be specifick enough. My other instance shall be taken from treacle, which, though allowedly a factitious body, and consisting of I know not how many ingredients shuffled together, was yet, in the days of *Galen*, to whom a book is attributed about it, and ever since has been, the famousst antidote, in these parts of the world, and has been celebrated, not only for its alexipharmical virtues, which alone are sufficient to intitle it to specifick ones, but for divers others, which are generally ascribed to it, some indeed upon the score of manifest, but others also upon that of occult qualities.

THE objection being thus dispatched, we may return to our medicinal stones, about which I shall venture to add, that, according to our way of explicating the production of them, a not impossible solution may be offered of this difficult phaenomenon; that sometimes stones, that are thought, without scruple, to be of the same kind (as hath been particularly observed by learned men of the lapis nephriticus) are of such different qualifications, that some of them prove very considerable remedies in cases, where others prove almost utterly ineffectual. And I have observed also, though very rarely, that a medical stone may have virtues, that are taught to be the properties of stones of another kind. For, according to our hypothesis, when the stony matter is impregnated, as it ought to be, with those minerals, that in the ordinary course of nature belong to that species, its virtue will be such, as it should be for kind, but for degree may be very various, answerable to the plenty, purity, subtlety, &c. of the mineral that impregnates it. But if the stony matter chance to be imbued with some other substance of a contrary nature, though, perhaps, the proportion of it may be so small, and the colour of it such, as not to make an alteration in the stone obvious to sense, and great enough to make it judged

judged to be of another species; yet it may so vitiate the matter, wherein its expected quality resides, or check and infringe its operations, as not to leave the stone any considerable degree of virtue. And on the other side, if it happen, that the mineral corpuscles, that are wont to impart a certain virtue to the stony matter of one gem, should, by some lucky hit, be so united with that of another sort of gems (of which case I formerly gave an instance in green diamonds) though the quantity of this unusual ingredient may be but very small, yet, if its efficacy be great, it may ennoble the stone with a notable degree of some such virtue, as is supposed not to belong to that species, but to another.

AND on this occasion I shall add, that I know a gentleman, a professed scholar, who to the eye seems to be of a complexion extraordinary sanguine: this person was for a long time so troubled with excessive bleedings at the nose, that notwithstanding all the remedies he could procure in an academy of physick, where he lived, he was divers times brought to death's door; till at length his case growing very famous, there was sent him by an ancient gentlewoman a blood-stone, about the bigness of a pigeon's egg, with an assurance, that it had done scarce credible cures in his disease, by being worn about the patient's neck. Upon the use of this stone he quickly recovered his health, and had long enjoyed it, when I conversed with him, but yet so, that when he left it off any considerable time, his distemper would return. And when I seemed to suspect, that imagination might have an interest in the efficacy of his remedy, he answered, that he was very well satisfied of the negative; and particularly upon this trial, that he had, by the hands of a third person, that lived not far off, and whom he named to me, stopped a hæmorrhage in a neighbouring gentlewoman, whom the violence of the distemper kept from knowing, that any thing had been applied to her, till a pretty while after the blood was stanch'd. I shall not here mention other instances, though very remarkable, of the efficacy of this stone, which I had, both from the gentleman itself, and an intimate friend of his, who is a very learned man and a physician; because I have said enough to make it seasonable for me to tell you, that notwithstanding all the odd operations of this stone, when I came to look upon it, it was so differing in colour and texture from what I expected, that I should have taken it much rather for a gem of some other species, than a blood-stone.

To confirm some of the particulars comprized in this our fourth argument, and shew the variety, and sometimes great plenty of mineral and other subterranean matters, that may concur to the composition of bodies, that pass for stones; I shall observe, that the subtilty and penetrancy of some liquors, if duly considered, may evince it to be possible, that such bodies should be petrified by them, and with them, as may in part consist of animal and vegetable substances, as in petrified skulls, bones, and pieces of wood: and we see, that soft stone, which is plentifully found near *Naples*, and commonly called the lapis lycurius, being rubbed a little and moistened with water, and then exposed to the sun in a due season of the year, will, in a very short time (as eye-witnesses have assured me) produce mushrooms fit to be eaten; as if even the seminal principles and rudiments of vegetables may be so preserved in a petrified earth, as to be able to disclose themselves, when they find an opportunity. To which agrees well what an eminent person, master of some of these stones informs me, that they now and then find them of a vast bigness, as if whole masses of earth, pregnant with the prolifick principles of mushrooms, were, by some supervening, but not very potently hardening petrescent liquor, turned into stone.

AND not only there may be bolusses, sealed earths, and such like fossils, that are commonly known to be medicinal, hardened into stone, by petrifying agents; but also other  
earths,



earths, subject to be petrified, may have medicinal and subtle particles of such a kind in them, as scarce any body would expect. But to omit instances, belonging to another paper, I have visited a certain clay-pit in a waste piece of ground, in which, at a considerable depth from the surface of the earth there lay a bed of clay, which by distillation yielded some acquaintances of mine a salt so volatile and strong, and so differing from other subterranean salts, that my examens did not discover the manifest qualities of it without some wonder; and the owners of it (persons curious and rich) did themselves use it as well as give it in physick, and cried it up for an excellent cordial, and a great opening and diaphoretic medicine.

THAT sublimable salts, sulphurs, bitumen (bodies, that communicate enough of their virtues) may be met with in the bowels of the earth, I have elsewhere shewn: and that such substances may be found in bodies that pass for stones, I have been induced to think by the chymical examen, that I purposely made of some such concretions, particularly of that solid and heavy one, that is commonly called Scotch-coal, from whence I obtained by distillation (wherein I somewhat wondered, other mens curiosity did not, as far as I knew, prevent me) a good proportion of oil or liquid bitumen, and no small number of saline particles, that seemed to be of an uncommon nature.

THAT metalline particles may concur to make up a body, that passes for a medicinal stone, may appear by native sulphur, which is itself a compounded body, besides a good proportion of mineral earth.

I HAD thoughts not to make an end of this discourse, without mentioning to you some attempts that I partly designed, and partly made, to illustrate some passages of it by purposely contrived experiments, whereof some were unprosperously, and others not altogether unsuccessfully tried. But not having the minutes of them by me, and not daring to trust my single memory in experiments so nice, and so long since made as those were, I shall here put an end to your trouble; especially since at length I perceive, that the forgetfulness of my first intended brevity has misled me so far beyond the bounds of it into excursions, whereinto the unforeseen connexion of things unawares engaged me, that I stand in need both of your pardon and my own; of yours, for having exercised your patience with a prolix discourse; and of my own, for having receded from my custom, by contributing to that prolixity, and by expatiating upon conjectures; to which the more I conform to my own practice, the less I am indulgent: though these may be the more pardonable, because I have proposed them but as guesses, not peremptory assertions, much less physical demonstrations. And if *Aristotle* himself, where he gives an account of phenomena appearing above the surface of the earth, scrupled not to think he had done enough, if he had shewn how such things may be produced; I hope it may be tolerable in me, who treat of things, that nature does privately in her dark and subterranean recesses, to have offered accounts, that are possible, if not probable. And yet I should have spent much less of my discourse upon conjectures, if I had not seen that they gave me rises to bring in more of natural history, than I could else decently do. But after all this, I confess to you (though you may think it a paradox) that one of the main causes of the prolixity of these papers was my haste, and that experience hath taught me on this occasion (as well as on some others) that there may be more truth, than there is likelihood, in the genteel conceit of a French secretary, that said, he had written his friend a long letter, because he had not leisure to write him a short one.

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T R A C T S.

CONTAINING

NEW EXPERIMENTS, touching the Relation betwixt  
FLAME and Air. And about EXPLOSIONS.

AN HYDROSTATICAL DISCOURSE, occasioned by some Ob-  
jections of Dr. HENRY MORE against some Explications of NEW  
EXPERIMENTS made by the Author of these TRACTS.

TO WHICH IS ANNEXED

AN HYDROSTATICAL LETTER, dilucidating an Experiment  
about a way of weighing Water in Water.

NEW EXPERIMENTS,

Of the POSITIVE or RELATIVE LEVITY of Bodies under Water,

Of the AIR'S SPRING on Bodies under Water,

About the DIFFERING PRESSURE of heavy SOLIDS and FLUIDS.

---

The PUBLISHER to the READER.

IT will, it is presumed, be altogether needless to preface any thing by way of commendation to the following Tracts; they will certainly commend themselves by their own worth to the intelligent and attentive reader, who might have seen them sooner, if the press had not detained them longer than was expected; since, to the publisher's knowledge, they were ready in the year 1671, except the hydrostatical discourse, and the explication of the author's experiment of weighing water in water, the former of which was finished in the beginning of this year 1672; though the latter could not be so till near the end of the same year, viz. the month of February, English stile, because the book of Mr. *George Sinclair's* Hydrostaticks, in which it is excepted against, came not, I think, before that time to *London*; I am sure, not to the view of the honourable Author. Farewel.

NEW



## NEW EXPERIMENTS

TOUCHING THE RELATION BETWIXT

## FLAME AND AIR,

SENT IN A LETTER

To the Learned Publisher of the PHILOSOPHICAL TRANSACTIONS.

SIR,

YOU may have observed, as well as I, that since the publishing of the experiments I sent you touching respiration, divers of our learned men have spent both thoughts and discourses in inquiring and disputing, whether there resides in the heart of animals such a fine and kindled, but mild substance, as they call a vital flame, to whose preservation, as to that of other flames, the air (especially as it is taken in, and expelled again by respiration) is necessary. This, among other considerations, makes me think it reasonable (though many avocations make it inconvenient) to complete the performance of the promise I made you, by adding to the experiments about respiration, which your commands have already obtained of me, those scattered notes, that I have been able to pick up about the relation betwixt flame and air. And though, I confess, they are very much inferior in number to the trials about respiration; and, that in making them it was not so much my design to complete an entire and distinct tract, though but a small one, of such experiments, as to gratify my own curiosity in the examining of a paradox or two I had been writing about flame; yet the nobleness of the question now under debate, and their pertinency to it, will possibly keep them, as few as they are, from being useless. And that also they may be the better kept from being unwelcome, I have chosen to make myself a relator of matters of fact, without engaging with either of the litigant parties in a controversy, wherein I am the less tempted to be partial, because I have not formerly declared my opinion about it, and at present, I see, on either side, persons, for whom I have no small respect and kindness.

And now, Sir, that you may not expect in the following papers such a number and variety of experiments, as I might perhaps be able to present you with, on some more tractable subject, I shall briefly mention to you some of the chief difficulties I met with in the making of these; which I do the rather, that if you and your ingenious friends have a mind to prosecute such trials, you may not be surprized with the difficulties I have met with; but provide at least against those foreseen ones, by which you will scarce fail to be encountered.

I SHALL then inform you, that the ensuing experiments were rendered uneasy and troublesome to me by this; that some of them could not be conveniently done at all seasons of the year, nor in any season in all weathers, but must be made not only in the day-time, but in sun-shine days. You will easily guess, that I speak of those experiments, that are to be made by the help of a burning-glass, casting the reflected or refracted beams of the sun upon the combustible matter placed in the exhausted receiver: for, by reason of the interposition of so thick a glass, whereby many of the incident beams of light are reflected, and others inconveniently refracted, there is ordinarily requisite a clear day, and a competent height of the sun above the horizon, and sometimes also a convenient time of the year, to bring such experiments, as we were speaking of, to a fair trial. Not to take notice, that in such attempts there usually intervene circumstantial difficulties, not so easy to be foreseen: and it not being summer, when I had occasion to make the following experiments, I could make but very few with the sun-beams; besides that there are divers others, which are not that way to be made so conveniently, if at all, as by the help of the fire.

BUT though the trials of this second sort had their conveniencies, in regard they might be made in any weather, and as well by night as day; yet they were not unattended with peculiar inconveniencies; some of which you will easily discern by the mention of them, that was necessary to be made in some of the relations themselves. And, besides more particular and emergent difficulties, there was this in general, that rendered these experiments troublesome; that, whether I made them in larger receivers, or in small, or in middle-sized ones, each of these cases had its inconveniencies: for very large receivers, besides that it was very toilsome and tedious to empty them of air, required so much time for the exhaustion, that too frequently, by that time the operator had done pumping, the included, or other heated body was grown too cold to perform the desired effect: and if the receiver were not considerably large, than the red-hot iron, or other included body, that was to burn the combustible matter, would much endanger the breaking of the over-heated glass, and not afford room enough for some phenomena to be fairly exhibited in; and, besides, create another difficulty, to which we found middle-sized receivers also obnoxious: for, several times, when the experiment required an intense heat within the receiver, then (especially if some casual obstacle hindered the quick exhaustion) the heat of the ignited iron, or some such other included body, would so melt or soften the cement that fastened the receiver to the engine, that, when the glass was brought to be well exhausted, and sometimes also before, the external air would, by its pressure and fluidity, squeeze or thrust in somewhere or otherwise yielding cement, and thereby cause in the instrument a leak, that would much incommode us, if not reduce us to begin the experiment again, inasmuch, that, for some trials, we were fain to provide a cement on purpose; the least fusible, that we used on other occasions, being yet found too fusible on these.

NOR were those I have already mentioned, the only difficulties and impediments I met with in making experiments about flame and air; but I shall not here trouble you with them in this place, where it may suffice for me to have mentioned those, that are of a more general nature, and are like the most frequently to occur.

BUT though I declined to name any other to you, than the foregoing difficulties in making the following experiments; yet I must not omit to take notice of one, that may occur to you about judging of them. For, in those trials that require to have an ignited iron or any such thing included in the receiver, it would usually happen, that so much heat would rarefy the air shut up in the mercurial gage, and consequently inable it to depress the mercury, that lies under it, far beneath the mark it would have staid



at, upon the mere account of so much ambient air pumped out: this would happen, I say, before the heated receiver was well exhausted; so that, if one be not aware of this, it will be obvious, by looking on the gage, to conclude the receiver to be well emptied, before it really is so. And therefore the safest way in these cases is, to continue to pump (without trusting to the ordinary marks) till you see, that the mercury will be no further depressed in the sealed leg of the gage; though otherwise, by concurring signs, one that is versed in those trials, may well enough judge, when he needs to pump no longer.

BUT perhaps you will here demand, whether, by our engine, we can competently withdraw the air out of a receiver; or whether, at least, that may not be much better done by the help of quicksilver, after the manner of the Torricellian experiment, in regard that ponderous liquor frees the glass, it deserts, from all the air at once, and exactly hinders the regrefs of it.

IN answer whereunto, I hope you do not expect, that I should contend for a favourable judgment of the engine I employ, than the virtuosi (as well foreign as English) have been pleased to pass on it already: and therefore, to tell you freely my thoughts about the main part of the proposed question, I shall readily avow to you, that I think, there may be experiments (such as some of those, where the included body need be but small, and where the being suddenly produced is chiefly desired in the effect) wherein, by the help of the quicksilver, the exhaustion of the air may be dispatched with greater celerity, and consequently make the effect be more conspicuous, than, by our ordinary way of trying, it would be in our engine; since the fall of the mercury does, as the objection intimates, produce a vacuum (in our sense of that word) very nimbly, whereby the expansion of the air is presently effected, and the aerial particles, harboured in the pores of any body placed in this deserted cavity, will thereby have opportunity more suddenly to expand themselves. But, on the other side, I might answer in general, that when I have particular occasions to dispatch the exhaustion of the air, I can very much hasten it by barely lessening, as I have several times done, the capacity of the receiver; inasmuch, that I have sometimes employed so small an one, that in half a minute, or much less, after it was fitted on, we could considerably exhaust it, and thereby produce phenomena exceeding conspicuous. And as to the experiments of this little tract in particular, it may be said, that, not to mention the troublesome, and other inconveniencies of needing to employ such an unweildy weight of mercury, you will easily find, by the phenomena of divers of the ensuing trials, that most of them cannot be with any conveniency, and some of them not at all, made in the Torricellian tubes. As for the ground of the objection, that the air cannot be so well drawn out by our way, as by the subsiding of the mercury; though you may think that very clear, yet one that were very jealous of the reputation of the instrument I employ, may perhaps reasonably enough question it. For the vacuum that is produced in the Torricellian experiment, as it is made all at once, so it is made once for all; and therefore, if there were any aerial particles lurking in the mercury, as there will be pretty store, if the quantity of that liquor be great enough to make a considerable vacuum, which if it be not, it will be too small for very many of our trials; they will remain in the deserted cavity at the top of the glass, and, by their expansion there, much hinder the full operation of an ambient vacuum upon the bodies placed in it. Besides that almost all such bodies, if they be dry, will be so incongruous to mercury (which scarce sticks to any consistent bodies but metals) that probably there will be no small number of aerial corpuscles intercepted between the mercury and those surfaces, to which it does not closely adhere; which

which aëry corpuscles, when the subsiding mercury deserts them, will be left to encrease the number of those, that, as we were saying, will emerge from the mercury; from which, as also from the pores of the included bodies, will perhaps arise divers new ones, from time to time, for a pretty while after. And in case the vacuum be made by a cylinder of two or three and thirty feet of water, as for some experiments that have been tried in *France* and *Italy*, hath been done, the emerſion of bubbles may last a long time, as may be gathered from some observations of mine, elsewhere related.

On the contrary, in our engine, though when the receivers are not very small, they are more slowly emptied; yet in recompence, we may continue the pumping out of the air as long, and renew it in the same experiment as often as we think fit: so that, if we perceive, that after the first exhaustion of the glass, there happen any aërial particles to extricate themselves successively out of the included body, we can, by resuming the pump from time to time, whenever need requires, free the vacuum from these also; which, in some cases, I have found to be longer and more copiously emitted by the included bodies, than any thing but jealous trials could have convinced me of. And to confirm what I have been saying by something historical, I shall add, that though the excellent Florentine academians are thought to have prosecuted the experiments about the vacuum made with mercury the furthest of any, yet some eminent members of that illustrious society were pleased to confess to me, that they never were able, by the help of mercury, to bring a glass-bubble, sealed up with air in it, to burst of itself by the withdrawing of the external air; which yet I have often done with the engine I employ, and convinced them, that I could do so, by doing it in their presence.

You will, perhaps, think it somewhat strange, to find, that I set down some of the following narratives in such a way, as does not express me solicitous to ascribe and vindicate to the air so absolute and equal a necessity to the production and conservation of all flames, as divers learned men have concluded from my former experiments. But I, that am content to be kind to the air, but not partial, shall not scruple to declare to you, that, as much as some may think me beholden to the air for any discoveries of itself, it may have vouchsafed me; yet, I think, a natural, as well as a civil historian, does, in his accounts of matters of fact, owe more to truth, than to gratitude itself. And though, wherever the air can challenge a clear, or, at least, a probable interest in a phenomenon, I am not only disposed, but glad to do it right; yet I would not easily assert to it a larger jurisdiction than I find nature to have assigned it; especially since, without partiality, that, I presume, may be shewn to be very large and considerable, and perhaps to reach to many things, wherewith men seem not to have yet taken notice, that it hath any thing to do at all.

WHAT hath been hitherto said, will not, I hope, seem impertinent or useless, whenever you shall fall upon the actual making of such experiments as you are about to read. But I fear, that to add any thing more (which were not difficult for me to do to the preliminary part of this small tract) would make it too disproportionate to the historical; from which I shall therefore no longer detain you.



THE FIRST TITLE.

Of the difficulty of producing FLAME without AIR.

EXPERIMENT I.

*A way of kindling brimstone in vacuo Boyleano, unsuccessfully tried.*

WE took a small earthen melting pot, of an almost cylindrical figure, and well glazed (when it was first baked, by the heat; and into this we put a small cylinder of iron, of about an inch in thickness, and half as much more in diameter, made red hot in the fire; and having hastily pumped out the air, to prevent the breaking of the glass; when this vessel seemed to be well emptied, we let down, by a turning key, a piece of paper, wherein was put a convenient quantity of flower of brimstone, under which the iron had been carefully placed; so that being let down, it might fall upon the heated metal; which as soon as it came to do, that vehement heat did, as we expected, presently destroy the contiguous paper: whence the included sulphur fell immediately upon the iron, whose upper part was a little concave, that it might contain the flowers when melted. But all the heat of the iron, though it made the paper and sulphur smoke, would not actually kindle either of them, that we could perceive.

EXPERIMENT II.

*An ineffectual attempt to kindle sulphur in our vacuum another way.*

ANOTHER way I thought of to examine the inflammability of sulphur without air; which, though it may prove somewhat hazardous to put it in practice, I resolved to try, and did so after the following manner:

INTO a glass-bubble of a convenient size, and furnished with a neck fit for our purpose, we put a little flower of brimstone (as likely to be more pure and inflammable than common sulphur); and having exhausted the glass, and secured it against the return of the air, we laid it upon burning coals, where it did not take fire, but rise all to the opposite part of the glass, in the form of a fine powder; and that part being turned downward and laid on coals, the brimstone, without kindling, rose again in the form of an expanded substance, which (being removed from the fire) was, for the most part, transparent, not unlike a yellow varnish.

ADVERTISEMENT.

THOUGH these unsuccessful attempts to kindle sulphur in our exhausted receivers were made more discouraging by some more, that were made another way; yet judging that  
last

last way to be rational enough, we persisted somewhat obstinately in our endeavours, and conjecturing, that there might be some unperceived difference between minerals, that do all of them pass, and are sold for common sulphur, I made trial, according to the way hereafter to be mentioned, with another parcel of brimstone, which differed not so much from the former, as to make it worth while to set down a description of it, that probably would not be useful.

BUT in this place, it may suffice to have given a general intimation of the possibility of the thing. The proof of it you will meet with under the third Title, when I come to tell you what use I endeavoured to make of our sulphureous flames.

### EXPERIMENT III.

*Shewing the efficacy of air in the production of flame, without any actually flaming or burning body.*

HAVING hitherto examined by the presence of the air, what interest it has in kindling of flame; it will not be impertinent to add an experiment or two, that we tried to shew the same interest of the air, by the effects of its admission into our vacuum. For I thought it might reasonably be supposed, that if such dispositions were introduced into a body, as that there should not appear any thing wanting to turn it into flame but the presence of the air, an actual accension of that body might be produced by the admitted air, without the intervention of any actual flame, or fire, or even heated substance; the warrantableness of which supposition may be judged by the two following experiments.

WHEN we had made the experiment, ere long to be related in its due place, (viz. Title II. Experiment the 2d) to examine the presumption we had, that even when the iron was not hot enough to keep the melted brimstone in such a heat, as was requisite to make it burn without air, or with very little, it would yet be hot enough to kindle the sulphur, if the air had access to it: to examine this, I say, we made two or three several trials, and found by them, that if some little while after the flame was extinguished, the receiver were removed, the sulphur would presently take fire again, and flame as vigorously as before. But I thought it might without absurdity be doubted, whether or no the agency of the air in the production of the flame might not be somewhat less, than these trials would persuade; because, that by taking off the receiver, the sulphur was not only exposed to fresh air, but also advantaged with a free scope for the avolation of those fumes, which in a close vessel might be presumed to have been unfriendly to the flame.

HOW far this doubt may, and how far it should, be admitted, we may be assisted to discern by the subjoined experiment, though made in great part for another purpose; which you will perceive by the beginning of the memorial I made of it, that runs thus;

### EXPERIMENT IV.

*A differing experiment to the same purpose with the former.*

HAVING a mind to try, at how great a degree of rarefaction of the air, it was possible to make sulphur flame by the assistance of an adventitious heat, we caused such an experi-



experiment as the above-mentioned to be reiterated, and the pumping to be continued for some time after the flame of the melted flowers of brimstone appeared to be quite extinguished, and the receiver was judged by those that managed the pump (and that upon probable signs) to be very well exhausted. Then, without stirring the receiver, we let in at the stop-cock very warily a little air, upon which we could perceive, though not a constant flame, yet divers little flashes, as it were, which disclosed themselves by their blue colour to be sulphureous flames; and yet the air, that had sufficed to rekindle the sulphur, was so little, that two exsuctions more drew it out again, and quite deprived us of the mentioned flashes. And when a little air was cautiously let in again at the stop-cock, the like flashes began again to appear, which, upon two exsuctions more, did again quite vanish, though, upon the letting in a little fresh air the third time, they did once more re-appear.

WHETHER, and how far such experiments as these may conduce to explicate what is related of fire suddenly appearing in long undiscovered vaults or caves to those that first broke into them, I may perchance elsewhere consider, but shall not here enquire, especially being not yet fully satisfied of the truth of the matter of fact.

## EXPERIMENT V.

*About an endeavour to fire gunpowder in vacuo with the sun-beams.*

WHATEVER hath been hitherto delivered, will not, I presume, make it unreasonable to enquire, whether, what interest soever the air appears to have in the production of those flames that are to last for some time, there may not easily be produced a momentary flame or flash, without any assistance from the air. Wherefore I employed some endeavours to discover, whether there were the same need of air to the going off of gunpowder, as to the inflammation of other bodies. And though my first attempt of this nature being unprosperous, it was concluded by the learned of the by-standers, that I should never be able to make a successful one to kindle gunpowder in an exhausted receiver; yet this did not hinder me from prosecuting a design, for whose feasibility I considered, that it might be alledged *a priori* (as they use to speak) that brimstone, which is one of the ingredients of gunpowder, appears by several trials to be sometimes capable of accension in our vacuum, and therefore probably may kindle the rest. But how far the firing of powder, without the help of air, is possible, will be best judged by the experiments you will meet with under the third title; and how far it is more difficult to be kindled in our exhausted receivers, than in the open air (which is an inquiry proper for this place) may be guessed by the subjoined trial; which, though it were made many years since (in the year 1660) before we had devised the mercurial gage, to examine how well the receiver was exhausted, I shall yet afford it a room in this place, because it was made in summer by the help of a burning-glass, which I could not employ to purpose in the winter season, wherein the two following trials were made.

To give you then some account of that part of the experiment which concerns our present inquiry, I will subjoin a transcript of what I find registered about it; which is to this purpose, and almost in these words: that, having conveniently placed three or four grains of gunpowder in the cavity of our receiver, and having carefully drawn out the air, we cast the sun-beams, united by a good burning-glass, upon the powder, and kept them there a pretty while to little purpose; till, at length, the powder, instead of taking fire, smoking only, and melting like a metal, those spectators that were of another

opinion than I was yet convinced of, would have me leave off. The further event of such trials more fully prosecuted you will find under the third title; all that will be pertinent to be here added being, that the newly recited experiment was not the single one we made about that time, that discovered a great indisposition even in gunpowder to be fired in our vacuum.

## EXPERIMENT VI.

*An attempt to fire gunpowder in vacuo, by means of a hot iron.*

WE took, by weight, what we judged a convenient quantity of gunpowder that was extraordinarily strong and well made; and having in our receiver, that was capable of holding about sixteen pounds of water, placed the formerly mentioned iron first heated red-hot, when the air appeared by the mercurial gage to have been diligently pumped out, we let down, by help of the turning-key, a small piece of thin paper, wherein the powder had been put, till we saw it reached the plate, by whose heat we hoped the paper would be destroyed, and the powder made to go off. But though both the one and the other had been purposely well dried near the fire, before they were put into the receiver; the desired explosion of the powder did not ensue. Yet there appeared upon the iron plate a pretty broad blue flame, like that of brimstone (whence it was judged the sulphureous ingredient of the gun-powder, that was kindled) which lasted so very long, as we could not but wonder at it. But, at length, the powder not going off and the still decaying heat of the iron forbidding us to wait any longer, we thought fit to take off the receiver, and found (as we expected) that the paper contiguous to the iron was, in part, destroyed by its heat; but most of the grains of the powder seemed not altered, and were found disposed enough to be fired, notwithstanding the consumption of the brimstone that had burned away.

## A P P E N D I X.

To confirm the foregoing experiment, by shewing how great a disposition to take fire there may be in gunpowder, that yet would not do so without air, I shall subjoin this observation.

HAVING reiterated the newly mentioned experiment after the like manner, and with the same receiver, and iron-plate, as formerly, we did not find any explosion to be made for so long a time, that thinking it in vain to wait any farther, we let in the air, which might perhaps, by the help of the remaining heat of the iron, procure the operation we at first desired. The event was, that after nothing had ensued for a good while, and we scarce thought that such a thing would happen; the powder suddenly went off with a great flash, and so shook the receiver that was yet standing on the engine, as to endanger the throwing of it down. Which circumstance I mention, to give you a caution that may prove useful, in case you try in close vessels experiments with gunpowder; since if they be not warily managed, they may sometimes (as I have had occasion to observe) prove dangerous enough; which will be the better discerned, if I add, that the powder that had this operation on a receiver (large enough to contain two gallons of liquor) was weighed before it was put in, and amounted but to one grain (though a greater



greater quantity might perhaps have been well enough ventured upon, if it had been but common gunpowder).

# EXPERIMENT VII.

*Reciting another way, whereby the firing of gunpowder in vacuo Boyliano was attempted.*

To diversify our ways of examining the indisposedness of gunpowder to be fired in our vacuum, we thought fit to add to the foregoing trials that which followeth.

INTO a pretty large and strong glass-bubble we put a few small corns of gunpowder, and having carefully exhausted it, and secured it against the return of the air, we put it upon a pretty quantity of live coals superficially covered with ashes; by whose heat the sulphureous ingredient of the powder was in part kindled, and burned blue for a pretty while, and with a flame considerably great (in proportion to the powder); upon whose ceasing, the powder, which, when all was done, did not take fire, appeared to have sent up, besides the flame, a pretty deal of sulphureous sublimate, that stuck to the upper part of the glass, and being held against a candle we caused to be brought in (for the experiment had been purposely made in a dark place) it exhibited divers vivid colours like those of the rainbow.

# EXPERIMENT VIII.

*About a trial made to fire gunpowder in our vacuum by the help of sparks.*

THOUGH in the fourteenth of the long since published physico-mechanical experiments there is recited a trial made about kindling of gunpowder with a pistol; yet I shall not forbear to subjoin the ensuing account, partly, because the receiver we then employed, being about four times, if I mis-remember not, as big as that we last made use of, it was very difficult to exhaust the one so well as the other; and partly, because we wanted some accommodations, with which we since furnished ourselves, and (having not then devised the mercurial gage we employed in the making this last experiment) we could not then judge so well, as we since could, of the degrees to which the receiver was emptied. And, therefore, when in the relation of that fourteenth trial, there is mention made of one attempt that did succeed, among divers that did not; there is towards the close an intimation given, that in spite of the great rarefaction that had been made in the air, there might yet be some little portion of it remaining in the receiver. I proceed then to the promised relation, which I find thus set down:

To prosecute the design of the foregoing experiment by a way somewhat differing from those hitherto mentioned, we made, though not without difficulty, the ensuing trial; one of whose scopes you will find intimated at the close of the relation.

WE took a small and very short pistol, and having well fastened it with strings to a great weight that was placed upon the iron-plate of our engine, we drew up the cock, and primed the pan with dry powder; then over both the weight and pistol we whelmed a receiver capable of containing two gallons of liquor, and having carefully cemented it on, we caused the air to be diligently pumped out; having before put in a mercurial gage, to help us to discern when it was exhausted. Lastly, ordering the pump to be plied

in the mean while, for fear some air should steal in, before the trial was completed, we did, by the motion of the turning-key, shorten a string that was tied both to it and the trigger of the pistol, by which means we did as much as we could towards the firing of the powder in the pan; but though the pan were made to fly open, yet the powder did not go off; whereupon letting in the air, and cocking the pistol again, without taking it off the weight it was tied to before, we drew out a little air, to be sure that the receiver was closely cemented on (which care we took in reference to another experiment) and then letting in the air at the top of the receiver, and stopping it in with the turning-key, we did, by the help of that key, draw aside the trigger again; whereupon, though there had been no new powder put into the pan, nor any left in it, but only some little that remained after the late trial, yet that readily took fire and flashed in the pan; which made it the more probable, that, in the former trial, sparks of fire had been struck out by the collision of the flint and steel; which was the more credible, because in another trial made the same hour in the same exhausted receiver, two of the assistants plainly saw a spark or two fly out upon the falling of the cock, though I, that chanced to stand in an inconvenient place, did not then perceive it. But afterwards, having caused the experiment for my fuller satisfaction to be repeated, I freed myself from need of trusting others eyes; so that it appears, that notwithstanding the great indisposition of gunpowder itself to be reduced into flame in our vacuum, yet even solid matter is not incapable of being ignited there, if it be put into a motion sufficiently vehement.

If this experiment had not been so very troublesome to make, I should have been invited to reiterate it, because a not contemptible scruple may be prevented, if the trial can be made to succeed, in regard that the going off of the whole gunpowder, by the falling of a spark or two only upon two or three of its grains, would argue, that the accension of the rest was made by the propagation of flame from the kindled grains to the rest; so small a portion of ignited and suddenly vanishing matter, as is to be found in a spark or two, being not likely to be able in so very short a time to impart a vehement, or so much as a sensible heat, to the whole aggregate of grains, or at least a great part of them, as the focus of a burning-glass, held long enough upon them to make them melt, may well be supposed to do.

## EXPERIMENT IX.

*Two ways of making aurum fulminans go off in our exhausted receiver.*

BECAUSE it is wont to be supposed, how justly I here dispute not, that aurum fulminans, as the chymists call it, is much of the nature of gunpowder, though by vast odds stronger than it; I thought it not unfit to make trial, whether it could be made to go off in our exhausted receiver; and accordingly, about the time that the other experiment of firing gunpowder by the sun-beams was made, we also made trial of this; and that, as I remember, in the same receiver, and with the same burning-glass. The event was, that, though the air had been pumped out, the concentrated beams of the sun made the aurum fulminans go off, and violently scatter about the cavity of the receiver a yellowish dust or powder, which other trials in the free air made us look upon as particles of the gold, that was the main ingredient of this odd composition.

THIS experiment we reiterated a good while after in another place, and with other vessels, and yet with the like success.

BUT



BUT in regard these trials being made by the united sun-beams, it was unavoidable that our eyes were before-hand affected with the vivid impressions of so glaring a light it seemed not safe to determine, by the bare going off, or shattering of the aurum fulminans, whether or no it afforded any flame or light upon its explosion; for, as we could not be sure of the affirmative, because our eyes could not discern any momentary flame or flash; so it seemed not safe to conclude the negative; since, though there had been such a flame, yet, if it had not been strong, it would not have been sensible to our eyes, whilst pre-affected by a powerful light. Wherefore we resolved to make this trial in the night with an iron heated, but not candent (that its light might not eclipse that, which the powder might afford); and having, after the manner already often recited, exhausted a pretty large receiver, and let down by a string half a quarter of a grain (by weight) of good aurum fulminans of our own preparing, loosely tied in a little piece of thin paper (which paper, former trials to another purpose kept us from fearing, that no hotter an iron than ours then was, would kindle) we found, as we expected, that after the powder had lain long enough upon the iron to be thoroughly heated, it went off all together, and, as the by-standers affirmed, with a flash: but my face being accidentally turned to remove a light that I feared might disturb us, I could not see the flash myself, and therefore caused the experiment to be made once more, to ground my narrative upon my own observation; which quickly assured me, that the luminous flash produced upon the explosion was not only sensible but considerable.

## THE SECOND TITLE.

Of the Difficulty of preserving FLAME without  
AIR.

SINCE it is generally, and, in most cases, justly esteemed to be more easy to preserve flame in a body that is already actually kindled, than to produce it there at first; we thought fit to try, whether, at least, bodies already burning might not be kept in that state without the concurrence of air. And though in some of our formerly published physico-mechanical experiments, it happened that actual flame would scarce last a minute or two in our large pneumatical receiver; yet, because it seemed not improbable, that mineral bodies once kindled might afford a vigorous and very durable flame, we thought fit to devise, and make the following trials; whence probably we might receive some new informations about the diversities, and some other phenomena of flame, and the various degrees, wherein the air is necessary or helpful to them.

## EXPERIMENT I.

*Reciting an attempt to preserve the flame of brimstone without air.*

We put upon a thick metalline plate a convenient quantity of flowers of sulphur; and having kindled them in the air, we nimbly conveyed them into a receiver, and made haste to pump out some of the included air, partly for other reasons, and partly that the cavity of the receiver might be the sooner freed from smoke, which would, if plentiful, both injure the flame, and hinder our sight. As soon as the pump began to be plied, or presently after, the flame appeared to be sensibly decayed, and continued to be lessened at every exsuction of the air; and in effect it expired, before the air was quite drawn out. Nor did it, upon the early removal of the receiver, do any more than afford, for a very little while, somewhat more of smoke in the open air, than it appeared to do before.

THE reiteration of this experiment presently after afforded us nothing new worth mentioning in this place.

## EXPERIMENT II.

*Relating a trial about the duration of the flame of sulphur in vacuo Boyleano.*

To vary a little the foregoing experiment, and try to save some moments of time, which on these occasions is to be husbanded with the utmost care; having provided a cylinder of iron larger than the former, that it might by its bulk, being once heated, both contribute to the accension of the sulphur, and to the lasting of its flame, we made a trial, that I find registered to this effect:

WE took a pretty big lump of brimstone, and tied it to the turning-key; and having got what else was necessary in a readiness, we caused the iron-plate to be hastily brought red-hot from the fire, and put upon a pedestal, that the flame might be the more conspicuous; and having nimbly cemented on the receiver, we speedily let down the suspended brimstone, till it rested upon the red-hot iron, by which being kindled, it sent up a great flame with copious fumes, which hindered us not from plying the pump, till we had, as we conjectured, emptied the receiver; which we could not do without withdrawing, together with the air, much sulphureous smoke, that was offensive enough both to the eyes and nostrils. But notwithstanding this pumping out of the air, though the flame did seem gradually to be somewhat impaired, yet it manifestly continued burning much longer, than by the short duration of other flames in our receivers, when diligence is used to withdraw the air from them, one could have expected. And especially one time (for the experiment was made more than once) the flame lasted, till the receiver was judged to be well exhausted; and some thought it did so survive the exhaustion, that it went not out so much for want of air, as fuel; the brimstone appearing, when we took off the receiver, either to have been consumed by the fire that fed on it, or to have casually run off from the iron, whose heat had kept it constantly melted.

IN case you should have a mind to prosecute experiments of the nature of this and the precedent, it may not prove useless, if I intimate to you the following advertisements.

1. For the red-hot iron above-mentioned, we thought it not amiss to provide, instead of the melting-pot employed in the first experiment, a pedestal, if I may so call it, made



of a lump of dried tobacco-pipe clay, that the vehement heat of the iron might neither fill the receiver with the smoke of what it leaned on, nor injure the engine, if it should rest immediately upon that; and this pedestal should be so placed, that the iron may be as far as you can from the sides of the receiver, which else the excessive heat would endanger.

2. To the above-mentioned concave iron that was to receive the brimstone, we did for some occasions cause to be fitted a thick convex piece of iron, shaped almost like a flattish button; which was not to be used constantly, but upon occasion, that, being laid red-hot over the melted brimstone, it might increase the heat, and keep the flame from having so broad a superficies, whereby it would consume its fuel too fast.

3. We sometimes thought it expedient, for the clearer discerning of what should happen in the receiver, to make the experiment by night, and remove the candles, when we were just about to pump, presuming, that the flame would be conspicuous enough by its own light; as indeed we found it to be, though its light were but dim, considering the greatness of the flame; whose colour, though it did not quite lose its wonted bluishness, seemed yet to have received a great and somewhat odd alteration.

THERE is one great inconvenience, scarce avoidable in this experiment, *viz.* that the fumes ascending very copiously do quickly much darken the receiver, and if the trial be long continued, line it with a kind of flower of brimstone, which obscures it much more, and therefore ought to be carefully wiped away, whensoever the receiver is taken off; upon which account you will not, I presume, wonder, if you shall find the phenomena of these experiments not always to be the very same with what you meet with in this paper; since, as it is very possible that we may not have been able to observe things so accurately by reason of the newly mentioned fumes and flowers; so it is not impossible that the difference, if there shall be any, of other men's observations from ours should proceed from the same cause.

BEFORE we pass from this second experiment, it will not be amiss to take notice, that though the flames of brimstone may be allowed to be somewhat more durable than the flames of vegetables are wont to be; yet it is not safe to conclude, that it was merely upon the account of their native vigour, that the flames above-mentioned lasted so long in our receiver.

FOR we seemed to observe that there was requisite a very intense heat of the iron to make the sulphur capable of flaming on it, when any considerable proportion of air was withdrawn. For which reason it seems expedient, according to what I lately intimated, that the iron that is to keep it melted, be of a good thickness, that it may the longer retain a competent heat; and we thought it contributed to the successfulest trials we made, that in them we used, besides the concave iron, the convex one mentioned in the second note.

### EXPERIMENT III.

*Of the lasting of the flame of a metalline substance in the same vacuum.*

THOSE sulphurs, that chymists call metalline, being supposed by many to be of a much more fixed nature than common sulphur, and it being indeed probable enough, that in them good store of very minute particles are crowded together, I thought fit to try, whether a body, wherein a vulgar chymist would think the sulphur of a metal to be the main ingredient, would afford in our vacuum a more vigorous or lasting flame, than

than that of common sulphur. And, though I will not here trouble you with my particular scruples about the chymists doctrine concerning metalline sulphurs, nor with the grounds, on which I devised the following inflammable solution of Mars (for I do not now give it a more determinate name) which some chymists will not perhaps dislike; I shall here annex the ensuing transcript of the trial itself.

HAVING provided a saline spirit, which by an uncommon way of preparation was made exceeding sharp and piercing, we put into a phial, capable of containing three or four ounces of water, a convenient quantity of filings of steel, which were not such as are commonly sold in shops to chymists and apothecaries (those being usually not free enough from rust) but such as I had a while before caused to be purposely filed off from a piece of good steel. This metalline powder being moistened in the phial with a little of the menstruum, was afterwards drenched with more; whereupon the mixture grew very hot, and belched up copious and stinking fumes; which, whether they consisted altogether of the volatile sulphur of the Mars, or of metalline steams participating of a sulphureous nature, and joined with the saline exhalations of the menstruum, is not necessary to be here discussed. But whencesoever this stinking smoke proceeded, so inflammable it was, that upon the approach of a lighted candle to it, it would readily enough take fire, and burn with a bluish, and somewhat greenish flame, at the mouth of the phial, for a good while together; and that, though with little light, yet with more strength than one would easily suspect.

THIS flaming phial, therefore, we conveyed into a receiver, which he, who used to manage the pump, affirmed, that about six exsuctions would exhaust. And the receiver being well cemented on, upon the first suck the flame suddenly appeared four or five times as great as before; which I ascribed to this, that upon the withdrawing of the air, and consequently the weakening of its pressure, great store of bubbles were produced in the menstruum, which breaking could not but supply the neck of the phial with store of inflammable steams, which, as we thought, took not fire without some noise; upon the second exsuction of the air, the flame blazed out as before, and so it likewise did upon the third exsuction, but after that it went out; nor could we rekindle any fire by hastily removing the receiver; only we found, that there remained such a disposition in the smoke to inflammability, that holding a lighted candle to it, a flame was quickly rekindled.

## EXPERIMENT IV.

*Of the duration of the flame of spirit of wine impregnated with a metal in the exhausted receiver.*

BECAUSE it may, upon grounds not improbable, be thought, that well-dephlegmed spirit of wine, being a pure æthereal liquor, which does not, like combustible sulphurs (whether vulgar or metalline) emit any visible smoke to stifle the flame (into which it may, in the free air, be totally resolved) if this spirituous, and thus qualified liquor could be duly associated with a metalline body, the resulting flame might be more than ordinarily vigorous and durable; I resolved to make an experiment of this sort, and having by a way, that I delivered in another paper [in a Paradox about the fuel of Flames] so united highly rectified spirit of wine with a prepared metal, that they would both afford a conspicuously tinted flame; we put this mixture into a small glass-lamp, made on purpose, and furnished with a very slender wick, which the mixture would not burn,



burn, whilst there was liquor enough to imbibe it well; and putting this lighted lamp into a convenient place of a receiver, that was not small, since it was able to contain about two gallons, or sixteen pounds of water, we made haste to cement on the glass to the engine, and yet found not in two or three several trials, that after the pump began to be moved, so little a quantity of tinted flame in that capacious glass lasted much, if at all, more than half a minute of an hour, estimated by a minute watch.

AND because the receiver we then made use of seemed to me, by reason of its size, and some accommodations that belong to it, proper enough to be employed about other trials, concerning the relation between flame and air; I thought fit to try, with the same small lamp and liquor, what other phenomena of that kind would be afforded by letting air in and out, according to the various exigencies of my particular aims.

BUT not having then, nor in some time after, the leisure and opportunity of setting down things circumstantially, I contented myself to take those short notes of the principal things, whereof I now subjoin the transcript.

WHEN the flame began to decay, the turning-key being now and then drawn almost out, the tinted flame lasted once a minute and a half, and another time longer.

THE turning-key being taken out in the beginning, the flame lasted two minutes or better.

A PIPE bedded in the cement at the bottom of the glass, and having at each end an open orifice almost of the bigness of that filled by the turning-key, which key was then removed from the top, the tinted spirit seemed to burn very conveniently, as if the flame would have burned very long, if we would have permitted it so to do.

THE orifice at the top being stopped with the turning-key, though the pipe were left open at the bottom, it plainly, in a short time, seemed much to decay, and ready to expire; whereupon I caused one to blow constantly, yet but very gently, in at the pipe with a pair of bellows, and by this means, though we did not keep the flame vigorous, yet we kept it alive for above four minutes; and then observing it to be manifestly stronger than it was, when we began to refresh it with the bellows, we ceased from blowing, and found, that though the glass pipe was still left open, yet within about one minute the flame was quite extinguished.

## EXPERIMENT V.

### *Of the conservation of flame under water.*

THE better to examine the necessity of air to flame, I thought fit not only to make the several trials mentioned in this paper, whether it would live in a medium much thinner than air; but also to try, whether it would be able to continue in a medium many hundred times thicker than air, namely in water.

I DOUBTED not but many would think this both an easy and a needless inquiry, since eminent writers, both ancient and modern, tell us without scruple, that Naptha and Camphire will burn under water; but I had never the good fortune to be able to make them do so; and may be allowed to doubt, whether these writers, notwithstanding their confidence, deliver what they affirm, upon experience not bare tradition. And though in celebrated authors I have met with divers receipts of making compositions, that will not only burn under water, but be kindled by it; yet I have found those I had occasion to consider, to be so lamely, or so darkly (and some of them I fear so falsely) set down, that by the following composition, how slight soever it may seem, I have been

able to do more, than with things they speak very promisingly of; since, though it will not be kindled by water, yet being once kindled, it will continue to burn under water.

AND that there might be no suspicion, that whilst the mixture continued under water, it did only, as it were, vehemently ferment, or suffer a violent agitation of its parts without having them kindled, till in their ascending they were actually fired by the contact of the air incumbent on the surface of the water; to obviate this suspicion (I say) we were careful to try the experiment, not only in other vessels, but in a large glass, the transparency of whose sides, as well as that of the contained water, would permit us to see, for a while, the burning of our composition, which was sometimes with a weight detained, and sometimes with a forceps held, till it was consumed, a good way under the surface of the water.

THE way of making the experiment is this; we took of gunpowder three ounces, of well burned charcoal one drachm, of good sulphur or flower of brimstone a little less than half a drachm, of choice salt-petre near a drachm and a half; which ingredients being well reduced to powder, and diligently mingled without any liquor, either a large goose-quill, whose feathery part was cut off, or a piece of a tobacco-pipe, of two or three inches long, and well stopped at one end, had its cavity well filled with this mixture (instead of which, beaten gunpowder alone might serve, if it did not operate too violently, or waste too soon) for the kindling whereof, the open orifice of the quill or pipe was carefully stopped with a convenient quantity of the same mixture, made up with as little chymical oil or water as would bring it to a fit consistence. This wild-fire was kindled in the air, and the quill or pipe, together with a weight, to which it was tied to keep it from ascending, was slowly let down to a convenient depth under water, where it would continue to burn, as appeared by the great smoke it emitted, and other signs, as it did in the air; because the shape of the quill or pipe kept the dry mixture from being accessible to the water (that would have disordered and spoiled it) at any other part than the upper orifice; and there the stream of kindled matter issued out with such violence, as did incessantly beat off the neighbouring water, and kept it from entering into the cavity that contained the mixture, which therefore would continue burning till it was consumed.

It is probable, that most men will conclude from this experiment, that air is not so absolutely necessary to the duration of flame, as some other of our trials seem to argue; and that there ought to be a difference made between ordinary flames, and those that burn with an extraordinary vehemency. But my design being, as I long since intimated, rather to relate trials than debate hypotheses, I shall only add, that it may be pretended on the behalf of the opinion, that this experiment seems to disprove, that, not to mention the air that may lurk in the pores of the water, or that which may be intercepted between the little grains of powder, whereof the mixture consists, the saltpetre itself may be supposed to be of such a texture, that, in its very formation, the corpuscles that compose it may intercept store of little ærial particles between the very minute solid ones which those corpuscles are made up of. And this inexistence of the air in nitre may be probably argued from the great windiness of the flame that is produced upon the deflagration of nitre. According to this surmise, though our mixture burns under water, yet it does not burn without air, being supplied with enough to serve the turn by the numerous eruptions of the ærial particles of the dissipated nitre itself.

On this occasion I remember, that in another paper I relate, that for divers purposes, and among them to remove this suspicion, I successfully tried to reproduce nitre in *vacuo Beyliano*, that there might not be any air, or at least any quantity worth heeding, intercepted between the convening particles, that by their coalitions made up the nitrous corpuscles,



pufcles, which, in favour of the neceffity of air to flame, may be pretended to be but fo many little empty bubbles clofe ftopped, whole moiſter parts may, by the fire that kindles the nitre, be exceedingly rarefied, and in that eſtate emulate air, and violently burſt their little priſons, and throw about the fragments of them with force, and in numbers enough to make their aggregate appear ſuch a flame, as is wont to be made by unctuous and truly combuſtible bodies; and yet this rarefied ſubſtance that thus ſhatters the nitrous particles, may really be no true and laſting air, but only vehemently agitated vapours, which preſently, upon the ceſſation of the heat, return to liquor; as we ſee, that the vapours of an æolipile that iſſue out after the aerial particles have been expelled, though they make a great noiſe and a temporary wind near the hole they ſteam out at, and would perhaps, if that hole were cloſe ſtopped, break the æolipile; yet are not true and permanent air, but at a ſmall diſtance off the inſtrument return into water.

BUT though I could ſuggeſt other ſuſpicions and conjectures about the inclusion of air between the particles of ſaltpetre, yet I forbear to mention them in a writing deſigned to be chiefly hiſtorical.

## EXPERIMENT VI.

*Relating an odd phenomenon about the flame of a metal in our vacuum.*

To the foregoing experiments made on purpoſe I ſhall add a phenomenon afforded us by chance, and yet not unworthy to accompany the reſt.

WHILST we were trying to kindle ſomething in our exhausted receiver, it happened by ſome accident or other, that the combuſtible ſubſtance that was to be kindled, fell beſides the iron, whereby our intended trial was defeated. But whiſt we were conſidering what was to be done on this occaſion, and had not yet let in the air that had been pumped out, the lights alſo continuing yet removed; we were ſurprized to ſee ſomething burn, like a pale blueiſh flame, almoſt in the miſt of the cavity of the receiver, and at firſt ſuſpected it to be ſome illuſion of the eyes; but all the by-ſtanders perceiving it alike, and obſerving that it grew very broad, we looked at it with great attention, and found it to laſt much longer than I remember I have ſeen any flame do in an exhausted receiver. I ſhould have ſuſpected it had proceeded from ſome brimſtone ſticking, without our heeding it, to ſome part of the iron, which we had formerly employed to kindle ſulphur in our receiver, had it not been, that, beſides other things, I remembered, that we had juſt before kept it red-hot in the fire, and conſequently muſt have burned away any little brimſtone, if there were any, that adhered to it; but though we much wondered, whence this our flame proceeded, I would not let any thing be done that might haſten its extinction; and at length, when it expired of itſelf, we let in the air, which had been till then kept out, and perceived upon the concave part of the iron (which we judged to be the place, where the flame had appeared) a piece of melted metal, which we concluded had been faſtened to the ſtring, that the fuel we deſigned to kindle had been tied to, in order to the letting it down to the more eaſily; and this made us conceive, that the ſtring happening to be burned by the exceſſive heat of the iron, the piece of metal fell into the cavity of it, and, by the ſame heat, the more combuſtible part, which the chymiſts call the ſulphur, was melted and kept on fire, and continued burning ſo long as we have related. The piece of metal was judged to be lead, but having not formerly obſerved ſuch diſpoſition in that metal to be inflamed, I conſidered it attentively, and perceived, that it was ſome fragment, that the

operator had chanced to light on, of a mixture of lead and tin, that I had (a while before, for an experiment not at all belonging to our present subject) caused to be colliquated in a certain proportion. Upon whose account it seems, the mixture of the ingredients had acquired such a new texture, as, whether by making the bodies open one another, or by what other means soever, fitted the mass to afford us the phenomenon above recited. And though I made an unsuccessful trial with a mixture of lead and tin, to produce such a flame upon the heated iron in the open air; yet the newly related experiment may suffice to argue, that there may be flames of metalline sulphurs (as the chymists call them) that will be, at least, as easily produced without the concurrence of the air, as that of common sulphur, and continue to burn in our vacuum longer than it.

### THE THIRD TITLE.

## Of the strangely difficult Propagation of ACTUAL FLAME *in vacuo Boyleano.*

I HAVE more than once observed, that some bodies (whereof I make particular mention in another paper) though they will not be turned into flame by very intense heats, and those of very differing kinds, are yet very readily kindled by an actual flame. So that the propagation of flame to contiguous bodies, that, according to the hitherto observed, and unquestioned course of things, must thereby in a moment, as it were, be actually inflamed, seems to be not only very easy, but almost infallible; and yet, that this propagation is not easy, or is perhaps scarce possible to be performed without the assisting presence of the air, may be gathered from the next following experiments; at whose titles though you will probably be surprized, in regard that by the two first experiments of the first title of this tract, it will scarce be expected that sulphur should be kindled in our vacuum; yet I presume your wonder will cease, when I put you in mind, that I formerly took notice to you of my having sometimes met with such sulphur as would be kindled there; and it was whilst that well-disposed parcel of sulphur lasted, that I took the opportunity of making with the flame of it the trials to which I now proceed.

### EXPERIMENT I.

*An ineffectual attempt to make flame kindle spunk in an exhausted receiver.*

HAVING placed the often-mentioned cylindrical plate of iron, first brought to be red-hot, in a receiver, capable of containing two gallons of water; and having also diligently pumped out the air, we kindled a little sulphur upon the heated plate, and then  
a piece



a piece of dried spunk, tied to a string, was, by the help of a turning-key, let down to the flame; and when the experiment was finished, and the spunk was taken out, we found it in divers places, not manifestly altered so much as in colour; and in those parts that had been most exposed to the flame, it was turned to a substance very differing from ashes, being black and brittle as tinder, and, like it, exceedingly disposed to be kindled upon the touch of the fire.

## EXPERIMENT II.

*An unprosperous attempt to make flame kindle camphire without the help of air.*

As a farther confirmation of the difficulty of propagating flame in our vacuum, we may annex the following trials.

INTO the lately mentioned receiver we conveyed the cylindrical plate of iron made use of in the former experiment; and when the air had been diligently pumped out, we did, by the help of the turning-key, let down upon the hot iron a piece of such brimstone, as would, in spite of so disadvantageous a place, be kindled with that heat. A little above this sulphur we had tied to the same string a piece of camphire, that being a body exceedingly apt to take fire, if not, as it were to draw it, at the flame of lighted brimstone. But our sulphur melting with the heat of the iron cylinder, dropt unluckily from the string it was fastened to before, and for the most part fell off. And as soon as it came to the ground, where it was distant from the vehement heat of the metal, the flame expired, and that part of the sulphur that happened to stick to the side of the iron, was inflamed by it. And I that chanced to be then in an inconvenient posture for seeing the camphire, could not, because of the smoke of the extinguished brimstone, well discern what became of it. But my amanuensis that happened to be on the best side of the receiver, affirmed, he plainly saw the flame of the brimstone reached the camphire, without being able to make it flame. Which seemed the less to be doubted of, because the camphire was by help of the turning key let down low enough, and if it had afforded a flame, the difference of colours betwixt that and the blue flame of sulphur would have made it very easy for me to have distinguished them.

ANOTHER trial I would have thoroughly made to kindle one piece of sulphur in our vacuum by the flame of another, tied a little lower in the same string, that it might first touch the heated iron, and be thereby set on fire; but, though we could find nothing that was visibly amiss in the kind of sulphur we then used, yet we were not able, even by a reiterated trial, to make it take fire upon the iron, where nevertheless it melted and seemed a little to boil.

A THIRD trial was not so unsuccessful; for having in the well-exhausted receiver let down upon the very hot iron a match made of a piece of card dipped in brimstone, the lower extreme of it was kindled by the contact of the hot iron. But though the sulphurated part of the match thus flamed away, yet the remaining part, which was a mere piece of card, was not thereby turned into flame, nor in most places so much as sensibly scorched or blacked; though, as I remember, the match had been purposely dried before-hand to facilitate its inflammation.

## EXPERIMENT III.

*A strange experiment upon gunpowder, shewing, that though it were fired itself, yet it would not fire the contiguous grains in vacuo* Boyliano.

THE preceding trials may suffice to manifest the difficulty of communicating flame, without the help of air, from one body to another, even when the bodies to be kindled are of a very inflammable nature. But because there is no propagation of flame made in any bodies that we converse with here below, with any thing near such celerity, as in the contiguous grains of gunpowder; a great heap whereof will, almost in the twinkling of an eye, be turned into flame by propagation from any one small kindled grain; nothing seemed fitter to manifest how much flame is beholden to air, than if such an experiment could be made, as might shew, that, even amongst the contiguous grains of kindled gunpowder, flame would not be propagated without the help of air. How far a trial of this nature may be made in our engine, the following narratives will best declare.

WE took some paper, and laying it upon some convenient part of the plate of the engine, we made upon it a train of dry powder, as long as the glass would well cover; then, carefully fastening on the receiver with good cement, we solicitously pumped out the air; which done, we took a good burning-glass, and about noon cast the sun-beams through it upon the train of some gunpowder; where, though the indisposition to accension was so great, that the powder did not only smoke, but melt without going off, and the operator, though versed in such experiments, would not allow that it would signify any thing to continue the trial any longer; yet, upon my being obstinate to prosecute it, he, being willing to follow the experiment, rationally considered, that the receiver we had been hitherto fain to use, was so opacous as to resist the entrance of many of the beams that should have their operation upon the powder: whereupon taking a finer glass that was lately come in, we laid by the former, and employed that, which, by reason of its transparency, so little weakened the beams of the sun, that being, according to my direction, held obstinately upon the same parts of the train, they were able to fire several of them one after another. But though the sun could thus kindle the powder, yet it could not make the flame propagate, but only those parts that were melted, did at length kindle and fly away, leaving the rest unaltered, as I curiously observed, finding several little masses of colliquated matter in several places of the train, with the powder unchanged in all the other parts of the same train that lay in a direct line; besides that some of the little colliquated masses were contiguous to the rest of the powder, which appeared unchanged, and kindled readily, and flashed all away, as soon as I caused the burning-glass to be applied to it in the open air.

## EXPERIMENT IV.

*Reciting another attempt to confirm the former.*

FOR further confirmation of so odd an experiment, I shall also add a short account of another made with gunpowder in our vacuum.

To try, on an occasion that need not here be discoursed of, whether, by the help of one of those little instruments that are now used at *London*, to examine the strength of powder, we could find any difference made by the absence and presence of the air, in the



the resistance of the instrument, or the effects of the powder on it; we fastened it to a competently heavy and commodiously shaped weight of lead; and when it was carefully filled and primed with powder, we placed it in a receiver of a convenient bigness, whence we pumped out the air after the usual manner, and perhaps with more than usual diligence. But though, at length, after the powder had long resisted the beams of the sun, concentrated on it by a good double convex burning-glass, it did, as I expected, take fire at the touch-hole, and fill the receiver with smoke; yet this kindled powder could not propagate the flame to that which was in the box, how contiguous soever the two parcels were to one another. And when the instrument was taken out into the air (by which it appeared how free the touch-hole was) as soon as ever new-priming, with the same sort of powder, was put to it, the whole very readily went off; and when, for further satisfaction, we caused the instrument to be new charged, and upon its taking fire only at the touch-hole in the exhausted receiver, we ordered new priming to be added, without so much as taking the instrument out of the receiver, though afterwards the receiver was closed again, but without being exhausted of air; the powder, though closely shut up in the glass, did readily go off, as well that which was in the box or cavity of the powder-trier, as that which lay on the outward part of the instrument. And this trial, for the main, was repeated with the like success.

## EXPERIMENT V.

*Briefly mentioning two differing trials, with two differing events, to kindle gunpowder in our vacuum.*

You will easily believe, that the event of the foregoing trials seemed strange enough to the ingenious persons that I had desired to be present at them; and perhaps, the attentive consideration of it may well enough suggest such odd suspicions and conjectures, as I have neither the leisure, nor the boldness to discourse of in this place.

But here I shall not dissemble my having, by a somewhat differing way, made a couple of trials, whereof, though the first may confirm the great indisposition of gunpowder to be kindled in our vacuum, yet the second seems to look another way.

The first is summarily set down in my notes to this purpose. [A few small corns of gunpowder being included in a very small bubble freed from its air, and secured against the return of it, or any other, and then applied warily to coals covered with ashes, did not go off, nor burn, but afforded a little yellow powder that seemed to be sulphur, and sublimed to the upper part of the glass.]

The latter event I found in the same paper to have been thus registered. [But two larger bubbles, though strong, whereof one had the air but in part, and the other carefully emptied, being provided, each of them with a greater quantity of powder (though scarce enough to promise such an effect) a while after they were put upon quick coals, each of them was blown in pieces, with a report almost like that of a musket; but though this was done in a dark place, yet we did not perceive, whether or no there were any real flame produced.]

The event of this trial seems at first sight to contradict the inference, that probably you have drawn from the foregoing experiments. But yet it may not be unworthy of our inquiry, whether this way of trial be as proper to give satisfaction to the curious, as that made with the sun-beams was. And I leave it to be considered, whether or no it may not be doubted, whether the going off of the gunpowder was caused by a successive,

five, though extremely swift propagation of real flame, from the first kindled grains to the rest; or did not proceed from this, that the coals acting strongly at the same time on the whole area, or extent of the powder that was next to them, and this in the absence of the air, each grain was in that case, as it were, a little granado, and the heap of them, being uniformly enough acted on by the fire, they were made to go off, as to sense, all at once, as if there had been but a contemporary explosion made of them all together by the action of the external fire, rather than any true accension made by the flaming grains of the unkindled ones. As, I remember, I have tried, that even in the open air one may, with a burning glass dexterously employed, make some part of a little parcel of aurum fulminans go off, whilst the neighbouring parts of the same parcel to which the focus does not extend with heat enough, will not be made to do so.

## NEW EXPERIMENTS

ABOUT THE RELATION BETWIXT

AIR and the FLAMMA VITALIS of ANIMALS.

(Sent to the same Person to whom the former Papers were addressed.)

THE twenty experiments hitherto set down under the three foregoing titles, by shewing the relation betwixt air and flame in general, may be serviceable to the inquirers into the nature of the vital flame in particular. But yet having had occasion to make some trials that more directly regard the requisiteness of air to the flamma vitalis or vital principle of animals; I shall now present you by themselves, as many as I could light on, without being solicitous that they should be quite differing from each other; because in so new and nice a subject, the affinity that may be found between some, either in regard of the subjects exposed to trial, or in the manner of making it, may be useful, if not necessary, to confirm things by the resemblance of events, or make us proceed cautiously and distinctly in pronouncing upon cases where the success was not uniform.

EXPE-



EXPERIMENT I.

*Wherein the durations of the life of an animal, and of the flame of spirit of wine, included together in a close vessel, were compared.*

We took some highly rectified spirit of wine, and put about a spoonful of it into a small glass-lamp, conveniently shaped and purposely blown with a very small orifice, at which we put in a little cotton-wick, which was but very slender.

We also provided a tall glass-receiver, which was in length eighteen inches, and contained above twenty pints of water. This receiver, which was open at both ends, was at the upper orifice (which was not wide) covered with a brass-plate, fastened on very close with good cement, for uses whose mention belongeth not to this place; and for the lower orifice, which was far the widest, we had provided a brass-plate furnished with a competent quantity of the cement we employed to keep the air out of the pneumatical engine; by means of which plate and cement we could sufficiently close the lower orifice (though a wide one) of our receiver, and hinder the air from getting in at it.

THESE things being thus prepared, we took the small glass-lamp above-mentioned, and having lighted it, we placed both it, and a small bird (which was a green-finch) upon the brass-plate, and in a trice fastened it to the lower orifice of the receiver, and then watched the event; which was, that within two minutes (as near as we could estimate by a good minute-watch) the flame, after having several times almost quite disappeared, was utterly extinguished; but the bird, though for a while he seemed to close his eyes, as though he were sick, appeared lively enough at the end of the third minute; at which time, being unwilling to wait any longer by reason of some avocations, I caused him to be taken out.

AFTER he had for a pretty while, by being kept in the free air, recovered and refreshed himself, the former trial was repeated again, and at the end of the second minute, the flame of the lamp went out; but the bird seemed not to be endangered by being kept there a while longer.

AFTER this we put in, together with the same bird, two lighted lamps at once, viz. the former and another like it, whose flames, according to expectation, lasted not one whole minute, before they went out together. But the bird appeared not to have been harmed, after having been kept five or six times as long before we took off the receiver.

IN the tall receiver above-mentioned we included a mouse, with a lighted lamp filled with the spirit of wine; but before the experiment was near finished, the mouse, being at liberty within the glass, made a shift to blow out the flame; which being revived without taking out either the lamp or the animal, the spirit of wine burned about a minute longer, during which time the mouse appeared not to be grown sick, no more than it did afterwards, when, for some minutes after the extinction of the flame, he had been kept in the same close and infected air.

AFTERWARDS we placed the same mouse in another receiver, which seemed to be by a third part less capacious than the former, and in it we also fixed a piece of slender wax-candle, such as is wont to be made up in rolls, and employed to light tobacco. This candle continued burning in this new receiver but for one minute, during which time it emitted store of smoke; but this not hindering the animal to appear lively enough, even after we had kept him much longer in that infected air, the same candle, without being

taken out, was lighted again, but burned not so long as before; yet it sufficed to darken the receiver, and therefore probably much to clog the included air, in which, nevertheless, the mouse being kept, by our guests, eight or ten minutes longer, he appeared, neither when he was taken out, nor a while before, to have received any considerable harm by his detention there.

## EXPERIMENT II.

*Of the duration of the life of a bird, compared with the lasting of a burning candle and coal in our vacuum.*

WE took a green-fish and a piece of candle of twelve to the pound, and included them in a great capped receiver, capable of containing about two gallons, or sixteen pounds of water, which was very carefully cemented on to the pump, that no air might get in or out. In this glass we suffered the candle to burn till the flame expired (which it did in more than one trial, within two minutes or somewhat less); at which time the bird seemed to be in no danger of sudden death; and, though kept a while longer in that clogged and smoky air, appeared to be well enough, when the receiver was removed. Afterwards, we put the same bird into the receiver with a piece of a small wax taper, whose flame, though it lasted longer than the other, yet the bird outlived it; and it was judged he would have done so, though the flame had been much more durable. After this, we included the same bird with the first mentioned candle in the receiver, which we had caused to be often blown into with a pair of bellows, to drive out the smoke and infected air; and then beginning to pump out the air, we found, that the flame began more quickly to decay, and the bird to be much more decomposed, than in the former experiments; but still the animal outlived the flame, though not without convulsive motions. The experiment we repeated with a piece of the fore-mentioned taper, and the same bird; which, though cast into threatening symptoms upon the gradual withdrawing of the air, outlived, not only the flame, but the smoke too, that issued from the kindled wick, which circumstance was also observed in the preceding trial.

LASTLY, having freed the receiver from smoke, and supplied it with fresh air, we put in with the same bird a piece of charcoal of about two inches in length, and half an inch in breadth, which had been, just before it was put in, well blown with a pair of bellows, that it might be freed from ashes, and thoroughly kindled; and made haste to pump out the air. This diligence was continued not only till none of the fire could be discerned by any of the by-standers, but till, in our estimation (which the event justified) it was irrecoverable by the admission of the outward air; which at its coming in found the bird very sick indeed, but yet capable of a very quick recovery. And this experiment was, with the same animal and coal re-kindled, tried over again with the same success.

WHETHER this survival of animals, not only to a flame, that emits store of fuliginous steams, as in this trial, but to that, which is made of so pure a fuel as spirit of wine, that affords not such steams, as in the former experiment; whether, I say, this survival proceed from this, that the common flame and the vital flame are maintained by distinct substances or parts of the air; or, that common flame making a great waste of the aerial substance they both need to keep them alive, cannot so easily as the other find matter to prey upon, and so expires, whilst there yet remains enough to keep alive the more temperate



temperate vital flame; or, that both these causes, and perhaps some other, concur to the phaenomenon, I leave to be considered.

## EXPERIMENT III.

*Of what happened to the light of glow-worms in the exhausted receiver.*

For the sake of those learned men, that have thought the light of glow-worms and other shining insects to be a kind of effulsion of the biolychnium, or vital flame, that nature had made more luminous in these little animals than in others; and which a very eminent physician of the college of *London* affirms to have felt in a warm climate more than sensibly hot; I shall subjoin on this occasion some trials made on glow-worms, which else should be referred to those experiments of mine about the relation betwixt air and light, that you were formerly pleased to publish.

We took two glow-worms, that shone vividly enough, especially one of them, whose light appeared strong and tinted, as if it had been transmitted through a blue glass: these we laid upon a little plate, which we included in a small receiver of finer glass than ordinary, that we might the better see what would happen: and having for the same purpose removed the candles, that no other light might obscure that of the insects, we waited in the dark, till that was conspicuous, and then ordered the air to be begun to be pumped out; and, as we expected, upon the very first exsuction there began to be a very manifest diminution of the light, which grew dimmer and dimmer, as the air was more and more withdrawn, till at length it quite disappeared, though there were young eyes among the assistants. This darkness having been suffered to continue a long while in the receiver, we let in the air again, whose presence, as we looked for, restored at least as much light as its absence had deprived us of. This experiment was repeated with one more of those insects; and the event was, that they all three gradually lost their light by the exhaustion of the receiver, and regained it, with some increase (as was judged) by the return of the air. And in this experiment we let in the air by degrees, and with an interval or two, to observe, as we did, that, as the diminution of light was greater and greater, when the air was more and more withdrawn, so the returning splendor was gradually increased, as we pleased to let in more and more air upon the worms.

## EXPERIMENT IV.

*Containing a variation and improvement of the foregoing trial.*

But here I foresaw, it might be suspected, that the disappearing of the light in our exhausted receiver did not so much proceed from any real, though but temporary, extinction or eclipse of it, as from this, that the glow-worms having, as I have often observed, a power of drawing the luminous part into the opacous part of their body, they might, finding themselves prejudiced by the withdrawing of the air, hide their light from our eyes, without losing it, till being again refreshed by the return of the air, they might be invited to protrude it again into the transparent part of their tails. This scruple seeming grounded upon the nature of the thing, I thought it worth while to remove it by the help of another observation, that I long since made, and have men-

tioned elsewhere about glow-worms; which is this, that if they be killed whilst they are shining, their luminous matter may continue to shine for a good while after it is taken out of their bodies; and accordingly having put some of that we took out of the forementioned insects, upon a little paper, and included it in the receiver we employed, the candles being removed, we perceived it to shine vividly enough before the pump was set on work, and afterwards to grow dimmer and dimmer, as the air was more and more drawn out, till at length it quite vanished; and it re-appeared immediately upon the air's return. This experiment was reiterated twice more with the same success for the main. But we took notice, that the luminous matter, after the air was let in, seemed to us not only to have regained its former degree of light, but sensibly encreased it (as it once happened also in the experiment made on the living worms) which, whether it was caused by any real change made by the recess and access of the air in the matter itself, or by the greater accustomance of our eyes to the darkness of the place, I dispute not; and shall only add this phaenomenon of one of our trials, that having a mind to see, whether a very little proportion of returning air would not suffice to restore some little light to the disappearing matter, it was somewhat strange to observe, that so very small a quantity of air, as was let in before the light was revived, was enough to make it become plainly visible, though but dim; in which state it continued, till we thought fit to let in more air upon it. Farther trials I could not make with these glow-worms, having received them but that night out of the country, and being the next morning to begin a journey.

## EXPERIMENT V.

*Wherein the former enquiry is farther prosecuted.*

AFTER the lately mentioned trials we made with the glow-worms, having procured two or three other of those insects, whereof one was judged to be as large as three ordinary ones, we found when we had brought them out of the country to *London*, that this great worm was dead, as far as we were able to judge, and finding him to retain a considerable degree of luminousness in the under part of his tail, we put him into the small receiver formerly mentioned, to try whether after the death of the animal, the shining matter would retain its former properties; but at the first time the air was pumped out after the usual manner, the light was not only not abolished, but continued vivid enough, and so it did when the air being let in, and again withdrawn, the trial was made a second time. But being unwilling to abandon the experiment till we tried it yet a little further, I caused the receiver to be exhausted yet once or twice more, and at length I perceived, that the light began to diminish, as the air was withdrawn; and last of all, it so disappeared, that the by-standers could not see it, whereas upon the re-admission of the air, the light shone vividly as before, if not more bright. This experiment was reiterated with the like success, and in both these times the like happened to the light of the dead one, and of a living one that we included with it, to be able to compare them together; though there were this disparity betwixt them, that the luminous part of the dead worm was much larger than that of the living, and the light of the latter appeared of a very greenish blue, whereas that of the former seemed to be of a white yellow.



EXPERIMENT VI.

*Made to examine whether animals be heavier dead than alive.*

It is a received tradition, that bodies when dead are much heavier than the same were when alive: the matter of fact being taken for granted, some will perhaps ascribe the change to the utter inability of a dead body any way to assist those that endeavour to remove it. But, according to the general opinion, this difference proceeds from the total extinction or recess of the spirits vital and animal, which being supposed to be not only agil but light, lessened the weight of the body they enlivened; and flame being conceived to be the lightest among bodies here below, it is not improbable, that some will ascribe the phenomenon to the levity of the flame, which by being diffused through the body of an animal, and vivifying it, deserves the name of vital. But I would not advise any to rely on this conceit, till they are duly satisfied of the truth of the matter of fact; which because I have not yet found, that any has endeavoured to try, I shall on this occasion give you the following transcript of one of my notes about statical experiments.

A MOUSE, weighing about three drachms and a half, being put in one of the scales of a very nice balance, was counterpoised together with a string, that was tied about his neck like a noose, and after a while, by drawing the ends of it, was there strangled. As soon as we judged him quite dead, we weighed him again, and though nothing was seen to fall from him; yet, contrary to the received tradition, that bodies are much heavier dead than alive, we found the weight to have left about  $\frac{7}{10}$  of a grain; which probably proceeded from the avolation of divers subtile particles upon his violent and convulsive strugglings with death. But this was no more than an experiment of this kind, made some years ago, induced me to expect and foretel:

AFTERWARDS in a larger balance, but a very good one, purposely made for nice experiments, we took a very young catlin, of between ten and eleven ounces in weight, and caused him to be strangled on the same scale, wherein he had been put. But he could not be dispatched so soon as an ordinary full grown animal; so that by that time he was quite dead, we found him not only not to be grown heavier, but lighter by four grains, which did not much surprize us, having elsewhere noted the life of so very young creatures of that kind not to be easily destroyed for want of respiration. And I remember, that, for trial's sake, another catlin of the same litter with this I have mentioned, being included in a receiver, wherein another animal of that size might probably have been dispatched in two or three minutes by the pumping out of air, was kept there somewhat above a quarter of an hour before he appeared to be quite dead.

ADVERTISE-

## A D V E R T I S E M E N T.

THESE two following attempts falling into the hands of the author after the preceding experiments were printed, it was thought fit to annex them here for the affinity of the subject.

## An ATTEMPT to produce LIVING CREATURES in *Vacuo* Boyliano.

**I**N reference to the opinion of those naturalists, that hold the seeds of living creatures to be animated, and especially to the hypothesis of those learned men that assert the *flamma vitalis* lately mentioned; it may be an enquiry of moment, whether or no in the seminal principles, or rudiments of animals, the manifest operations of life may be excited without the concurrence of the air, whose interest in the production and conservation of flame may be gathered from the foregoing experiments. For, it seems likely to prove no inconsiderable discovery in reference to the lately mentioned hypothesis, if it be found, that the principle of life in seminal rudiments needs, as well as other flames, the concurrence of the air to actuate it.

I THOUGHT fit, therefore, notwithstanding the great and almost insuperable difficulties, which it was easy enough for me to foresee I should meet with, to attempt the hatching of eggs in our vacuum: but though I made some unsuccessful trials of this kind, in order to a discovery about respiration (not here to speak of the attempts I made about the animation of putrid matter) yet leaving the mention of them to its proper place, I shall only take notice in this, what directly concerns the present inquiry. Considering then, that pregnant females cannot be made to live and bring forth young in our exhausted receiver, and that the eggs of birds, and such greater animals, do, in this colder climate of ours, require to be hatched by the incubation of the females, or other birds; I thought the fittest subjects I could both make choice of, and procure for the designed experiments, would be the eggs of silk-worms. For, having many years since tried several things about those insects, and among others found, that their eggs would be hatched, not only by the heat of one's body (though that be the usual way) but by the warmth of the sun even here in *England*; if they be kept till the spring be far enough advanced: remembering this, I say, I got a good number of silk-worms eggs; and having caused three conveniently shaped, but very small receivers, to be purposely made, that differed very little (and that accidentally) either in size or figure, we conveyed into each of them, together with a small stock of mulberry-leaves, such a number of eggs, as we thought sufficient to make one morally secure, that at least some of them were prolifick: this done, we carefully exhausted one of them, and secured it against the return of the air; the two others we left full of air: but having left in one a little hole for the air to come in and get out at, we stopped the other so close, as to hinder all intercourse between the included air and the external. All things being thus prepared, we exposed the receivers to a south-window, where they might lie quiet, and where I either came, or sent to look on them from time to time; the spring being then



so far advanced, that I supposed the heat of the sun would be of itself sufficient to hatch them in no long time.

As to the success of this trial, my not being able to find any register of the particular phenomena that occurred, keeps me from venturing to relate it very circumstantially; but this I remember in general, that both I and others took notice, that in the unexhausted receivers there were divers eggs hatched into little insects, that perforated their shells, and crept out of them; though afterwards, for want of change of food, or air, or both, few or none of them proved long-lived. But though the eggs in these receivers began to afford us little animals in a few days; yet the eggs in the exhausted receiver did not, in many more, afford us any. And though I will not venture to say, how long precisely we kept them in the same window, after some of the above-mentioned eggs were hatched; yet (if I much mistake not) it was, from first to last, about three or four times as long; and I remember, we kept them till it was thought to no purpose to wait any longer, and agreed in imputing the not hatching of the eggs by the so long continued action of the sun to the absence of the air.

What other phenomena occurred to us in making this experiment, and another not unprosperous one upon the eggs of flies, you may expect, when I can light on my notes about them, or have my memory refreshed by those that assisted at the making of them.

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## An ATTEMPT made upon GNATS in our Vacuum.

ELSEWHERE mention, that it has been observed by a couple of our virtuosi (whom I there name) and several times by me, that here in *England* multitudes of gnats are generated of little animals, that live, for a part of the summer, like fishes in the water; and considering, that by these a very unusual passage is made from swimming to flying animals, I thought them very fit subjects, whereon to make the following experiment.

[PARTLY to try, whether at least an animal already living and moving in our vacuum may be able to attain the perfection due to it, according to the course of nature; and partly to examine, whether, in case he should attain it, at least the lighter sort of winged insects, may be able to fly in that place; and partly to discover, whether an animal, that had long lived in our vacuum, would, when turned to a fly, be able to continue alive without respiration, he had never been accustomed to, in its pristine form or state; we took divers of those little swimming creatures, which in autumn, especially towards the end of it, are wont to be turned into gnats, and having put a convenient number of them together in a fit quantity of rain-water, wherein they had been found and kept, into a small receiver, the air was pumped out, and the vessel secured against its return, and then set aside in a place, where I could observe, that the day after some of these little animals were yet alive and swimming to and fro, not without minute bubbles adhering to them; but at the end of a day or two after that, I could not perceive any of them to survive their dead companions, nor did any of them recover, when fresh

fresh air was let in upon them. But though this experiment were the best I was then able to make, yet I resolved, if God should vouchsafe me life and health, to repeat it the ensuing autumn; that, wherein it was made, proving so cold and unseasonable, that a number of these little creatures, put up with water into another small receiver, died all within a few days, though none of the air was exhausted; and several, that I kept in an ordinary glass, that was divers times unstopped to give them fresh air, did yet perish at no ordinary rate. And I confess (as unkind as this trouble of mine may seem to the air) that the failing of this and some other experiments of producing animals in our exhausted receivers was the more unwelcome to me, because I had, and have still a great desire to see, if it be possible, what would happen to animals, which had been produced in a place free from the pressure of the atmosphere, as if they had been born in *Epicurus's* imaginary intermundane spaces, upon their coming to be suddenly surrounded with our heavy air, and having their tenderly framed bodies exposed to its immediate pressure.]

## NEW EXPERIMENTS

ABOUT

## EXPLOSIONS.

(Annexed, by way of APPENDIX, to the former PAPERS.)

**F**ORASMUCH as some of the learned men, that are the grand assertors of the *flamma vitalis* (whose opinion occasioned my presenting you the foregoing experiments) do also, with the justly famous *Dr. Willis*, explicate many of the motions of animals, especially those performed in the muscles, by the explosions made of certain juices or fluid substances of the body, when they come to mingle with each other: and forasmuch also as I do not remember, I have heard the maintainers of this hypothesis insist on other instances in favour of it, than the going off of gunpowder; which being not a liquor, but a consistent and brittle body, and requiring for its explosion either actual fire, or a far intenser heat, than can be supposed natural in men, and other animals; I was induced to suspect, they were not yet provided with better examples; and therefore I presume, it will be looked upon, as a thing neither useless, nor altogether impertinent, if, without offering to determine any thing about the truth of the opinion, I supply the embracers of it with two or three examples of explosions made by the bare mingling of liquors; which I shall borrow from the elsewhere-mentioned notes, that I drew up some years ago, in order to the improvement of some parts of physick.

EXPE-



EXPERIMENT I.

*Of an explosion made with the spirits of nitre and wine.*

WE took spirit of nitre, so strong, that the fumes made the upper part of the glass it was kept in, always reddish; and having put but one ounce of it into a bolt-head, with a long neck, capable to contain, as we guessed, twelve or sixteen times as much, we caused an equal weight of alcohol, or highly rectified spirit of wine, to be taken, and a little of it being put to the spirit of nitre, it presently made so strong and quick an expansion or explosion, that some of it flew out of the glass, and hit against the ceiling of the room (where I saw the mark of it) and falling upon his face that held the glass, made him think (as he told me) that fire had fallen upon it, and made him run down the stairs like a madman, to quench the heat at the pump. Wherefore, bidding the laborant proceed more warily, I ordered him to put into the bolt-head but part of a spoonful of spirit of wine at a time; and yet, at each of a pretty many affusions that I staid to see the effect of, there would be a great noise, as of an ebullition, though no store of froth produced, and accompanied with so great a heat, that I could not hold the glass in my hand; and immediately there would issue out a copious and red smoke; to which when I caused a little candle to be held, though at near half a foot distance from the top of the bolt-head, it would presently take fire, and burn at the top of the bolt-head like a flame at the upper end of a candle, till I caused it to be blown out, that fresh spirit of wine might be poured in; which, when it was all mingled with the other liquor, the heat and conflict ceased.

DIVERS other phenomena relating to this experiment (by which I intended to make out more things than one) belong not to our present subject, and are already set down in other papers. But yet it will be pertinent to shew in this place, that the noise and ebullition produced in this mixture is not unaccompanied with a briskly expansive, or an explosive motion. To make then an experiment to this purpose, and yet avoid the danger, whereto the making of it unwarily might expose both the vessels and us, we put an ounce of such strong spirit of nitre, as is above-mentioned, into a moderately large bolt-head furnished with a proportionable stem, over the orifice of which we strongly tied the neck of a thin bladder; out of which most part of the air had been expressed, and into which we had conveyed a small phial, with a little highly rectified spirit of wine: then this phial, that before was closed with a cork, being unstopped, without untying or taking off the bladder, a small quantity, by guess not a quarter of a spoonful, of the alcohol of wine, was made to run down into the spirit of nitre, where it presently produced a great heat and commotion, and blew up the bladder, as far as it would well stretch, filling also the stem and cavity of the glass with very red fumes, which presently after forced their way into the open air, in which they continued for a good while to ascend in the form of an orange-coloured smoke.

EXPERIMENT II.

*Of an explosion made with oil of vitriol and oil of turpentine.*

If I had at hand the papers, you have divers times heard me speak of, about heat, I could give you the particulars of some trials about explosion, that perhaps you would

*New Experiments about Explosions.*

think more pertinent than despicable: but, for want of those papers, I must content myself to tell you in general, that I remember, that I have, more than once, taken strong oil of vitriol, and common oil of turpentine, and warily mixed them in a certain proportion, by shaking them very well together; and that thereupon ensued (what I had reason to look for) so furious an agitation of the minute parts of the mixture, and so vehement or sudden expansion or explosion, as did not only seem strange to the spectators, but would have proved dangerous too, if I had not taken care before-hand, that the trials should be made in a place where there was room enough; and that even the operator, that shook the vessel, should stand at a convenient distance from the mixture.

## EXPERIMENT III.

*About an explosion made by two bodies actually cold.*

I REMEMBER not, that I found the assertors of explosions in animals to have taken notice of a difficulty, which to me seems not uneasy to be observed, and yet very worthy to be cleared. For it is known, that fishes, and those especially of the vaster sort, can move and act in the waters with a stupendous force; and yet it is affirmed by those that pretend to know it, that the blood of most fishes is still actually cold: and I remember, I found the blood even of those I dissected alive to be so. From whence most men would argue, that even in the vast sea-monsters there can be made no explosions, these being still effected by or accompanied with an intense degree of heat.

It were incongruous to my design to examine this difficulty, as it directly regards the explosions said to be made in animals: but speaking of explosions in general, perhaps I might do the favourers of vital ones (if I may so term them) no unacceptable piece of service, by experimentally shewing that it is not impossible, though it seem very unlikely, that explosions should be made upon the mixture of bodies, which, whilst they seem to put one another into a state of effervescence, are really cold, nay, colder than before their being mingled. Of these odd kind of mixtures, I remember I have in another \* paper set down some trials, that I made to other purposes, as well with two liquors, as with a liquor and a solid body; which later sort I there mention my having made by an improvement of an experiment of the excellent Florentine virtuosi. And among those trials I find one, whose pertinency to the matter in hand invites me to annex as much of it, as is proper in this place.

THERE were put two ounces of powdered sal armoniac into a pretty large glass-tube, hermetically sealed at one end; into the same a slender glass-pipe, furnished with two ounces of oil of vitriol, was so put, that, when we pleased, we could make the liquor run out into the larger tube, which, after these things were done, was closed exactly, so that nothing might get in or out. My design was, that this instrument should be so warily inverted, that the operator might get out of the way, and the oil of vitriol falling slowly upon the sal armoniac, should, without producing any heat, produce an explosion not dangerous to the by-standers. But whilst I was withdrawn to a neighbouring place to write a letter, the operator not staying for particular directions, rashly inverted the instrument, without taking care to get away; whence it happened, that as soon as ever the contained liquor, being too plentifully poured out, came to work on

\* About the production or extrication of air.



the fal armoniac, wherewith it is wont to produce cold, there was so surprizing and vehement an expansion or explosion made, that with a great noise (which as the laborant affirmed, much exceeded the report of a pistol) the glasses were broken into a multitude of pieces, many of which I saw presently after, and a pretty deal of the mixture was thrown up with violence against the operator's doublet and his hat, which it struck off, and his face; especially about his eyes, where immediately were produced extremely painful tumors, which might also have been very dangerous, had I not come timely in, and (to add that upon the by) made him forthwith dissolve some saccharum Saturni in fair water, and with a soft sponge keep it constantly moistened by very frequently renewed applications of the liquor: by God's blessing upon which means, within an hour or two, the pain, that had been so raging, was taken away, and the fretting oil of vitriol was kept from so much as breaking the skin of the tumors that it had made.

THE first part of the relation of this trial might have been omitted, or at least shortened, unless I had designed to communicate unto you a way of doing what I do not know to have been attempted by others, namely to put bodies together, when and by what degrees one pleases, after the glaſs, that contains them, has been hermetically sealed up; which mechanical contrivance, especially as it may be varied, may be, as I have tried, usefully applied to more purposes, than it were proper here to take notice of.

BUT to conclude with a word or two touching the foregoing experiment, I shall only add, that another time we made a like trial a safer way, by tying a bladder so to the top of a bolt-head, into which we had before-hand put the fal armoniac, that, by warily moving the bladder, whence the air had been expressed, we could make some of the fal armoniac, we had lodged in its folds, to fall upon the liquor, with which it presently made an explosive mixture, that quickly blew up the bladder.

BUT these, Sir, are bare conjectures, left to be, after a farther discussion (if you think them worthy of it) determined by you, to whom as these papers are addressed, so they are also submitted by the writer of them.

I am, S I R,

Yours, &c.

A N

# HYDROSTATICAL DISCOURSE,

OCCASIONED BY THE

OBJECTIONS of the Learned Dr. *HENRY MORE*,

AGAINST SOME

EXPLICATIONS of New EXPERIMENTS made by Mr. *BOYLE*;

A N D N O W

Published by way of PREFACE to the three ensuing TRACTS.

To the R E A D E R.

**W**HEN I determined to write this polemical discourse, I did not forget, that when I first ventured some of my trifles abroad into the world, my friends obtained from me a promise, that after I should have answered the two first, that should expressly write against me (which happened to be the learned *Linus* and Mr. *Hobbes*) to shew that I was not altogether unacquainted with a way of defending truths, I would afterwards write no book in answer to any that should come forth against mine; for, not only my friends, but I, thought it enough for a person, that never was a gown-man, to communicate freely his thoughts and experiments to the curious, without despairing, that those things that should be evidently true, would be able to make their own way, and such as were very probable, would meet with patrons and defenders, in so inquisitive



sitive an age as ours. And indeed I do not find that either upon the account of my writings, or ingenious men's opinion of them, I have had much cause to repent the keeping of my promise, notwithstanding the writings that have impugned some of mine, but without much prejudice, that I know of, either to the proposed truths, or the proposer of them. And therefore I should not at all have entered upon defence of what is attacked of mine by the learned Dr. More, if I had not supposed, that it would not require a book, but might be dispatched in a preface: for having by me some little tracts that should, though the doctor had never engaged me, have been imparted to the publick, and observing that the new experiments contained in one or other of them would by an easy application be brought to confirm my formerly delivered explications of other phenomena, and enervate the doctor's objections against them; I thought I might, without long troubling the reader, or myself, defend what I looked upon as truth, by answering some incidental passages of the doctor's discourse, and referring the reader, for the main points in controversy between us, to those experiments of the following tracts, which clearly contain the grounds of deciding them. But yet this consideration would not perhaps have engaged me to write the following preface, if the objections I was to answer had not been by a person of so much fame, proposed with so much confidence, and though with very great civility to me, yet with such endeavours to make my opinions appear not only untrue, but irrational and absurd, that I feared his discourse, if unanswered, might pass for unanswerable, especially among those learned men, who, not being versed in hydrostaticks, would be apt to take his authority and his confidence for cogent arguments; and who (not observing how liberal some men are of titles to the arguments, that please them) would make a scruple of thinking, that what is with great solemnity delivered for a demonstration in a book of metaphysicks, can be other than a metaphysical demonstration. The care therefore, that what I judge to be true, should not be made to pass for absurd, which is a degree beyond what is merely erroneous, by being so severely handled by a person of Dr. More's fame and learning, induced me to begin the following paper; which should have been shorter than now it is, but that I was persuaded to lengthen it beyond what was either necessary or designed, that I might, by the addition of some few thoughts and experiments on the occasions that were suggested to me, endeavour to clear up and confirm some hydrostatical truths, that, I fear, are but by very few either assented to, or perhaps so much as understood, and so might make the reader amends for the trouble I was forced to give him in a dispute, which I apprehended he might otherwise think himself but little concerned in. And he will, I hope, easily discern, that I have no mind to burthen him in my preface with things not pertinent to the scope of it, if he take notice, that both for his sake and the learned doctor's (whose civility I would not leave unanswered) I have restrained myself to the defensive part, forbearing to attack any thing in his *Enchiridium Metaphysicum*, save the two chapters, wherein I was particularly invaded.

BUT though I have declined the delivering my opinion of the doctor's book, yet I dare not forbear owning my not being satisfied with that part of his preface, which falls foul upon Monsieur *des Cartes*, and his philosophy. For though I have often wished, that learned gentleman had ascribed to the divine Author of nature a more particular and immediate efficiency and guidance, in contriving the parts of the universal matter into that great engine we call the world; and though I am still of opinion that he might have ascribed more than he has to the supreme Cause, in the first origin and production of things corporeal, without the least injury to truth, and without much, if any, prejudice to his own philosophy; and though not confining myself to any, yet, I do

not

not profess myself to be of the Cartesian: yet I cannot but have too much value for so great a wit as the founder of it, and too good an opinion of his sincerity in asserting the existence of a deity, to approve so severe a censure, as the doctor is pleased to give of him. For I have long thought, that in tenets about religion, though it be very just to charge the ill consequences of men's opinions upon the opinions themselves; yet it is not just, or at least not charitable, to charge such consequences upon the persons, if we have no pregnant cause to think they discern them, though they disclaim them. And since men have usually the fondness of fathers for the offspring of their own brains, I see not, why *Cartesius* himself may not have overlooked the bad inferences that may be drawn from his principles (if indeed they afford any such) since divers learned, and not a few pious persons, and professed divines of differing churches, have so little perceived, that the things objected are consequent to such principles, that they not only absolve them as harmless, but extol them as friendly and advantageous to natural religion. And I see not, why so great and radiant a truth, as that of the existence of a God, that has been acknowledged by so many mere philosophers, might not as well impress itself on so capable an intellect, as that of Monsieur *des Cartes*; or that so piercing a wit may not really believe he had found out new mediums to demonstrate it by. And since the learned *Gassendus*, though an ecclesiastick, had been able, as well safely, as largely to publish the irreligious philosophy of *Epicurus* himself; it seems not likely, that so dexterous a wit as that of Monsieur *des Cartes* could not have proposed his notions about the mechanical philosophy, without taking so mean a course to shelter himself from danger, as in the most important points, that can fall under man's consideration, to labour with great skill and industry to deceive abundance of ingenious men, many of whom appeared to be lovers of truth, and divers of them lovers of him also. And I am the more averse from so harsh an opinion of a gentleman, whose way of writing, even in his private letters, tempts me very little to it, because I cannot think him an atheist, and an hypocrite, without thinking him (what Dr. *More* has too much celebrated him) to call him a weak head, and almost as bad a philosopher as a man. For, as far as I understand his principles, some of the most important points of his philosophy (which, if it were needful, I could name) are interwoven with the truth of the existence of a God, or do at least suppose it, and are not demonstrable without it. But I must not prevent the Cartesians, who, now he cannot do it for himself, I doubt not will apologize for their master; though looking upon him as a great benefactor to, though not the first founder of the mechanical philosophy, I could not consent, by a total silence upon such an occasion, to become any way accessory to the blemishing of his memory.



A N

H Y D R O S T I C A L  
D I S C O U R S E, &c.

S I R,

UPON the advertisement you gave me yesternight, that I was particularly concerned in the learned Dr. *More's Enchiridium Metaphysicum*, I this day turned over the leaves of one, which I have freshly received from the reverend author himself; and and being assisted by the series of the titles, I quickly lighted on that part of the book, whose subject made me expect to find myself questioned there, as I presently found I was. For though that civil adversary is pleased to omit my name, and, the farther to disguise it, employs, instead of it, a great and unmerited encomium; yet by the book he cites, and the experiments against which he argues, it is very easily discoverable, that his objections are meant against me, who see yet no cause at all to be scrupulous to own my name, and the doctrine delivered in the passages he is pleased to oppose.

I DOUBT not but you will presently desire to know what I think of this much expected work; but when I have told you, that I have gained time to peruse only (and that but cursorily) the twelfth and thirteenth chapters, you will, I question not, excuse a person that does exceedingly want health, and yet wants not almost continual avocations, if I now content myself to give you my thoughts of that part of the newly-mentioned chapters, which properly relates to me; I say, that part of the chapters, because there are others, wherein I need not interest myself. For, to omit other paragraphs, the doctor has, in the former part of the twelfth chapter, thought fit to separate from my explication of the phenomena in question betwixt us that of the learned *Henricus Regius*; and the latter part of the same chapter he employs in an ingenious dispute against those that would have the aerial particles act with perception and design, and (as he speaks) *pro ne rata*; which opinion you will easily believe I neither was of, nor am like Sect. 16, 17.

It remains then, that setting aside those discourses of the twelfth chapter, wherein it is needless, that I should make myself a party; I proceed to consider those paragraphs, which will be easily guessed to be levelled at my explications, and by which I must confess, I cannot at all be yet convinced of their being false ones. But in doing this, I shall not only, in compliance with my present haste, but also to express my respect to the learned doctor, forbear to say any more, than what I shall judge requisite to answer the objections that directly concern my own explications, without meddling, by way of retaliation, with his hypotheses or opinions, or endeavouring to set any passages of his writings at variance among themselves, or to take those little advantages, which are usually fought for by disputants.

I SHALL

I SHALL not trouble you, nor tire myself with any schemes, since the doctor has taken the pains to insert those, that are necessary for his purpose, in his book, and I have not my own at hand. Wherefore, not doubting, that you have by you those books of mine he refers to, and supposing, that you will, whilst you are reading, have also this book, with the inserted schemes before your eyes, I shall not spend time on any further preamble, but immediately enter upon the consideration of the objections I am to answer.

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## THE FIRST SECTION.

### C H A P. I.

THE first explication of mine, that the learned doctor animadverts upon in his twelfth chapter, is that which I give in the thirty-third of my physico-mechanical experiments, touching the spring and weight of the air; where I relate, that the sucker in the air-pump of our engine, having been forcibly depressed to the lower part of the brass cylinder, which yet was carefully closed at the top, so that the cavity of the cylinder was empty of air; this sucker, I say, would, in this case, appear spontaneously to remount towards the top of the cylinder, though it were clogged with a hundred pound weight to hinder its ascent. Which phenomenon I ascribed to this, that the sucker being, by the withdrawing of the air in the cylinder, freed from the wonted force of the springy air, that endeavoured to depress the internal part of it, was not enabled, by the appendant weight, to resist the pressure of an atmospherical cylinder equal in diameter to it, which, pressing against its lower or external surface, endeavoured to impel it up.

Now the doctor having, in the two first paragraphs, made a description of my engine, (which I shall now pass over) does in the third teach us, that the corporeal cause, if there be any, of the ascent of the sucker, must be either in the sucker itself, or in the almost exhausted cavity of the cylinder, or, lastly, in the external air. Which premised, he does in the same third section, and in the fourth, endeavour to prove at large, that the cause is to be derived neither from the one, nor from the other of the two first. And therefore I, that maintain neither of the opinions he disputes against, shall leave those paragraphs of his untouched. Nor shall I meddle with the fifth, sixth, and seventh, where he argues against the explication of some, that would solve the phenomenon upon some Cartesian grounds, and as well amply, as particularly against the solution, that he supposes would be given of it, congruously to his own sentiments, by the learned *Regius*. These discourses, I say, of the doctor's I leave untouched; because it is at length in the eighth paragraph, that he impugns that solution of the phenomenon, which he ascribes to me, whose opinion he first delivers, though not just in the terms I would express it myself; yet I dare say very sincerely, and so near my sense, that I shall forthwith pass from the eighth section to the beginning of the ninth, where he begins to propose his objections, which he is pleased to usher in with a compliment



pliment to me, that I should be very vain, if I looked upon as any thing more than a compliment.

To his first objection, proposed in these words, *Primò enim, si hæc solutio verè mechanica sit, quæ tandem causa verè mechanica assignari potest gravitationis singularum particularum, totiusque atmosfæræ in suis locis? nam quod materiam subtilem attinet, &c.* I answer, that I did not in that book intend to write a whole system, or so much as the elements of natural philosophy; but having sufficiently proved, that the air we live in, is not devoid of weight, and is endowed with an elastical power or springiness, I endeavoured by those two principles to explain the phænomena exhibited in our engine, and particularly that now under debate, without recourse to a fuga vacui, or the anima mundi, or any such unphysical principle. And since such kind of explications have been of late generally called mechanical, in respect of their being grounded upon the laws of the mechanicks; I, that do not use to contend about names, suffer them quietly to be so: and to entitle my now examined explication to be mechanical, as far as I pretend, and in the usual sense of that expression, I am not obliged to treat of the cause of gravity in general; since many propositions of *Archimedes*, *Stevinus*, and those others that have written of staticks, are confessed to be mathematically or mechanically demonstrated, though those authors do not take upon them to assign the true cause of gravity, but take it for granted, as a thing universally acknowledged, that there is such a quality in the bodies they treat of. And if in each of the scales of an ordinary and just balance a pound weight, for instance, be put; he that shall say, that the scales hang still in æquilibrium, because the equal weights counterpoise one another; and in case an ounce be put into one of the scales, and not into the opposite, he that shall say, that the loaded scale is depressed, because it is urged by a greater weight than the other, will be thought to have given a mechanical explication of the æquilibrium of the scales, and their losing it, though he cannot give a true cause, why either of those scales tends towards the center of the earth. Since then the assigning of the true cause of gravity is not required in the staticks themselves, though one of the principal and most known of the mechanical disciplines; why may not other propositions and accounts, that suppose gravity in the air (nay prove it, though not *à priori*) be looked on as mechanical?

## C H A P. II.

THE next thing the doctor opposes to my explication, is a resolute denial, that there is any such gravitation as I pretend, of bodies, or their particles, in their proper places. But because, for the proof of his negation, he refers us to the next chapter, we shall hereafter have a fitter place than this to consider it in.

THIRDLY, he tells us, we may justly doubt of the equal diffusion of the springy power, or the pressure of the air every way. In what sense, in some cases, I admit of a small inequality between the pressure of fluids against differing parts of a surrounded body, I have \* elsewhere declared, and need not here discourse of; since in the case before us, and in the like, that pressure is inconsiderable enough to be safely neglected. And whereas our author thus argues, *Semotâ vi elasticâ, particule tamen atmosfæræ deorsum tenderent. Est igitur depressio quædam deorsum præter vim elasticam ipsi superaddita; sursum non item, sed elastica sola, estque suppar ratio in pressionibus transversis &*

\* See the Hydrostatical Paradoxes, especially Parad. 7.

*obliquis*: I presume he did not sufficiently consider our hypothesis and the nature of the pressure of fluid bodies, that have weight: for water, to which no springiness is ascribed, as there is to air, but which acts by its weight and fluidity, is able, upon the score of those qualities, to buoy up great ships, that the ebbing tide often leaves upon the strand.

Page 139. AND whereas the learned examiner proposes a fourth objection in these terms; *Quibus omnibus addas, difficile esse intellectu, si unius cylindri atmosphæe pondus æqualis diametri cum embolo reflectione in fundum emboli derivetur, cur non quinque alii cylindri aeris, qui circumstant embolum, in ejus fundum eodem modo simul agere possunt, ita ut vis sursum impellens embolum sextuplo major sit, quàm hactenus ab hujus opinionis fautoribus existimata est. Quod si sit, tunc certè, siquo artificio fieri possit ut unius solius cylindri actio in embolum admitteretur, reliquorum quinque exclusa, & pari tamen facilitate embolus ascenderet, sed totam hypotesin ingeniosam tantummodo esse fictionem.* I presume, hydrostaticians will think this might have been spared. For they will tell him, that there can be no more of a fluid press directly upward against the cylindrical orifice of a body immersed in that fluid, than a cylinder of that fluid of the same diameter with the orifice, the lateral pressures bearing against the lateral parts of the cylinder. And therefore if you invert, for instance, a pipe open at both ends, and filled to a height with oil, with common water; the oil, that is kept up by the pressure of the water upwards, will keep at the same height as to sense, whether the vessel, that contains the water, be broad or narrow, provided it be somewhat larger than the orifice of the pipe.

AND now, to invalidate yet further the precedent objections made by the doctor, I shall add, that it need not be thought incredible, that the atmosphere by its weight, or the spring of the air compressed by that weight, should be able to raise up four-score or an hundred pounds, hanging at the sucker; since I have \* manifested two or three years ago, by a clear and cogent experiment, that a little air in a bladder will, by its mere spring, be able to heave up a weight of a hundred pounds, and this without the help of any rarefaction by heat. By which experiment may be also confirmed, what I delivered a while since about the endeavour of the air, that is wont to be included in our brass cylinder, by expanding itself to thrust away the sucker (which, in regard of the structure of the pump, it can do no otherwise than downwards) with a depressing force, equivalent to the pressure upwards of the atmosphere, against the external part of the same sucker.

### C H A P. III.

BUT I shall not insist upon the foregoing objections, because the learned doctor himself tells, that their attempts may seem to be but light skirmishes in comparison of that which follows. Whereunto I shall therefore apply my attention.

THIS grand objection our learned adversary takes from the already often-mentioned ascent of the sucker clogged with a hundred pounds weight, and recommends by this  
 ge 140 introduction: *Etenim ex ipsis phenomeni visceribus robustissimum jam contra omnem mechanicam illius solutionem argumentum eruo, & quod non solum contra vim aeris elasticam supra dicto modo explicatam militat, sed etiam contra Cartesianum illum aeris conatum*  
 ge 140 *nixumque, &c.* Which premised, the argument itself is thus proposed: *Est enim* (says

\* See Continuat. of New Exper. Physico-Mechan. Exper. 48.



he) *juxta bujus experimenti phenomenon, vis illa aeris elastica nixisque expansiorius) major multo, quam quæ fieri potest a rerum natura, quàmque quotidianis illis phenomenonis congruit. Nam si nixus hic elasticus tantam vim elasticam haberet, ut plus centum pondo plumbum sursum posset propellere, omnes profectò rerum terrestrium compages tantâ violentiâ comprimerentur, ut nullæ, nisi quæ admodum firmiter compactæ sint, tantæ compressioni resistere possent, quin refrigerentur, vel partium collisione ita contererentur, ut brevi tempore perirent, &c.*

THOUGH this objection be specious enough, yet it presents me with no difficulty, that I was not well aware of; as I presume you will easily perceive by what you will meet with in the following papers, especially that which consists of experiments and considerations about the differing pressures of solids, weights, and ambient fluids. The nature of which pressure and its equality (as far as in our controversy it is needful to be supposed) will, I hope, satisfy you of the invalidity of the proposed objections; especially since the doctrine it impugns, namely the weight and pressure of the atmosphere, is not a bare hypothesis, but a truth made out by divers experiments, by which even professed opposers of it have publickly acknowledged themselves to be convinced.

#### C H A P. IV.

IN the next paragraph (which is the eleventh) the learned doctor adds a further objection, wherein he supposes, that there is laid upon a wooden scale, of the same diameter with the above-mentioned sucker, a lump of butter of the same largeness with the scale. Whence he argues, that if our hypothesis take place, the butter must be pressed against by two cylinders of air, the one pressing it upwards, the other downwards, and the pressure of them both amounting to two hundred pounds. But, says he, the butter is not pressed at all, as appears by this, that no serous humour is squeezed out of it towards the edges, not so much as in those parts that lie parallel to the horizon, whence the conclusion seems easy to be deduced.

BUT in the twelfth paragraph, the doctor himself proposes a solution, which he might easily foresee I would employ to invalidate his argument; namely, that the air pressing, as well against the sides of the butter, as against the top and bottom, hinders the mass from horizontally extending itself. And whereas, by way of reply to this subterfuge, as it is called in the margin, he subjoins, *Cui respondeo, quod tamen hoc nihil prohibet, quo minus in omnes partes horizontales exprimatur humor serosus & lacteus, si revera esset ulla bujusmodi pressura elastica, qualis fingitur:* the reply is easy, that the pressure of the ambient air, which is a fluid more subtil than butter-milk, will as well hinder the starting out of that liquor, as of the parts of the butter itself; as he will easily grant, that attentively considers the nature of the thing, and remembers how air keeps water from running out at the little holes of a gardener's watering-pot closed at the top. What the objector adds about the extrusion of what he calls a subtler element (supposed to be harboured in the butter) by the pressure of the atmosphere, in case it had any, I think it would not be difficult to answer, if we considered, that a great and undeniable pressure, applied to water, does not sensibly condense it, or deprive it of its fluidity, because of the grossness and strength of its parts. But the argument being but transiently mentioned by the author, and grounded upon a Cartesian supposition, that I never employed, I leave it to those, that may think themselves concerned (which I am not) to make a solemn answer to.

Page 142.

Page 143. AND whereas our learned examiner superadds, *Quod tamen si butyri massa in disci ligneæ speciem reducitur, cujus margo centum vicibus areâ sit minor, interque duas laminas ligneas ejusdem formæ ac latitudinis posita, filis suspenderetur in aere tanquam in lance, ita ut pressura aeris elastica, quâ ab infra, quâ desuper, ducentis fere vicibus excessura sit pressionem in marginem butyri, butyrum tamen nibilo arctius comprimitur per vim aeris elasticam, nec aliter hic afficietur quàm antea*: he seems not to have sufficiently considered the laws of the hydrostaticks, according to which, supposing the pressure of the atmosphere that he rejects, the butter ought not to be deprived of its shape. For the pressure of the ambient air, being equal on all sides, if we suppose the superficies of the butter to be distinguished into a multitude of little equal portions, each of these, whether they be situated horizontally, or on the edges, can be pressed against but by an atmospheric pillar equal to its basis; and the horizontal portions, if I may so call them, cannot be thrust out of place, without there be at the same time squeezed out some of the lateral portions, which yet cannot be so displaced, because they also are, with equal force, pressed inwards by little ærial pillars, whose bases are contiguous to them, and bear against them. Which answer, though of itself sufficient, may be much confirmed by the instance, you will hereafter meet with, of a lump of butter, that kept its irregular shape, in spite of a great and manifest pressure of the water that surrounded it.

AND this answer may suffice to disprove what the doctor annexes in the beginning of the thirteenth paragraph, about the vast excess of pressure, which the air exercises upon the flat and horizontal surfaces of the above-mentioned lump of butter, in comparison of the pressure the marginal parts of its surface can be exposed to. What he adds, and illustrates with a scheme, about the hand's being assisted with the pressure of the air, it concerns not me to answer. But whereas among the places, where the elastic power of the air is understood not to reach, he reckons a pail-full of water, with a lump of butter put in it; he supposes that, which our hydrostaticks will by no means allow, and which is disproved by several, both of our former experiments, and by those you will meet with in the following papers. By which it appears, that the pressure of the atmosphere is exercised, as indeed I do not see what should hinder it from being, even upon bodies that are quite immersed under water; and by which, added to what has been hitherto discoursed in answer to the learned doctor's objections, you will easily judge, how deservedly he shuts up the arguments, we have been examining, with this

Page 143. conclusion. *Adeo ut extra omnem controversiam positum videatur, quod nulla est ejusmodi vis elastica in aere, qualem è doctis nonnulli supponunt, multoque minus tam fortis, ut centum librarum pondus superet. Quod erat demonstrandum.*

#### C H A P. V.

BUT this is not all the doctor urges against me in this chapter; for in the fourteenth paragraph he seconds his former argument by another, drawn from this experiment of mine, that having taken two round marbles, whose surfaces, that were to be contiguous, were as well ground very flat, as carefully polished; and having placed them one directly upon the other, they did in a horizontal posture so firmly cohere, without the help of any glue, or viscous body\*, that the upper marble being pulled up, would take up the lower, though clogged with a weight of fourscore and odd pounds.

\* See the History of Fluidity and Firmness, p. 222. of the second edition.



THIS experiment, when I many years ago first published it, I referred to the action of the atmosphere, which pressing equally and strongly against the surfaces of both the marbles, except where they were contiguous, the higher could not be drawn directly upwards from the lower (and consequently must be followed by it) by a less force, than that which was equivalent to the weight of as great a cylinder of the atmosphere as leaned upon the upper marble.

THIS experiment thus explained, though it hath been judged a very favourable one to the hypothesis, on whose behalf I alledged it, does yet to the justly famous doctor seem a very considerable argument against it, though for this judgment of his he urges only this reason, that if the force, with which the air presses the lower marble against the upper, be able to sustain that marble, though clogged with the great weight above-mentioned, the same pressure of air would much more easily support a plate of wood brought to a true plain, and not loaded with any weight, if the wooden plate were substituted to the lower marble, and, instead of it, applied to the upper.

BUT since the experiment, as I proposed it, did upon trial succeed very well, it had not been amiss, if the learned examiner had considered it as it was really and successfully made, and shewed why the pressure of the ambient air was not able to hinder the separation of the marbles: and his needless substitution of a wooden plate, instead of the lower marble, easily suggests a suspicion, that there may lie some fallacy, though not intended by him, in the variation he proposes of the experiment. And he seems to have himself had thoughts of this kind, by taking notice, that it may be answered on our behalf, that a wooden plate cannot be so exactly applied to the upper marble, but that there will be a little air intercepted between it and the bottom of that stone. And though, having granted that it may be so, he employs two pages to shew, that this intermediate air could not keep the pressure of the atmosphere from supporting the unclogged plate of wood, if it had been that pressure, which, when there was no such intermediate air, had sustained the lower marble with all the appendant weight; yet I confess, his proofs seem not to me to be answerable to the assurance he uses in speaking of them. His examples taken from gunpowder and wind you will easily judge not to be very proper, where we are not considering a force, that acts by a sudden and vanishing impetus, but a constant and equal pressure. And as to his other instance, which is taken from five men, that thrust against the sixth (standing with his back to a wall) who is but as strong as any one of them; I answer, that neither is this example near enough of-kin to our case. For each of these five men is supposed to have an equal power of thrusting, proper to himself, and independent from all, or any of the other four. And the sixth man is likewise supposed to resist but by his own single force, without having his power of reacting increased by the force wherewith the others thrust against him. But in our case the thing is quite otherwise; for supposing that some aerial particles be so placed, that a solid body hinders them to recoil or expand themselves, we are to consider, that as the contiguous corpuscles of air press against them, not by their own single weight or pressure, but as they transmit the action of all the other particles of the air, which by their weight or pressure thrust them on; so the aerial particles, contiguous to the solid body, resist not barely by that force, which they would have if they were not compressed, but by virtue of the springiness they acquire upon the score of the forcible inflection they sustain from the action of the corpuscles, that either mediately or immediately thrust against them; and consequently, in proportion to that external force, the elasticity of these compressed particles will be increased, as we see, that a bow, or other springy body, the more it is bent by an external force, the greater power it has to resist further compression. Upon which grounds it need to be no wonder,

der, that a small portion of air, being almost included in a solid body, and having for some (though but very little) time been exposed to the outward air, should be capable of resisting the pressure of as much of the whole atmosphere, as can come to press against it. For, this pressure of the atmosphere being continual, if the springiness of the aerial particles were not now great enough to resist that pressure, they must necessarily have been before-hand infected or compressed by it, till the endeavours of the one and the other were reduced to an equipollency. Of which I shall give you an instance in so obvious a body as a bubble at the top of the water. For though there be but a little air included in a very thin and transparent film of water, yet this little air is so well able to resist the weight of all the atmosphere that can come to bear against it, that all the pressure of it is not able to make the film shrink, or become wrinkled; which it would do, if the corpuscles of the internal air were not reduced to a springiness, which makes its power of resisting equal to the endeavour of the external atmosphere to compress it. And to let you see, that we may well conceive such a springiness of the air included in the bubbles, I have elsewhere related, how, by barely withdrawing the pressure of the ambient air from glass-bubbles, hermetically sealed with air in them, not compressed beyond its usual state, the spring of the internal air would make the bubbles fly in pieces: and this will happen to stronger glasses than bubbles, as you will find in one of the former experiments\*. And if we would illustrate what we are debating of by an example, it should not be by considering, as the doctor does, the endeavour of five men against the sixth, that hath his back to the wall; but that of five bladders full of air, piled up, and resting upon a sixth. For in this case, whatever force or power of pressing we suppose in the incumbent bladders, they all bear jointly upon the lower, which continuing at a stand, must thereby be so compressed, as to be able to resist their joint endeavours; as it is manifest, because otherwise it would not continue in that state, but be farther compressed; which is against the supposition.

THIS notion about pressure and resistance I have the more particularly deduced, because I found many modern naturalists, and even hydrostaticians themselves, to be great strangers to it. For which reason I shall add, that I have evinced it by purposely devised experiments in the continuation of the physico-mechanical experiments† about the air. Were it not for this, I should perhaps have spared myself the labour of setting down these thoughts, as not necessary to the solution of the doctor's objections. For he admits a layer, or (as he aptly speaks) an area of aerial particles to be interposed between the upper marble and the wooden plate; and therefore the flatness and stiffness of those two bodies must keep them from an immediate contact, as well at the edges, as by the help of the same area they do elsewhere; and consequently, that interposed air may communicate with the ambient air. From whence the laws of the hydrostatics (which I have elsewhere shewn) will allow me to conclude, that the weight of the atmosphere endeavours to depress the upper surface of the wooden plate; and so what the examiner urges of the inconsiderable resistance, that the few aerial particles, interposed between the flat bodies, can make to the great pressure of the column of air, that thrusts the wooden against the marble plate, would not conclude, though our former answer could not have been made; since the resistance, made by the interposed aerial particles to the pressure upwards of the atmosphere, is not, in our present supposition, made by those particles alone, but by the weight of the lateral and superior part of the atmosphere, exercised by the intervention of these particles. Which being so, what the learned doctor

\* See the Tract about the Pressure of the Air's Spring on Bodies under Water.

† Exper. xxv, and elsewhere.



adds, that the weight of the wooden plate itself is here of no consideration, must needs be a mistake. For the two equal atmospherical pressures, the one against the upper surface of the wooden plate, and the other against the lower, countervailing, and consequently frustrating the endeavour of each other, the gravity of the wood itself will suffice to make it fall, as well as if it were pressed against by neither of them. And from this discourse you will easily judge, whether the doctor had reason to say as he does, *Quam ab omni ratione igitur absorbum est, ut superficies illa sive area aerearum particularum, quæ insinuant se laminam ligneam inter & marmor, solidam columnam hujusmodi particularum, vi elastica sursum enitentium, contra laminam ligneam obnitendo vincat, ipsamque laminam in terram deturbet.* Page 146.

C H A P. VI.

WHAT he adds in the sixteenth number against those, that fancy the aerial particles to be endowed with perception, and to act with design *pro re nata*, does not all concern me; and what he adds in the next paragraph, wherewith he concludes his twelfth chapter, I shall altogether pass by, as far as it concerns the extravagant conceit he opposes. But because at the close of the paragraph he makes an inference, which comprizes our opinion also; since he concludes, that the experiment by him alledged, *Certissimum est indicium, particulas aerias nec cum consilio nec sine consilio inferius marmor sustinere nec sufficere*: it will not be amiss to shew, that our opinion is undeservedly included in the inference; which I shall do by briefly solving the phenomenon the doctor lays so much weight on. For if we conceive with him, that the two flat marbles formerly mentioned be suspended, and that to the lower of them, a flat wooden plate of the same shape and extent be applied; I see no cause to wonder, why the two marbles should stick together, and not the lower of them to the wooden plate. For, as I lately noted, there being an area, or bed of aerial particles interposed betwixt the marble and the wood, the weight of the atmosphere, exercised by the intervention of those aerial corpuscles, ought to be æquipollent to the pressure of the atmospherical cylinder that bears against the lower surface of the plate; which consequently by its own weight must drop down: whereas there being no such layer of aerial particles interposed betwixt the two marbles, the pressure of the ambient atmosphere, which touches them every where, save where their polished surfaces are contiguous, must keep them strongly coherent. Page 150.

I PRESUME I need not mind you, that hitherto I have discoursed upon supposition, that the doctor experimentally knows, what he delivers concerning the non-adhesion of an exactly smooth wooden plate to a marble one; and upon his concession, that because of the want of sufficient congruity between the surfaces of two bodies, there is a bed of aerial corpuscles interposed between them. But now, I think, it will not be unfit to take notice to you, that though to illustrate, on this occasion, a subject that is generally so little understood, as the exercise of pression among fluid bodies, I have answered my learned adversary's objections, as I had nothing more to say for my explication of the suspension of coherent marbles, than what I many years since delivered in the little tract by him cited; yet I have since abundantly confirmed that explication by the 50th of the experiments published in my continuation; which if the doctor had been pleased to read, perhaps he would have received the same satisfaction, that other learned men have done; since there I experimentally shew, that the undermost marble, with.

without the accustomed clog, would, upon the bare withdrawing of the sustaining air, drop off from the upper. And whereas the two marbles in our vacuum would not cohere, as soon as the formerly excluded air was let in upon them, it did by its super-vening pressure make them stick together very strongly.

## THE SECOND SECTION.

### C H A P. I.

**I** PROCEED now to the second of those two chapters, that I am interested to consider, in which the learned examiner is pleased to attack three or four of my hydrostatical opinions and explications; in the defence whereof, I hope, I shall be the less put to exercise your patience, because the learned doctor himself is pleased to grant me almost as much as I need desire concerning the truth of the hypothesis, whereon my paradoxes and explications are founded. For whereas the main thing I supposed in my hydrostatical papers is, that in water, though stagnant, the superior parts do actually, though not always prevalently, gravitate upon the inferior, or (if you will) press upon them, even when they do not sensibly depress them; the doctor in divers places allows this hypothesis to be consonant to the principles of the mechanical philosophy; and accordingly having shewed that in a suspended tub of water the whole liquor gravitates upon the bottom of the tub; he subjoins, *Jam verò cum tota hæc aqua constet ex particulis aqueis non compactis vel concretis, sed solutis à se invicem, impossibile est, ut omnes fundum situle premant, nisi infima quæque ab omnibus superioribus prematur, quemadmodum clarè demonstravimus in secunda sectione hujus capituli; nempe, si nullæ causæ nisi purè mechanicæ (quales sunt motus localis, magnitudo, figura, &c.) in edendo hoc phænomeno se intermiscant.*

Page 161. **AND** elsewhere in the same chapter he speaks thus of the gravitation of liquors (towards the close of the second paragraph.) *Necesse utique est, ut partes singule gravitent, cum totius sit gravitatio, si non sit aliquid immateriale principium in rerum natura, &c.* And adds, at the beginning of the next number; *sanè atque huic externi motus hypothesi, & gravitationis elementorum in propriis locis inde necessariò emergentis, apprime consonum est primum illud experimentum, quod scriptor profert in paradoxis suis hydrostaticis.*

**AND** now, Sir, I presume you do not much wonder, if I think these concessions reach the main thing I pretend to. For though I do as freely and heartily, as the doctor himself, who, I dare say, does it very sincerely, admit, or rather assert an incorporeal being, that made and governs the world; yet all that I have endeavoured to do in the explication of what happens among inanimate bodies, is to shew, that supposing the world to have been at first made, and to be continually preserved by God's divine power and wisdom; and supposing his general concurrence to the maintenance of the laws he has established in it, the phænomena, I strive to explicate, may be solved mechanically, that is, by the mechanical affections of matter, without recourse to nature's abhorrence of a vacuum, to substantial forms, or to other incorporeal creatures. And therefore, if I have shewn, that the phænomena, I have endeavoured to account for, are explicable by the motion, bigness, gravity, shape, and other mechanical affections of the small parts of liquors, I have done what I pretended; which was not to prove,

that



that no angel or other immaterial creature could interpose in these cases; for concerning such agents, all that I need say, is, that in the cases proposed we have no need to recur to them. And this being agreeable to the generally owned rule about hypotheses, that *entia non sunt multiplicanda abque necessitate*, has been by almost all the modern philosophers of different sects thought a sufficient reason to reject the agency of intelligences, after *Aristotle*, and so many learned men, both mathematicians and others, had for many ages believed them the movers of the celestial orbs.

C H A P. II.

BUT you will tell me, that the doctor's concessions will not avail me, since he urges against the gravitation of the elements in their proper places, which gravitation he would have to be suspended by his incorporeal principle, an experiment which, he says, is most manifestly repugnant to our hypothesis. He conceives then, that in a tub or pail-full of water, with a perfectly cylindrical cavity, whose diameter is of sixty-two parts, there is violently kept at the bottom, by the help of a stick, a round plate of wood, whose diameter amounts but to sixty-one of those parts; and that as soon as ever the stick is removed, the wooden plate will emerge to the top and float. *Quod*, says he, *prorsus impossibile esset, si omnes partes aque ab (FG) ad (HF) non solum junctim fundum vasis, sed singule singulas in eadem serie subjeetas actu premerent*. To which assertion he immediately subjoins this argument to prove it by; *Cum diameter laminæ lignæ (HM) partes* Page 155. *61, habeat æquales, diameter vasis (HI) habeat 62, manifestum est, quod superficies fundi vasis ad superficiem laminæ se habet ut 3844, ad 3721; quorum differentia est 123. Itaque rotundum intervallum inter latera vasis & marginem laminæ lignæ habet se ad aream laminæ, ut 123 ad 3721, hoc est, area laminæ lignæ excedit aream dicti intervalli plusquam triginta vicibus. Ac proinde aqua incumbens lignæ laminæ excedit magnitudine aquam incumbentem dicto intervallo inter marginem laminæ & latera vasis plus quam triginta vicibus. pondusque sive pressio hujus alterius pondus pressionemque vincit plusquam aquam ipsi subjectam, ut hujus vi sublevetur lamina, quam vis tricies major deprimat. Quod* (says he, by way of inference) *æque absonum atque absurdum phenomenon esset, &c.*

How little this ratiocination agrees with the experiments I have formerly told you of, about the cases wherein light bodies will be detained under water, or emerge to the top of it, you will easily perceive, if you compare the one with the other, which you may quickly do, if you please to compare the doctor's discourse with the following narratives of those trials \*, to which alone I might therefore refer you. But yet in the mean time, you may, if you think fit, consider a little, whether the argument, whereon the doctor lays so much stress, be any more than a paralogism.

FIRST then, since according to his computation the area of the interval, between the sides of the vessel and the edges of the round board, is 123 of such parts, whereof the area of the board amounts to 3721; it is evident that there must be room enough for the water to pass between the sides of the vessel and the edges of the board, which is supposed on all hands to be of some wood lighter in specie than water, since else it would not emerge upon the withdrawing of the stick.

NEXT, this board or wooden plate is not here intimated, or supposed to be (and indeed in practice can scarce be) made exactly congruous to the bottom of the vessel, and conse-

\* See the tract of the positive or relative levity of bodies under water. Exp. I. &c.

quently the water may get in between them; for which cause it is necessary to keep the wooden plate forcibly down with a stick, which else were needless. And consequently this interposed water will communicate with the laterally superior water in the vessel, which superior water may, according to the laws hydrostatical, by the intervention of the interposed, exercise its pressure upwards against the lower surface of the wooden plate.

THIRDLY, the doctor's scheme allows and assists us to conceive (which we may do however) an imaginary plane of water to be parallel to the bottom of the vessel, and to pass along the bottom of the board; so that of the water that lies between this plane and the bottom of the vessel, one part is covered by the wooden plate; and the other between the edges of that and the sides of the tub, is covered with the incumbent water only.

### C H A P. III.

THESE things being premised, I thus argue; it is manifested by hydrostaticians after *Archimedes*, that in water, those parts that are most pressed, will thrust out of place those that are less pressed; which both agrees with the common apprehensions of men, and might, if it were needful, be confirmed by experiments. It is also evident that that part of the above-mentioned imaginary plane, that is covered by the wooden plate, must be pressed by a less weight than the other part of the same plane; because the wood being bulk for bulk lighter than water, the aggregate of the wood and water incumbent on the covered part of the same plane must be lighter in specie than the water alone, that is incumbent on the uncovered part of the same plane; and consequently this uncovered part being more pressed than the other part of the plane, the heavier must displace the lighter, which it cannot do but by thrusting up the board, as it does, when the external force that kept it down is removed. And, to add this upon the by, this greater pressure against the bottom than against the top of bodies immersed in water specifically heavier than they, is a true reason of their emersion, as I have elsewhere shewn. So that there happens no more in this case than what usually happens in the ascension of bodies in liquors specifically heavier than themselves, on the account of the newly mentioned difference of pressure. And it is with an express, or supposed exception of such a difference, which in many other cases may be safely neglected, that I desire you to take notice of in most places of this discourse I speak of the pressure of ambient fluids on immersed solids, as uniform or every way equal.

IT is true, that, according to the doctor's supputation, if the solid cylinder, consisting of the wooden plate, and all the water directly incumbent on it, were put into an ordinary balance, it would there many times outweigh the hollow cylinder of water alone, that leans upon the uncovered part of the imaginary plane. And that is it that seems to have deceived the learned doctor. But there are divers hydrostatical cases, wherein the phenomenon depends not so much upon the absolute weight of the compared bodies, as upon their respective and their specific gravity; on whose account it is, that a small pebble, for instance, that weighs not a quarter of an ounce, will readily sink to the bottom of the river, on whose surface a log of wood of a hundred pounds in weight will float. It is a rule in hydrostatics, that when two portions of water, or any other homogeneous liquor press against each other, the prevalency will go, not according to the absolute weight, but the perpendicular height of those portions. And accordingly we find, that if a slender pipe of glass, being filled with water, have its lower orifice unstopped at the bottom



of a vessel of water, which contains much more of that liquor than the pipe; yet if this last named water were, for instance, two feet high, and that in the vessel but one, the water in the pipe will readily subside, till it come almost to a level with the external water, though it cannot do so without raising the whole mass of water that stagnated in the vessel.

AND now I shall subjoin an experiment, which, though at first it may seem slight, and was made in lesser glasses and quantities than I would have employed, if I could have procured better accommodations, has the advantage of requiring no curious instruments, and yet I hope will serve for an ocular proof of the fallaciousness of that reasoning the doctor is so strangely confident of.

WE took an open-mouthed glass, such as some call jars, and ladies often use to keep sweetmeats in, which was three inches and a half, or better in diameter, and somewhat less in depth, and had the figure of its cavity cylindrical enough. Into this having put some water to cover the protuberance wont to be at the bottom of such glasses, we took a convenient quantity of bees-wax, and having just melted it, we poured it cautiously into the glass, warmed before-hand to prevent its cracking, till it reached to a convenient height. This vessel, and the contained liquors we set aside to cool, in expectation, that when the heat that had dilated the wax, was gone, it would shrink from the glass, and consequently leave a little interval every where between the concave superficies of the vessel, and convex of the hardened wax; which accordingly came to pass, and saved me the labour of getting the wax shaped for my purpose with tools; which might have been done, but not without trouble and less exactness. And now it was easy for me to try the experiment I designed; for pouring in warily some water between the glass and the wax, so that it filled all the interval left between those two bodies, both at the bottom and the sides, the wax was made presently to float, being visibly lifted up from the bottom, and its upper part appearing a little above the level of the water, which was no more than I did, and had reason to expect, according to the true principles of hydrostaticks. For water being somewhat, though but little heavier in specie than wax, and that which was poured into the bottom and stagnated there, being pressed by the collateral water, every way interposed between the concave part of the glass, and the convex of the wax (so that this collateral liquor answered what I lately called a hollow cylinder of water in the doctor's experiment) that part of the stagnant-water that was leaned upon by the wax, being less pressed than the other part of the same stagnant water was by the water incumbent on it; this latter must displace the former, which it could not do, but by raising up the wax that leaned upon it. And yet this collateral water was so far from being heavier than the wax its pressure impelled up, that both the collateral and the stagnant water all together, being weighed in good scales, amounted to little above a quarter of the weight of the wax, which happened by reason of the narrowness of the vessel, which, if it had been wide enough, I doubt not but the experiment would have succeeded, though the wax had outweighed the collateral water ten times more than in our experiment it did. But that the solid body exceeded almost four times the weight, not only of the collateral, but the stagnant liquor too, does sufficiently overthrow the doctor's ratiocination. Whose fallaciousness will yet further appear by two other improvements, among others, which I made of one experiment.

FOR, I. though we poured in more and more water, as long as the vessel would contain any, the cylinder of wax was but lifted higher and higher from the bottom of the glass, but did not appear raised, more than at the first, above the upper surface of the water; which argues, that it was not at all the quantity of the inferior water, which was

continually increased, but the pressure of the collateral water, which continued still at the same height in reference to that wax, that caused the elevation of the body.

AND, II. to manifest yet more clearly the doctor's mistake, I devised the following trial. We took a round plate of lead about the thickness of a shilling, and having made it stick fast to the bottom of the cylinder of wax, to make this body sink the more directly, we placed one after another, upon the upper part of the wax, divers grain weights (first wetted to keep them from floating) till we had put on enough to make the wax subside to the bottom; for the facilitating whereof we had pared off its edges; by this means, the glass having been at first almost filled with water, there swam about an inch or better of that liquor above the upper surface of the wax. And lastly, we took off by degrees the grain weights that we had put on, till we saw the wax, notwithstanding the adhering lead, rise, by degrees, to the top of the water, above which some part of it was visibly extant.

FROM this experiment I thus argue: it is manifest, that, according to the doctor's supposition, here was incumbent upon the wax a cylinder of an inch in height, and of the same diameter or breadth with the round surface of the wax; whereas upon the removing part of the water, that lay at the bottom when the wax began to rise, there was incumbent no greater weight than that of the collateral water, and as much of the superior and stagnant, as was directly incumbent upon that collateral water (and would have deserved the same name, if we had supposed the convex surface of the wax to have been continued upwards as high as the glass reached). But now, whereas, according to the doctor's ratiocination, this cylinder of water incumbent on the wax, being an inch deep, and a good deal above three inches broad, must press the wax with a greater weight by several times, than that which the lateral and hollow cylinder of this stagnant water could have upon the rest of the collateral water; yet the height of this aggregate of collateral waters being the same with that of the wax and the water swimming upon it, the difference of the pressure was so small, that barely taking off a weight of four or five grains, the wax would, notwithstanding the pressure of the water incumbent on it, be impelled up and made to float; and by the like weight, put again upon it, it would be made to sink, and by another removal of such a weight (for I purposely reiterated the trial more than once) it would, though slowly, re-ascend. And these phenomena do so much depend upon a mechanical æquipollence of pressure, that even four grains would not have been necessary to make the wax rise or sink, if it had not been for some little accidental impediments that are easily met with in such narrow glasses; for otherwise in a larger vessel we have made the same lump of wax readily enough sink or float, by the putting in or taking off a single grain or perhaps less.

By this you may see, that for the regulation of hydrostatical things, nature has her balance too, as well as art; and that in the balance of nature the statical laws are nicely enough observed.

You may also take notice, upon the by, how little the weight of the cylinder of water upon a body immersed in stagnant water is considerable, whilst there is a pressure of collateral water to counterbalance it; since in this last trial, though the cylinder of incumbent water did continually increase or decrease in length, whilst the lump of wax was sinking or emerging; yet the same despicable weight of a grain or less, that was just able to depress it beneath the upper surface of the water, did by its pressure or removal procure its sinking to the very bottom, or rising again to the top, and on both occasions with an equal slowness, bating that little acceleration of motion that ought to happen upon another account, and which therefore is to be observed in the wax, during its rising as well as during its sinking.



C H A P. IV.

SOME other phenomena I produced, by varying the hitherto mentioned experiment, which are very favourable to our notions about hydrostaticks. But, since they do not directly concern the present controversy, I shall in this place only annex a couple, the former whereof affords an easy confirmation of that paradox, which we lay as the ground of divers others, and the contrary whereof is maintained not only by doctor *More*, but by many other famous and learned men; namely, that in stagnant water the upper parts do actually prefs the lower.

We took then a very slender pipe of glass, whose cavity was narrower than that of an ordinary goose-quill, that heterogeneous liquors may not be able to get by one another in it. This pipe, near one end, was bent upwards like a siphon, that it might have a short leg, as parallel as the artificer could make it to the longer. Into this crooked pipe we put a little oil, and then held it perpendicularly in a somewhat deep and wide-mouthed glass, filled partly with water and partly with a lump of wax, of the bigness and shape of that already mentioned; that so the pressure of the incumbent water upon the open orifice of the shorter leg, might impel the oil into the longer leg, somewhat above the surface of the water in the vessel; which it was convenient should be done, that we might the better see the motions of the oil, and which we knew must be done by the course we took; both because oil is lighter in specie than water, and consequently required not an equal height of water to counterbalance it; and because, in very slender pipes, water is wont to ascend a little above the level of the external water, whereinto they are immerfed. The pipe being, as was said, held upright, it was easy to take notice by a mark fixed on the outside, to what height the oil reached in it.

Now if we conceive a horizontal plane, parallel to the bottom of the vessel, to pass by the basis of the floating wax, it is evident by what has been formerly shewn, that, of this imaginary plane, that part, on which the wax is incumbent, is as strongly pressed by the weight of the wax, as the lateral part of the same plane is by the weight of the water incumbent on it (otherwise these pressures would not be æquipollent, but the wax would be raised); and consequently that part of this plane that is placed directly over the orifice of the shorter leg of the pipe, is no more pressed than any equal portion of that part of the same plane that is covered by the wax. This body being taken out of the water, the liquor subsided a great way in the vessel, and so did proportionably the oil in the longer leg of the pipe. And lastly, having weighed out in a good pair of scales as much water as we found the wax to amount to, this liquor was, instead of the wax, poured into that which remained in the glass; whereupon the oil in the longer leg of the pipe was again impelled up very near to the former mark to which it had been raised by the wax. Whence we may gather, that the water newly put in, though in the air it weighed no more than the wax, yet it did as much press the water that lay beneath the forementioned imaginary plane, and consequently that which was directly over the shorter leg of the pipe, as the wax that had been taken out, had done. And since we have already proved that the wax did considerably press that plane, it ought not to be denied, that the water also (which instead of it was able to impel upon the oil in the pipe) did in like manner press that plane; and consequently, that water may be gravitated in water as well as a solid body, such as wax is, can. And this is the first additional use, I told you, I would make of our experiment.

BUT, to come now to the second, there is another phenomenon of it, viz. the above-mentioned tenderness of nature's balance, whose use seems to be of no less general concernment

cernment to the true doctrine of the hydrostatics. For, by duly considering that phenomenon, and reasoning a while upon it, we may be helped to rectify that plausible mistake, which has long deluded both philosophers and mathematicians, and does yet impose on most of them; namely, that a body does not actually gravitate when it does not descend. For we have seen already, and shall further shew by and by, that the sunken wax, and the brass grains that lie on it, do actually press or gravitate upon the subjacent water and bottom of the vessel on which it is incumbent; and consequently its pressure being not surmounted by that of the collateral water, which is unable to raise it, must be as great as that of this collateral water. Therefore, when upon the removal of a single grain, the wax, with its incumbent weight, is made to ascend, and that but very slowly, it is evident, that it was so far from not gravitating before, because it did not actually descend, that it retained its gravity even whilst it ascends; as may appear not only by the slowness of its motion upwards, proceeding from its being in nature's balance very little less heavy than it need be, to countervail the pressure of the collateral water; but by this also, that if but a single grain be laid on it, when it begins to rise, its ascension will be checked and hindered, which could not be done by the addition of so inconsiderable a weight, if the wax and the adhering metal did not, even during their ascent, retain their former gravity, though that were frustrated as to the act of descending, or so much as keeping their station by the prevailing pressure of the collateral water; so that, since, as we found, the wax and adhering metal amounted to a good deal above four thousand grains, it did in the balance of nature weigh, whilst it was ascending, not so much as a four-thousandth part less than it did, whilst it was actually descending.

## C H A P. V.

I SHOULD beg your pardon, Sir, for having detained you so long with my reply to a single objection of the doctor's, how pompously soever proposed; but that I thought it not amiss to do some service to the true theory of hydrostatics, by taking this occasion to present you some things that I thought not unlikely to illustrate some parts of that theory, though above what was necessary to answer the doctor's argument; to which, I confess, I was troubled to see so learned a man subjoin the following conclusion: *Hæc tam luculenta demonstratio contra gravitationem particularum aque inter se quamvis junctæ fistulæ fundum urgeant, si non sit vera atque solida, equidem nec mei ipsius nec ullius unquam mortalis in posterum ratiociniis credam.* But I hope he will not be as bad as his word, but will be pleased to consider, as well as I do for him, that a man may be very happy in other parts of learning, and of greater moment, that has had the misfortune to mistake in hydrostatics, a discipline, which very few scholars have been at all versed in, and about which divers of those few have had the misfortune to err, not only in the conclusions they have drawn, but in the very principles they have embraced.

To the foregoing argument, the doctor, though he declares he thinks it needless, adds in the fifth paragraph another, taken from the last experiment of my hydrostatical paradoxes, by which he ingenuously acknowledges, that I seem at first sight to have demonstrated what I pretend to, about the gravitation of the upper parts of stagnant water upon the lower. And I am sorry that I cannot, in return, acknowledge that his objection, at first sight, seemed to me a cogent one, for neither at the second nor third perusal can I clearly discern where his ratiocination lies, supposing it to be meant for an answer to my experiment. And though I consulted with some learned members of the Royal Society, whereof two are mathematicians, and one his particular friend; yet they all confessed he



had not sufficiently explained himself on this occasion, nor could they shew me to what argumentation I might properly direct my reply. Only one of the doctor's correspondents, having seriously perused his discourse, and the annexed scheme, told me, that what seemed the most probable to him, was, that though the doctor was too civil to give me, *in terminis*, the lye; yet he did indeed deny the matter of fact to be true. Which I cannot easily think, the experiment having been tried both before our whole society, and very critically, by its royal founder, his majesty himself. But since you have your self seen, and made it more than once, I need not spend words to convince you, that the matter of fact is true.

BUT after I had in vain sought the doctor's meaning where I expected it, chancing lately to cast my eyes on another place, where I saw my scheme repeated, I find this passage in the explication he endeavours to give of the phenomenon by his hylarchical principle: *Cum verò tam profunde immergitur tubus, ut obturaculum tangat superficiem VW, vis retractionis aeris ita augetur, ut etiam ponderis appensi superadditam depressionem superet. Videtur igitur quasi quædam sursum-susilio aeris in tubo contenti, & conformis ac contemporanea aquæ compulsio in obturaculum, quo tam firmiter in os valvulæ comprimitur, ibique cum appenso pondere sustentatur.* What considerable interest the supposed, but unproved retraction of the valve, or the air itself, can have in this phenomenon, I confess I do not discern; not being able to see, but that the experiment would succeed, when tried *in vacuo*, although all the atmospherical air were annihilated. But if I mistake the doctor's meaning, I am to be excused, since I do it not willingly, and his own obscurity has been accessary to it. Nor am I very apprehensive of being unable to defend my account of an experiment, which (as you know) has had the good fortune to recommend the doctrine, for the proof whereof I devised it to many learned and curious persons, several of which were sufficiently indisposed to admit it.

AND to avoid all mistakes and disputes that may arise (which I think they must do needlessly) upon the score of the valve employed in our experiment, I shall remind you of another, that I remember I have sometimes shewn you, and divers other virtuosi, though I remember not whether I have mentioned it in any of my published writings. The sum of this trial is, that an arbitrary quantity of quicksilver being, by suction, raised into a very slender glass pipe, whose upper orifice is stopped with the experimenter's finger, to keep the mercury from falling before its time, the open end of the pipe with the mercury in it is thrust into a competently deep glass of water till the little cylinder of mercury have, beneath the surface of the water, attained to a depth that is at least fourteen times as great as the mercurial cylinder has of height. For then, the finger being removed from the upper orifice, the glass-pipe will be open at both ends, and there will be nothing to hinder the quicksilver's falling down to the bottom, but the resistance of the cylinder of water that is under it, which cylinder can resist but by virtue of the weight or pressure of the stagnant water that is superior to it, though but collaterally placed above it; and yet this water being by the pipe, whose upper part is higher than its surface, and accessible only to the air, kept from pressing against the mercury any where but at the bottom of the pipe, and being about a fourteenth part of the weight of an equal bulk of mercury, it is able at that depth to make the subjacent water press upwards against the mercury, which is but a fourteenth part as high as the water is deep, with a force equivalent to that of the gravity wherewith the mercury tends downwards. And to manifest, that this phenomenon depends merely upon the æquilibrium of the two liquors; if you gently raise the lower end of the pipe towards the surface of the water, this liquor, being not then able to exercise such a pressure as it could at a further and greater depth, the mercury preponderating, will, in part, more or less, as the pipe is  
more

more or less raised, fall out to the bottom of the glafs. But if, when the quicksilver is at the first depth, instead of raising the pipe, you thrust it down farther under the water, the pressure of that liquor against the mercury increasing with its depth will not only sustain the mercury, but impel it up in the pipe to a considerable distance from the lower orifice of it, and keep it near about the same distance from the surface of the laterally superior water. And this experiment may not only serve for the purpose, for which I here alledge it, but also, if duly considered and applied, may very much both illustrate and confirm the explication formerly given of the seemingly spontaneous ascent of the clogged sucker in our exhausted air-pump.

THE last argument the doctor urges against the gravitation of water in what they call its proper place, is deduced from what happens to the divers, who, in the middle of the sea, though the salt-water of that be much heavier, than that of fresh water rivers, do not find themselves oppressed, or so much as feel themselves harmed or compressed by the vast load of the incumbent water.

BUT that the equality of the pressures of an ambient fluid will go a great way towards the solving of this difficulty, you will find by the experiments and considerations you will meet with in the following \* papers, to which, for that reason, I refer you. And though the doctor in this same paragraph objects, *tamen hæc pressio æqualis sit, nihil tamen impedit, quò minus subtiliores partes corporis magisque fluidas exprimat & elidat*: I remember I answered that exception before, by saying, that those liquors that he supposes should be squeezed out, cannot be so, because there is as great a pressure against those parts at which they should issue, as against any of the rest, if the parts that should be squeezed out, be not too spirituous and subtle, which if they be, I should gladly learn how the doctor knows, that no such minute and spirituous particles are really expelled; especially if that be observed, which we shall soon have occasion to relate, that a small animal, being vehemently compressed in water, seemed a little, though but a little, to shrink.

BUT that we may the more distinctly consider this grand argument, taken from the experience of the divers, that is wont to be employed by the schools, and others, for the vulgar opinion, and is now urged by the learned doctor to prove his; it will be convenient to observe, that it does, at once, both propose a question, and contain an objection, grounded upon the surmised insolubleness of that question.

AND to begin with the problem, "whence it is, that divers are so far from being killed "or oppressed by the weight of the incumbent water, that they are not so much as hurt "by it, nay, that they scarce feel it at all?" we may take notice, that there is in it somewhat supposed, as well as somewhat demanded. For, in the question, it is taken for granted, that divers, though at never so great a depth, feel no pressure exercised against them by the water; which is an affirmation in point of fact, of whose truth I make some question, for the reasons I shall ere long have occasion to mention.

BUT it will clear the way for what is to follow, if I here divide the noble and difficult problem we are to consider, into two questions; the first, why a diver should not be oppressed and crushed to death by the pressure of the incumbent and ambient water? And the second, why at least he should not be made sensibly to feel it, by suffering some considerable inconvenience from it?

IN answer to the first of these questions, you will easily perceive that divers things may be pertinently applied, that you will meet with in the following paper, to shew the difference betwixt the pressure of fluid and that of solid bodies. And that *de facto* the

\* The Author means the new experiments of the differing pressure of heavy solids and fluids.



pressure of water may be exceeding great, without destroying an animal quite surrounded with that liquor, I have long since shewn in another \* treatise, by the experiment of a little tadpole, which being, together with the water it swam in, included in a bent glass sealed at one end, the animal was not killed or sensibly hurt, but only (according to what was lately noted by anticipation) seemed to shrink into somewhat, and but little lesser dimensions.

If it be here alledged, that this experiment makes rather against me than for me, the learned doctor having made use of it with a scheme to explain it in his sixteenth paragraph; it will be fit for me to consider his objection. Having then recited the matter of fact newly delivered, he adds, *Quod certe fieri non posset nisi juxta legem quartam contrusio particularum aquæ contra se invicem principio bylarcbico inbiberetur & eluderetur. Atque hinc fit, ut quomvis aqua in tubo (ABC) vi trudit (GF) aliquandò facta sit condensatio, partes tamen sic compressæ, ut proprius ad se invicem accedant, nibilo inter se sunt comprimentiores.* And then subjoining the following passage; *Neque enim sequitur ex earum contactu, quod premant se invicem, quandoquidem particule, uti fit in auris corporibus, in unum coalescere possunt, & tamen non mutuò se premere* (wherein are some things that might be questioned, if it were necessary) he thus pursues his discourse: *Cum verò hic particule aquæ, se omnino premerent se invicem, pressura in gyrium, columnæ aqueæ, ducentos vel trecentos pedes, æneæ verò, plus viginti vel triginta pedes altæ, pressionem adæquaret, luculentum est indicium, quod revera particule se invicem non premant. Nam planè est incredibile, columnam æneam pro corpore quidem gyrini latam, sed altam viginti vel triginta pedes & amplius, gyriquoque ad perpendicularum incumbentem, omnia viscera tam tenelle gelatinæ non esse elisuram.* Notwithstanding which allegation I am apt to think you will judge the argument, from this experiment, to be more probable on my side than on the doctor's. For there being in our case an animal, exceedingly much more tender than a man, exposed to a pressure, which he affirms is so great, that if it were exercised on the tadpole, it ought to squeeze out all his guts, I think, I may pretend to have given a pertinent instance, that a diver may be at a considerable depth under water preserved from being crushed to death by the weight of it. And whereas the doctor tells us, that the cause of the incolumity of the tadpole is, that the pressure or contrusion of the particles of the water against one another is hindered or frustrated by the *principium bylarcbicum*, I reply, that what I affirm is matter of fact, and evident (namely, that there was a great external force duly, and yet ineffectually applied to press to death, by means of the water, the animal swimming in it); but that this mechanical force was suspended, or made ineffectual, by some invisible and immaterial agent, is but the doctor's hypothesis, and a thing, which, whether it be true or no, is at least not manifest.

HAVING said thus much about the first question, I now proceed to the second, "Why divers, though at never so great a depth, complain not of the pressure of the water, nor suffer any harm nor inconvenience by it?"

AND here, Sir, the question highly meriting a particular curiosity, I shall not scruple in the more full enquiry I am now entering upon, as well sometimes to employ and enlarge particulars already mentioned in the last of the following papers, as oftentimes to strengthen them with new ones. And I shall also, for a while, suspend my difference with the doctor, and addressing myself to you, who, I am sure, will allow me, that water weighs in water, propose, according to my custom, not as a dogmatist, but as an inquirer, some particulars that may tend to the solution of a problem, which I take to be as difficult as noble. Not that I doubt, but it must and will be explicated upon the

\* The Author points at the Appendix to the Hydrostatical Paradoxes.

mechanical principles; but partly, because the application of them to the solution will not offer itself to every seeker; and partly, because we are not yet well furnished, either with experiments made on bodies under water, or so much as with so competent an account of the matter of fact, as I think may keep wary men from hesitations about it. For what is commonly reported concerning the divers, is (as has above been intimated) grounded but upon their own relations and answers, perhaps amplified or procured by leading questions from persons, who are generally either slaves or ignorant men, taken from the less sober part of the illiterate vulgar, and prepossessed with the common opinion of the non-gravitation of water in its own place; and consequently are not like to make over-accurate observations, but prone to refer the inconvenient alterations they feel, to any other cause than the pressure of the water, which they are taught to be none at all. If observations about diving were made by philosophers and mathematicians, or, at least, intelligent men, who would mind more the bringing up out of the sea instructive observations, than shipwrecked goods, we should perhaps have an account of what happens to men under water, differing enough from the common reports.

You will in one of the following papers find mention of a learned physician of my acquaintance, that, upon his diving leisurely, perceived a constriction to be made of his thorax by the action of the surrounding sea-water.

Purch.  
Tom. IV  
Lib. 8. P.  
1587.

A SPANISH prelate that lived long in *America*, speaking of the deplorable condition of those wretched Indians that were employed by their inhuman masters about the fishing for pearls, gives us this account of them: "It is impossible that men should be able to live any long season under the water, without taking breath, the continual cold piercing them; and so they die commonly parbreaking of blood at the mouth, and of the bloody-flux caused by the stomach. Their hair, which are by nature coal-black, alter and become afterwards a branded russet, like to the hairs of sea-wolves, &c."

Purch.  
Tom. I.  
Lib. 4. C.  
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AND a general of the English in the *East-Indies*, being by them employed on an embassy to the emperor of *Japan*, has this passage concerning some female divers, that he met with in his voyage: "All along this coast, and so up to *Ozaca*, we found women divers, that lived with their household and family in boats upon the water, as in *Holland* they do the like. These women would catch fish by diving, which by net and line they missed, and that in eight fathom depth. Their eyes, by continually diving, grew as red as blood, whereby you may know a diving woman from all other women." I know it may be said, that these diseases may proceed from the coldness and moisture, or other qualities of the sea; nor would I confidently reject such a surmise; but it may also be possible, that the compression they suffered under water, might have, at least, a share in the production of these ill effects. For how are we yet certain, that the pressure of the water against their bodies, though it does not manifestly dislocate any solid or firm part, but only somewhat press inwards, as in the above-mentioned tadpole the outward skin and the fibres (both which will easily yield a little way, without being painfully stretched) may not, by straitening the vessels, and otherwise inconveniently alter the circulation of the blood and the motion of the humours, spirits, and other fluid parts of the body? And I am not sure that much of the cold that divers are wont to complain of, when under water, may not be a disaffection produced in the nervous and membranous parts, occasioned by the compression of the ambient water, there being divers things, and pressure among others, besides actual cold, that will make men complain of being cold; and in our case, this sensation may be excited, or assisted, by the hindering of the usual perspiration at the constricted pores of the skin. And it seems not impossible, that one, not so ignorant and heedless as divers are wont to be, may refer a new sensation,



fenſation, that really proceeds from preſſure, to other cauſes; ſince learned and intelligent men, when prepoſſeſſed (as theſe common divers uſually are) with the vulgar opinion about the non-gravitation of water and air in their natural places, do almoſt always refer \* an experiment of my engine to ſuction, which is indeed the effect of the preſſure of the ambient (as I have † elſewhere clearly ſhewn) and affirm, that the pulp of the finger or hand is drawn up into a hollow pipe, into which it is indeed thruſt by the weight of the ambient air. But all theſe things I have mentioned, not as if I laid any great weight upon each of them, but to let you ſee that it was not altogether without cauſe that I complained of the incompetency of the hiſtory of what divers feel under water; eſpecially at great depths, where this want of information may be more conſiderable; for, as far as I have yet learned by peruſing voyages, and enquiring of travellers of my acquaintance, the places, where they are wont to dive for pearl, are but moderately deep, and indeed ſhallow, in compariſon of the great depths of the ſea; ſo that if we were furniſhed with as many relations of theſe profound places, as we have of the others, poſſibly the accounts would be different enough to render doubtful, or correct the received opinions about the conditions of divers at the bottom of the ſea. For, I remember, that a credible eye-witneſs, who, if I miſtake not, was the intelligent *Oviedo*, ſpeaking of the pearl-fiſhing on the American iſland of *Cubagua*, has, among many other notable obſervations, ſuch a paſſage as this: “ But whereas the place is very deep, a man cannot naturally reſt at the bottom, by reaſon of the abundance of airy ſubſtance, which is in him, as I have oftentimes proved. For although he may by violence and force deſcend to the bottom, yet are his feet liſted up again, ſo that he can continue no time there. And therefore where the ſea is very deep, theſe Indian fiſhers uſe to tie two great ſtones about them with a cord, on each ſide one, by the weight whereof they deſcend to the bottom, and remain there, until them liſteth to riſe again, at which time they unlooſe the ſtones, and riſe up at their pleaſure.”

AND now to come cloſer to the explication of our difficult problem; there yet occurs to me nothing more likely in order to it, than what I have already mentioned in the paper you will meet with about the differing preſſures, &c. And therefore it ſhall here ſuffice me to enlarge, and by further conſiderations and experiments confirm, what is there more ſummarily diſcourſed; namely, that the phænomenon may depend chiefly upon theſe two things, the uniform preſſure of the fluid ambient, and the robuſt texture of a human body expoſed to this preſſure.

IN one of the following ‡ papers, you will find examples of the great preſſure that may be ſuſtained unharmed by ſuch frail bodies as eggs and thin glaſſes, that one would expect ſhould be broken in pieces thereby, provided the preſſure be exerciſed by the intervention of an ambient liquor, as water. And by the account, elſewhere referred to, of the tad-pole, it ſeems highly probable that even that tender animal, when it ſeemed by ſome ſmall diminution of the bulk to be every way a little compreſſed inwards, was put to no conſiderable, or perhaps to any ſenſible pain or inconvenience, ſince it ſeemed to ſwim without any irregular motions, which would in likelihood have enſued, if it had been much harmed or incommodated. Which example, with thoſe formerly pointed at, may teach us, that there may be a vaſt difference betwixt the reſiſtance that a body can make, when compreſſed immediately by ſolid bodies, and when in the compreſſion every way

\* The reaſon of which experiment may be gathered from the fourth Chapter of the Author's long ſince published Defence againſt *Linus*.

† In a paradox about ſuction.

‡ New experiments about the differing preſſure of heavy ſolids and fluids.

ambient fluids intervene. Which you will the less admire, if you consider, that by reason of the grossness, hardness, or rigidity of visible solid bodies, the pressure can never be made every where so equally, as by the parts of liquors, whose smallness, which renders them singly invisible, fits them to accommodate themselves far more closely and conveniently to all the superficial parts of the body immersed in them, and to have the force of the compressing body more uniformly distributed to them. But because the instances referred to are taken from bodies surrounded with water, I will take two or three about the resistance of bodies to violently compressed air; partly, because those made in our engine are wont to be performed with air, not condensed, but rarefied or expanded beyond its usual consistence; and partly, because it will not be denied, that the corpuscles of air may be really compressed or thrust against one another, since it is clear that they may be crowded into far less room than they possessed before, and bear so strongly against the glasses that imprison them, as not seldom, if too much compressed, to burst them in pieces.

CONSIDER then, that among bodies not fluid, the swims of smaller fishes are likely to be judged none of the most able to resist compression, since they consist of bladders so thin and delicate, that a piece of fine Venice paper is very thick in comparison, and that they contain nothing in them but soft air, not compressed by any outward force. I caused one of these bladders, of above an inch in length, and proportionably great, to be taken out of a roach, and anointed it with oil to keep it supple, and preserve it from being pierced or softened by the water; and having by a weight of lead, fastened to the neck of it, let it down to the bottom of a hollow cylindrical tube, sealed at one end, and made purposely large, and about 56 inches long, for some hydrostatical experiments; we could not perceive, that by the weight of all the incumbent water it was manifestly compressed, or that it did discover the least wrinkle or other depression of the very thin membrane, though stuffed but with air. And this trial was made more than once with the same success; and yet, that this proceeded rather from the robustness of the bladder, that was able to resist the weight of a taller pillar of water, than from the non-gravitation of water in the upper part of the tube on that in the lower, we shewed, by presently letting down such a mercurial gage, as is described, and often mentioned in the Continuation of our New Experiments. For letting down this by a string to the bottom of a tube, the weight of the incumbent water forced up some of the mercury out of the open leg of the siphon into the sealed one, and consequently compressed the air included there, which, though it were not very much, yet it was very manifest. For the uncompressed air being three inches and  $\frac{5}{8}$  in length, we judged it at the bottom of the tube about  $\frac{2}{3}$  by the intrusion of the mercury that was impelled up; and to satisfy myself, and others, that if the incumbent water had been heavy enough, it would have visibly depressed the bladder in spite of any *principium hylarchicum*, since I could not have a tube long enough, the bladder was sunk into a crystal glass, that had a long and cylindrical neck, and was so well stuffed with a stopple, that was cylindrical too, that it was very difficult for any thing to get out betwixt it and the orifice of the glass; then a competent quantity of air being left above the water, the stopple was warily, and by degrees thrust down, and so lessening the capacity of the glass, compressed the air that was next it, and by the intervention of that, the water that was under it. And though there did not, upon a slight compression of the outward air, appear any sensible operation upon the bladder that was at the bottom of the water; yet, upon a farther intrusion of the stopple, the pressure being increased, the immersed bladder discovered not only one, but two considerably deep wrinkles, which presently disappeared upon the drawing up of the stopple. Upon whose being thrust in again, depressions were again to be seen on the swim. And we having



having been careful to convey into the same glass such a mercurial gage as has been lately spoken of, we estimated, by the condensation of the air in the sealed leg of that gage, that the bladder had been exposed to a pressure, that might be equivalent to that of a pillar of about forty feet of water.

THIS, I hope, will lessen the wonder, that bodies of so firm a texture as those of lusty men, should support the pressure of the water at such depths, as divers are wont to stay at; since we see, what resistance can be made by so exceeding thin and delicate a membrane stuffed only with air, in comparison of the strong membranes and fibres of a man, stuffed besides air, with more firm parts. I will not here urge, that great weights may be sustained in the air by such tendons or cords of fibres, and by other fibres, as it were, interwoven into membranes, in comparison of what an ordinary man would expect: but I shall invite you to consider with me, that not only upon the account of the stable parts of the human body, but of the spirits too, it may resist very violent pressures (and such as perhaps have not yet been considered) of a fluid body, not only without any manifest contusion or dislocation of parts, but without any sense of pain; which I suppose you will grant me, if, considering what great effects gusts of wind have upon doors, trees, nay masts of ships, blowing them down, nay breaking them; and that yet a man, without being extraordinary strong, will stand against the impetuosity of such a strong wind, and walk directly against it, by virtue of the vigour of his muscles and spirits, without being thrown down, or bruised by so violent a current of air as beats upon him, but without so much as complaining that he feels any pain; and this, though the wind that beats against him, however it be a fluid body, yet because it acts as a stream, does not uniformly compress him, but invade only the fore-part of his body. Likewise, in the lifting up heavy weights by porters, carriers, and other lusty men, we may see the slender tendons of the hands loaded with a hundred, or a hundred and fifty, or perhaps a far greater number of pounds, without having their fibres so far compressed or stretched as to make the lifters complain of pain, though sometimes they may of difficulty. So that (as I could, if it were needful, confirm by other instances) a human body is an engine of a much firmer structure than scholars are wont to take notice of. And here let me add, that I doubt whether, if the structure of a man were not considerably (though not perhaps equally) firm, he would, especially in a deep sea, be able to bear the pressure of the water, though not immediately applied, without pain. For (to give you one reason more of my not acquiescing in vulgar reports about diving) having several times conversed with a man, apt enough both to enquire and observe, who got his living by taking up ship-wrecked goods, he answered me, when I asked him, whether he felt any peculiar pressure against the drums of his ears, which are membranes not so well backed as those of other parts; that when he staid at a considerable depth, as ten or twelve fathoms, under the surface of the sea, he felt a great pain in both his ears, which often put him to shifts to lessen it; which, by his manner of describing it, I concluded was from the incompetent resistance of the air, which he acknowledged to me, he found by manifest tokens to be notably compressed by the superior water. Which relation from such a person does not only confirm our explication, but likewise warrant us to doubt, whether the common reports that are made concerning divers be fit to be relied on, without further examen and observation.

IN the mean time, I shall add two or three experiments more, to confirm the resistance that animals may make to a great pressure, when exercised by the mediation of a fluid body. And I the rather gave you an account of this way of making trials, because it may be also helpful to discover the resistances of inanimate bodies, whose shape and consistence we may choose to vary, almost at pleasure, to the pressure of (totally, or in great

great part) ambient fluids. And if I had been furnished with a tube wide enough, and a quantity of mercury great enough, I might, by the way, have shewn you, that whatever the learned Dr. *More* is pleased to suppose, that to butter itself, even as considerable a pressure may be so applied, as not to be able to make it yield thereunto. For on this occasion I shall add, that I well remember, that, among other trials to the same purpose, I caused a piece of fresh butter, about the bigness of a small hen-egg, to be brought to an irregular shape, that if the compression were such, as many would expect, the long corners, or solid angles, being at least flattened, the butter might be reduced into a more capacious figure, and less remote from roundness. But, though having put this lump of butter into a bladder almost full of fair water, we proceeded, both in the same brass cylinder, and much after the same manner, that I employed about the egg mentioned in the fourth experiment of the tract of the differing pressure of heavy solids and fluids; yet I found, that after the plug had been loaded with a weight of lead of above fifty pounds, neither I, nor the operator, perceived the irregular figure of the butter to be altered. Nor was this the only trial of this kind I made with the like success upon butter, though I dare not charge my memory with the circumstances; and therefore I shall, without delay, proceed to what I was about to recite concerning the resistance of animals.

WE took then a common flesh-fly, neither of the biggest sort of all, nor of the least, but of a middle size; and having put it into the shorter leg of a bent glass, which we caused to be hermetically sealed at the end, there was put in as much mercury as filled that leg, and a part of the other, leaving little more than an inch of air between the quicksilver and the sealed end, that there might be room both for the fly, and the condensation of the air, and then with a little rammer, fitted for the purpose, we caused the mercury in the open leg to be thrust against that in the sealed leg, which thereupon did necessarily crowd the air near the fly into less room; so that, by our guess, it was condensed into about a third part of the space, which it possessed before, and which it regained when the rammer was withdrawn; and though this were done more than once, yet not only the fly was thereby not killed, but not so much, that appeared as sensibly hurt; and I perceived her, whilst she was pent up, to move her legs, and to rub them one against the other, as it is usual with that sort of insects to do of their own accord in the free air. Nor did I question, but that, if the glass had not been inconveniently shaped to admit the rammer farther into it, the fly would have supported a far greater pressure.

ANOTHER experiment, to the same purpose, we tried with water instead of mercury; but, whereas this last named liquor could neither wet nor drown our fly (for which reason I chiefly made choice of it) the other did first wet its wings, and soon after, by a mischance, drown it. But first we had an opportunity to compress the air into a third, if not into a fourth part of its former dimensions; and yet the fly continued to move divers of her parts, and especially her legs, very vigorously, as if nothing troubled her, but her being, as it were, glued to the inside of the glass by part of her wetted wings. And this, I hope, will keep the resistance of divers to the ambient water from seeming incredible; since such flies were able to resist, and, for aught appeared, without harm or pain, the pressure of the crowded particles of the air; though we guessed this to have been as much compressed by the force of the rammer, as it would have been by a cylinder of water of fifty, or between fifty and sixty feet high. By which also we may be helped to conceive, how great a difference there is, whether the same pressure be exercised by a solid, or by a fluid body. For, according to our estimate, the pressure against the body of the fly was as great, as if a slender pillar of marble, having the fly

for



For its bafe, and eighteen or twenty feet in height, had leaned upon the little animal; which, I prefume, you will eafily think was more than enough to cruft her to death.

But becaufe, though the foregoing trials are not like to be rejected by the fkillful, yet they require a fomewhat dextrous and nimble experimenter, and leave fomething to his eftimate, I will fubjoin an experiment more eafy to be made, and wherein the weight may be determined by meafure, rather than conjecture, being made to be perpendicularly incumbent on the fly, or other animal. For the experiment may be as well made on other infects, as worms, though fome, that I had provided, chanced to mifcarry before they came to be ufed.

We took then fome ordinary black flies (fuch as ufe to haunt butchers ftalls in warm feafons) of a middle fize (the length of the body and head of one animal, which for trial's fake we meafured, being about three eighths of an inch) and having placed one of them with the head upwards, that there was fome diftance left betwixt her and the fealed end of the glafs tube nine or ten inches long; we poured in quickfilver very flowly and cautiously, left the force of fo heavy a body, acquired by the acceleration of its defcent, fhould, more than the mere weight itfelf of the liquor, opprefs the fly. To this effect flooping the glafs very much towards the horizon, and letting the mercury pafs into the tube thorough a funnel, whole lower part was very fender, that it might come down but by little and little, we at length got in as much mercury as the tube would receive, and then holding it upright, we watched, whether the fly would make any motions; and finding, that ſhe did manifefly ftir notwithstanding the incumbent mercury, we meafured the height of the mercurial pillar, reaching from the middle of her body to the top of the liquor, and found it to be about eight inches; and the quickfilver being poured out, the fly appeared to be fo lively and vigorous, that I doubted not, but if we had a longer glafs, the experiment had been much more confiderable. But, when afterwards I was able to procure a better tube, the feafon of flies being almoft quite paft, I could ſcarce get any, and thoſe not brisk, as they are wont to be in ſummer. But however, we repeated the experiment with one of the beft we could take of the above-mentioned fize, and ordering the matter fo, that the mercury incumbent on her (for there was ſome beneath her) appeared to be of a greater height than the formerly employed tube was of, we ſaw her move one or other of her little legs divers times, though the tube were held upright; and therefore meafuring the height of the mercury above her, we found it to amount to ſixteen inches and better, and then freeing her from this preſſure, we obſerved, that ſhe immediately found her legs again, and moved up and down briskly enough; but when ſhe was loaden with twenty-three or twenty-four inches of the ſame quickſilver (though the liquor were ſoon after poured out) ſhe gave no ſigns of life; which I ſuſpected might happen, not ſo much from her having been oppreſſed by the greatneſs of her weight, as from the great care of the operator to let down the mercury very obliquely and warily upon her. And this I was the rather confirmed in, becauſe, having got another fly of about the ſame bignefs, though when ſhe was at the bottom of the quickſilver, the ſeemed ſo compreſſed, as not to have any motion, we could take notice of, yet upon her being taken out of the glafs, ſhe preſently appeared to be alive by walking about, and beginning to diſplay her wings, though the pillar of mercury, that had leaned upon her, amounted to above twenty-seven inches. And I preſume, the ſucceſs would have been much more confiderable, if the experiment had been tried in the ſummer, when theſe creatures are brisk and lively, and not, as it was, in the winter; beſides that probably theſe little animals were hurt or weakened by the violence, that would ſcarce fail to be uſed in catching them, and putting them into a place and poſture in the glafs, as was required; the actual coldneſs

coldness of the quicksilver perhaps also making them somewhat torpid, whilst it touched them so many ways. And it must not be here omitted, that a fly, that seemed but about half so big as one of those hitherto mentioned, being well placed, with some mercury under it, in a glass pipe held upright, sustained a mercurial pillar of somewhat above twenty-five inches; and though she was not observed to move under so great a weight, yet when once it was taken off, she did not appear hurt, much less crushed to death by it, and probably would have escaped under a much greater weight, if the tube, which was too large, had not already employed all the stock of mercury we then had at hand. But I do presume, that what we did try, will be available to our purpose, since we see clearly, that so small an animal as a fly may survive so great a pressure, and that she could not only live, but was able to move such long and slender bodies as her legs, when she was pressed against by above sixteen inches of mercury, and consequently by a weight, equivalent to a pillar of water of above eighteen feet and a half, which being above five hundred and ninety times her own length, and, according to the estimate our measure suggested, many times more her own height; so that a diver six feet tall (which is somewhat more than an ordinary man's stature) to have as many times his height of water above him, as our fly might have had, and yet have moved under it, must dive, at least in fresh water, to near a hundred fathom, which is a far greater depth, perhaps by five or six times, than, for aught I could learn by enquiry, the divers either for coral or pearl are wont to descend.

AND now, Sir, having tendered you the likeliest conjectures, that occurred to me about the solution of this difficult problem; I shall return to doctor *More*, and consider the objection he frames from the supposed insolubleness of it. And on this occasion, I shall have two or three things to represent to you.

THE first is, that there would be much more weight in what he objects, if our assertion of the gravitation of water in water were, like the *principium bylarbicum*, a mere hypothesis advanced, without any clear positive proof; whereas our doctrine is not only elsewhere directly proved by particular experiments, but by the very controverted one of the tadpole; to elude whose force, so ingenious a person is fain to fly to a principle, that, to say here no more, is not physical. And from this first of the things I lately mentioned, I shall hasten to the second, because it will require to be longer insisted on.

I SHALL then further represent, that whatever power he is pleased to suppose at the bottom of the sea, to suspend the impression of the incumbent water, I think, that supposition ought to give place, if not to our former ratiocinations, yet to experience itself, which shews, there really is a great pressure exercised by the water at the bottom of the sea. Sir R. M. I remember that a friend of the learned doctor's and mine, who is so eminent a virtuoso, as to have been often president of the royal society, related a while since to me, that a mathematical friend of his, whom he named, having had an opportunity to try an experiment, I have in vain endeavoured to get tried for me, had the curiosity to let down in a deep sea a pewter-bottle, with weight enough to sink it, that he might try, whether any sweet water would strain in at the orifice or any other part; but when he had pulled it up again, he was much surprized to find the sides of his pewter-bottle very much compressed, and, as it were, squeezed inward by the water. I also, not long since, enquired of an observing acquaintance of mine, that has a considerable estate in *America*, whether he had not tried to cool his drink, when he sailed through the torrid zone, by letting down the bottles to a great depth into the sea, and if he did, in what condition he found them when they were drawn up again. To which he answered, that he had several times employed that expedient for the refrigeration of his drinks, but was at first amazed to find the corks, with which the strong stone-bottles had



had been well stopped before, so forcibly and so far thrust in, that they could scarce have been so violently beaten in with a hammer, and it was scarce possible to get them out. And another ingenious person, that practises physick in the *Indies*, having the like question put to him, answered me, that he had some while since had the curiosity to try, in a very deep part of the sea, whether any fresh water would strain into stone-bottles through a thick cork strongly stopped in, and having let it down with a convenient weight to one hundred fathom, was much disappointed, when he drew it up, by finding, that the pressure of the water at so vast a depth had quite thrust down the cork into the cavity of the bottle (which else perhaps would have been crushed to pieces); an effect, which he would scarce have expected from the strokes of a mallet. And if to all this it be objected, that it was not the pressure, but the coldness of the water, that did the recited feats, by condensing the included air, and obliging nature to do the rest for fear of a vacuum; I will not launch into the controversy, whether nature do any thing *ob fugam vacui*, but only answer, that I cannot find, by the relations of the divers, or otherwise, that it is ever so cold at the bottom of the sea, as it is frequently above ground in winter, when great fishes are commonly said to return to the deep parts of the sea for warmth; and yet, in the sharpest winters, I never observed corks to be driven in by the cold of the ambient; nay, I purposely tried with a frigorifick mixture, that very intense degrees of cold, such as would quickly freeze many liquors, would not occasion the breaking of thin bubbles of glass, purposely blown at the flame of a lamp and hermetically sealed.

And to shew *ad oculum* (as they speak) that water may press more and more, as it grows deeper, against the stopple of a bottle, though the vessel be inverted, I will subjoin this experiment. Because we have no water hereabouts, that is near deep enough to force in a cork, as the sea-water did in the above recited trials, I thought of a way of so closing the glass vessel, as that the stopple should keep asunder the air in the vessel and the outward water, and hinder all immediate intercourse between them, and also make some resistance against the pressure of the external water, and yet be capable of freely moving up and down, and so be a good succedaneum to a solid stopple. Taking then a glass phial, furnished with a somewhat long cylindrical neck, whose cavity was large in proportion to the rest of the vessel, we put into it as much quicksilver as would in the neck make a short mercurial pillar of between half an inch and an inch; then a piece of very fine bladder, dipped in oil, was so tied over the orifice of the glass, that no mercury could fall down, or get out, nor water get in at the orifice, and yet the bladder, by reason of its great limberness, might be easily thrust up towards the cavity of the phial, or depressed by the weight of the mercury. This little instrument, first furnished with a weight of lead to sink it, being inverted, the mercury descended into the neck, and closed the orifice as exactly as a stopple, and yet, with its lower part, depressed the bladder beneath the horizontal plane, that might be conceived to pass by the orifice; then the glass being a while kept in the water, that the included air might be brought to the temperature of the surrounding liquor, and by a string let further down into the same glass vessel to about two feet in height, the pressure of the liquor against the orifice of the phial did by degrees drive up the bladder and the mercurial stopple into the cavity of the neck, as was manifest by the ascension of the quicksilver; and when the instrument was leisurely drawn up again, the weight of this mercury made it subside and plump up the bladder again as before. An experiment a-kin to this, and therefore fit to confirm it, I have delivered in another discourse\*.

\* See the Paradox about fusion.

AND here I shall subjoin what very opportunely occurred to me since the writing of the last page. Meeting casually with an ingenious mechanician (whom you will find I have elsewhere † mentioned) that devised a suit of cloaths and other accommodations, wherein I once saw him let down into the water, by whose help, and that of a boat, he could, and did continue there a great while, at a considerable depth under water, and there work; I asked him afresh (to obtain fuller informations than formerly) whether he felt not the pressure of the water against his breast and belly; to which he answered me (more circumstantially than he had before) that when he was about four or five yards under water, though but in the river *Thames*, his breast and abdomen were so compressed, that there being hardly room enough left for the free motion of his lungs, he could scarce fetch his breath, and was necessitated to make them draw him quickly up, and that, among his later trials to improve his engine, having for remedy hereof caused a kind of armour for the chest and back to be made of copper, though the stiffness of the metal defended him from receiving any mischief in those parts, yet in the others, where only the leather, though strong, was interposed, when he came to the depth of about six fathom, though in fresh water, he found a great pressure against his legs and arms and all the other parts against which the water was able to thrust the leathern suit inwards. And this pressure being found by him, as he told me, pretty equal, against all the exposed parts (for from the other, which were more yielding and obnoxious, the armour kept it off) he received no mischief from it, nor yet much incommodity (and some he might expect from the stiffness and unequal yielding of the leather): so that he could stay under water, though not still at so great a depth, about two hours or longer. And upon the whole matter he answered me, that he was well satisfied by his trials, that the ambient water endeavoured to press him and his diving suit every way inwards. Whether the coldness of the water had any interest in this phenomenon, I particularly enquired of the engineer; but he replied, that by reason of the tightness of his diving suit or instrument, the warm steams of his body, that were pent in, and other concurring circumstances, kept him from feeling any cold, and made him sometimes feel a greater heat than he wished. He has promised me, before it be very long, to make for me a trial or two, that I propounded to him, from whose success, if he can but reduce them to experiment, I hope to be able to present you a farther confirmation of our hypothesis. In the mean time, the things already recited, together with the preceding experiments, may well suffice for our present purpose. For, by what hath been said, it appears, that water does actually press against bodies, whether specifically lighter or heavier than itself, placed under water, and that this pressure increases with the height of the water above the emerged bodies. And this being so, it is not more necessary for me, than for men of other opinions, to give a clear reason, why divers can resist so great a pressure of the incumbent water. And the pressure of the water in our recited experiment having manifest effects upon inanimate bodies, which are not capable of prepossessions, or giving us partial informations, will have much more weight with unprejudiced persons, than the suspicious, and sometimes disagreeing accounts of ignorant divers, whom prejudicate opinions may much sway, and whose very sensations, as those of other vulgar men, may be influenced by predispositions, and so many other circumstances, that they may easily give occasion to mistakes. I know, that learned men, that never were conversant in hydrostaticks, are wont to think it very difficult, if not impossible, to conceive, how so weak a thing, as they fancy an animal to be, should avoid the being oppressed, or so much as harmed by so great a weight

† In the tract of the differing pressure of heavy solids and fluids.



of water. But they that shall attentively consider what has been offered towards the removal of this difficulty, and remember how little they would have believed, that there is so great a difference, as we have by the tadpole, the fly, and other instances shewn there really is between the pressure of solid and of fluid bodies, will, I presume, be apt to think it fit, that if, for want of a sufficient history of matters of fact, any scruple remain about the solution we have offered from the nature of the uniform pressure of fluids, and the firm structure of the human body, we should, to remove those remaining scruples also, rather range about for other physical helps to solve more completely the problem, about such a thing as compression, which is an action purely corporeal and mechanical, than for want of a ready and complete solution to fly to the immediate interposition of an immaterial and intelligent, yet created agent, to explain clearly whose manner of working would be a much more difficult task, than the solution of the phenomenon without it.

AND now, Sir, having presented to you the reflections I thought requisite to write upon the learned doctor's discourses against my hypothesis and explications, relating to the gravitation and pressure of fluids, I have little more to trouble you with in this paper. For though in the latter part of the thirteenth chapter the doctor is pleased to spend divers pages in the explication of divers of my hydrostatical phenomena by the agency of that incorporeal director, that he calls *principium hylarchicum*; yet since these explications of his are rather attempts to accommodate the phenomena to the hypothesis, than objections directly levelled against my solutions, I shall altogether forbear to examine them; the main thing that I intended in this paper, according to what I told at the beginning, being to shew that the arguments urged against the mechanical solutions of the experiments by me recited, do not evince any of them to be erroneous. And I have neither the design nor the leisure solicitously to examine the doctor's hylarchical principle. Of which I shall only say, that though he tells us, it is *paratum ad movendum quoquo versum materiam pro data occasione*; yet since he also tells us, *Quò particule molis corporeæ, sive stabilis sive fluidæ, à principio hylarchico in unam aliquam partem omnes junctim urgeri possunt & premi, quamvis singule singulas in nullam partem premant, quodque pro magnitudine molis major minorque totius sit pressio*; and that the force, by which it endeavours to keep the elements in their true and natural consistence, though it be very great, is not invincible: I see no need we have to fly to it, since such mechanical affections of matter, as the spring and weight of the air, the gravity and fluidity of the water, and other liquors, may suffice to produce and account for the phenomena, without recourse to an incorporeal creature, which it is like the Peripateticks, and divers other philosophers, may think less qualified for the province assigned it, than their fuga vacui, whereto they ascribe an unlimited power to execute its functions. I leave it therefore to you, Sir, to judge, which of the two ways of explicating an hydrostatical phenomenon, the learned doctor's, or that which I have made use of, relishes most of the naturalist. And I shall only tell you, that if I had been with those Jesuits, that are said to have presented the first watch to the king of China, who took it to be a living creature, I should have thought I had fairly accounted for it, if, by the shape, size, motion, &c. of the spring-wheels, balance, and other parts of the watch I had shewn, that an engine of such a structure would necessarily mark the hours, though I could not have brought an argument to convince the Chinese monarch, that it was not endowed with life. From which comparison you will easily gather, that what I have thought myself concerned to do in this place, was not to demonstrate in general, that there can be no such thing, as the learned doctor's *principium hylarchicum*, but only to intimate, that, whether there be or not, our hydrostatics do not need it. Nor do I think it necessary

to the doctor's grand and laudable design, wherein I heartily wish him much success of proving the existence of an incorporeal substance. For, as I think, truth ought to be pleaded for only by truth; so I take that which the doctor contends for, to be evincible in the rightest way of proceeding by a person of far less learning than he, without introducing any precarious principle; especially experience having shewn, that the generality of heathen philosophers were convinced of the being of a divine architect of the world, by the contemplation of so vast and admirably contrived a fabrick, wherein, yet taking no notice of an immaterial *principium kylarchicum*, they believed things to be managed in a mere physical way, according to the general laws, settled among things corporeal, acting upon one another. And after this I have nothing more to say, but that I would not have any thing that I have said, misconstrued to the learned doctor's prejudice. For it is not necessary, that a great scholar should be a good hydrostatician. And a few hallucinations about a subject, to which the greatest clerks have been generally such strangers, may warrant us to dissent from his opinion, without obliging us to be enemies to his reputation. And therefore, if you have found any thing in this paper inconsistent with a just tenderness of that, you have not only my consent, but my desire to alter it, as an expression that doth not well comply with my intentions of not appearing any farther his adversary in our debate, than the desire of shewing myself a friend to the truth I was to defend, should exact of,

S I R,

*Yours, &c.*



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A N

# HYDROSTATICAL LETTER,

Written *February 13, 1673.*

CONTAINING

A Dilucidation of an Experiment of the Honourable Author of these TRACTS, about a Way of weighing Water in Water, upon the Occasion of some Exceptions made to it by Mr. *George Sinclair* \*.

\* In his *Hydrostaticks*, printed at *Edinburgh*, 1672, p. 146. ff.

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To the R E A D E R.

WHEN this discourse was just finishing in the press, there came to the publisher's hands a dilucidation of an experiment of the honourable author of these tracts, about a contrivance of his for estimating the weight of water in water, formerly published in Number L. of the *Philosophical Transactions*, and by the following discourse cleared from the exceptions to be met with in Mr. *George Sinclair*'s book, entitled *The Hydrostaticks*, &c. printed at *Edinburgh*, 1672. Which dilucidation, because of the affinity of the subject, was thought fit to be here annexed.

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## HYDROSTATICAL LETTER, &amp;c.

S I R,

CALLING this night in *Paul's Church-yard* for the ingenious Mr. Ray's *Travels*, that you yesterday commended to me, I was also shewn a new treatise, that I never saw before, of a learned gentleman, and hastily running over the index, found an experiment of mine declared insufficient; and though, being hindered to make haste home, it be so late, that, far from having time to peruse the book itself (which I tell you, that you may not now expect any character of it from me) I have been scarce able to read over, more than once, what directly concerns me in it; yet I shall adventure to say something about it this night, for fear I should not, in so busy a time as this, be allowed to do it to-morrow.

Numb.L. WHEREAS then the learned objector, having recited my experiment about weighing water in water, as you were pleased to publish it in a book enriched with so many better things, the *Philosophical Transactions*, begins his animadversions with saying, that "herein is a great mistake;" I shall not in that much oppose him; for possibly the dispute between us is not much more than verbal. And because my experiment coming abroad by itself, and supposing thing, that I had formerly proved, and published, but which were not expressly referred to in it, I wonder not; that my meaning should not by all readers be fully understood. And therefore, to explain myself on this occasion, give me leave both to repeat my opinion, and to shew you on what occasion, and how far, I designed to confirm it by this experiment. My opinion then was, and still is, that as water is a heavy fluid, so it does retain its gravitation and power of depressing; by which I mean a tendency downwards (whatever the cause of that gravity be) whether it have under it a body either specifically heavier or lighter than itself, or equiponderant to it. For I see not what should destroy or abolish this gravity, though many things may hinder some effects of it. And therefore I suppose, that water retains its gravity not only in air, but in water too, and in heavier liquors, and consequently, by virtue of this, the liquor presses upon them; but if a surrounded fluid have, upon the score of its specific gravity, an equal, or a stronger tendency downwards, than water, it will, by virtue of that, be able to impel up this liquor, or to keep it from actually descending: so that a portion of water, supposed to be included in a vessel of the same specific weight with water, this portion, I say, placed in a greater quantity of the same water, will neither rise nor fall, as I have elsewhere shewn; but yet it retains its gravity there; only this gravity is kept from making it actually descend by the contrary action of the other water, whose specific gravity is supposed equal: as when a just balance is loaded with a pound weight in each of its scales, though neither of the weights actually descend, being hindered by its counterpoise, yet each retains its whole weight, and with it presses the scale it leans upon; so that our lately mentioned included portion of water does really press the subjacent water, though it does not actually depress



depress it, or (as perhaps a school-man would phrase it) does gravitate on it, but not pregravitate. Nor do I think, that the only way of judging, whether a body gravitates, is to observe, whether it actually descends, since in many cases its gravity may be proved by the resistance it makes to heavy bodies, which if it were not one, would raise it: as may be declared by what I just now noted about equal weights in a balance: And for want of this distinction I have known even learned men, treating of hydrostatical things, mistake both me and the question.

THE next thing I had to tell you, is, that the adversaries I had to deal with, both in print, and in discourse, denied, that in standing water, the upper parts did press or gravitate upon the lower; and though they could not but grant, that the whole weight of the water did gravitate upon the bottom of the vessel; yet they would have the parts of it to do so *actione communi* (as they speak) and fancied I know not what power of nature to keep the homogeneous portions of water, as well as other elements, from pressing one another, when it is in its proper place. Against this opinion (which I presume my learned adversary and I agree in opposing) it was alledged, besides other things, which I found many, otherwise good scholars, were not fitted to understand, that if a glass phial or bottle, well stopped, were deeply immersed under water, it would strongly tend upwards; but if it were dextrously unstopped, when it was thus immersed, so as the water could get in, abstracting from or allowing for the weight of the glass itself, it would by the water, that crowds in and thrusts out the air, be made strongly to tend downwards, and continue sunk. But this not satisfying, because it was pretended, that the reason of the empty bottle's emerging, when stopped, was the positive levity of the air it was filled with, and the sinking of it, when unstopped, was from the recess of the same air, that by the intruding water was driven with large bubbles out of the bottle; I thought this evasion might be obviated by contriving an experiment, wherein the water should be plentifully and suddenly admitted into the glass, and yet no air expelled out of it (which circumstance I therefore took notice of, where I say, "no bubble of air appeared to emerge or escape through the water") so that if then the glass, that was kept up before, should fall to the bottom, with a gravitation amounting to a considerable weight in respect of its capacity, the sinking of it could not by them be ascribed, as they suppose, with positive levity, but to the weight of the admitted water, which, when thus weighed, would be invironed with water of the same kind: and to shew, that this admitted water might have a considerable weight, notwithstanding the place it was in, I employed a pair of scales after the manner that is recited in the experiment.

By what I have been discoursing, you may conceive, that however my expressions disagree with those of my adversary, the distance of our opinions is not so wide, as at first sight it seems. For he allows, as well as I, that the superior parts of water do by their gravity (for I know not on what other score they can do it) press the inferior. But this he would not have amount to this expression, "that water weighs or gravitates in water;" whereas I scruple not to cloath my sense in that expression, because I think water does always exercise its gravity, though it does not always pregravitate, or actually descend, being often, as I noted above, either impelled up by an opposite and prepollent weight, or hindered from descending by the resistance of other water, that counterpoises it: so that, if he thinks, that in my experiment I meant to propose a method of making water descend in water, and weigh it in that liquor with a pair of scales, just as if I would weigh in the same water a piece of lead, or a portion of mercury, which are bodies much heavier in specie than water, either he mistakes my intention, or I did not sufficiently declare it. But that which I designed to shew, and, for

for aught I can yet see, have shewn, was, that by the help of an ordinary balance it may be made appear, that water admitted into the glass-bubble, I employed, did make the glass-bubble weigh so much heavier than it did before that liquor entered into it; and that this new weight, that was manifested by the balance, was not due, as my adversary supposed, to such a recess of the air, as I mentioned a while ago.

AND now, Sir, it will be proper to take notice of some passages in the objector's discourse, in order to dilucidate the subject of it. Whereas he says (page 149, and 150.) "Take a piece of wood, that is lighter in specie than water, and add weight to it by degrees, till it become of the same weight with water; knit it with a string to a balance, and weigh it in water; and you will find the whole weight supported by the water." I answer, that this does not at all overthrow my opinion, but agrees very well with it. For suppose the weight you add to the light wood be lead, it cannot be said, that the metal loses its native ponderosity, whilst it rests in the water; and the reason, why it descends not, is, that it and the wood it is joined to, are hindered by the counterpoise of the collateral water, which, by its pressure, would raise the surface of the water, whereon the floating or swimming body leans, if it were not hindered by the weight of these incumbent solids: and this resistance of theirs to the endeavour upwards of the water, being exercised only upon the account of their gravity, shews, that they do in my sense gravitate, though not pregravitate.

AGAIN, if you please to consider the case put by the objector, page 151, and cast your eyes upon his scheme, which, supposing you to have his book, I shall, for brevity  
 Page 151. sake, make use of at present; you will find him thus argue: "Now, I say, it is six ounces of the weight (*B*) that makes this alteration, and turns the scales: for if twelve ounces sink the glass below the water, when it is full of air, and no water in it, then surely six are sufficient to sink it, when it is half full. And the reason is, because there is a less potentia, or force, in six inches of air, by the one half, to counterpoise a weight of twelve ounces, than in twelve inches of air. Therefore this air being reduced from twelve inches to six, it must take only six ounces to sink it."

To which I answer, that I know not yet what, on this occasion, he means by a potentia, or force, in six inches of air, to counterpoise a weight of twelve ounces. For by the term counterpoise, where the question is about weighing, one would think he speaks of weight; and yet air, according to the vulgar opinion, is positively light; according to us, though it have a gravity, yet in our case that must amount to so little, that what air the bubble needed to fill it, could not weigh at most above four or five grains, which therefore might safely be neglected. But, according to my opinion, the reason of the phenomenon is clear enough, without meddling with the potentia of the air. For if we conceive a horizontal plane to divide the water mentally, and pass by the bottom of the suspended bubble; before the little stem be taken off, there is a far greater pressure upon the other parts of that plane, than upon that which lies under the bubble, in regard they are pressed by the weight of the collateral water (*A*, *L*, *G*, *D*, *M*, *C*.) whereas the other is pressed only by the weight of a body very much lighter than its equal bulk of water: so that, to keep the bubble from being forcibly buoyed up, there was requisite eighteen ounces of lead, that make up the plummet (*B*), to detain it under water, and keep the beam of the balance horizontal; that when access is given (at *C*) to the neighbouring water, it is by the weight of the collaterally superior water impelled into the cavity of the bubble, where the air, being much rarefied before, could not resist its ingress, and thereupon, six ounces of water getting in, that part of the imaginary plane, on which the bubble was incumbent, is pressed by a greater weight.



weight than formerly by six ounces, and consequently, there needs the like weight in the opposite scale of the balance, to reduce the scale to an æquilibrium. And if we suppose, with our author, the glass to be completely full of water, and the counterpoise in the scale (O) to need six ounces more to make a new æquilibrium, the account of the phenomenon will be the same, as, if you attentively consider it, you will clearly perceive. And the reason why the additional weight of six ounces is required, will be, that the upper half of the bubble, that before contained less than three or four grains weight of air, being now filled with water, amounted to six ounces more of water than formerly, and so the counterpoise, in the opposite scale (O) will need the weight of six ounces to make a new æquilibrium.

CONGRUOUSLY to this explication, when the examiner says, “ Now I enquire, whether Page 152.  
“ these eighteen ounces are the æquilibrium of the water within the glass, or of the  
“ weight of the lead (B)? It is impossible they can counterpoise both, seeing the water  
“ is now twelve, and B eighteen. It must then either be the counterbalance of the wa-  
“ ter, or the counterbalance of the lead. It cannot be the first, because twelve cannot  
“ be in æquilibrium with eighteen; it must then be in the second: or if these eighteen  
“ ounces in the scale (O) be the counterpoise of the water within the glass, I enquire  
“ what sustains the weight of the lead (B)? the weight of it cannot be sustained by the  
“ water, because it is a body naturally heavier than water, it must therefore be sustained  
“ by the balance.” I answer, that this specious objection seems (for it is somewhat  
obscurely worded) to be founded upon a mistake of my meaning in the question. How-  
ever, as to the phenomenon itself; according to my sense, the eighteen ounces in the  
scale (O) are the counterpoise of the eighteen ounces, that hang from the opposite and  
æquidistant scale, and make up the leaden plummet (B) which answer, I see not how  
our author prevents. But then you will ask, what counterpoises the water in the  
bubble, which alone weighs twelve ounces? I answer, that it is the gravitation of the  
collateral water, which presses the other parts of the lately-mentioned imaginary plane,  
as much as the water in the bubble, the weight of the glass being here not reckoned by  
either of us; and the water incumbent on the bubble does press that part of the plane  
on which they lean; so that there being in all thirty ounces to be sustained, the eighteen  
of the plummet, and the twelve contained in the glass, the lead, that hangs in the wa-  
ter, is counterpoised by eighteen ounces in the scale, and the water in the bubble by the  
pressure of the collateral water.

BUT you will say, that it appears not, that the included water presses at all, since it  
does not at all descend. To which I answer, that as long as the water was getting into  
the cavity of the bubble, so long it did manifestly gravitate upon the subjacent plane,  
and actually descend, raising the counterpoise in the scale: but when, by adding more  
weight to that counterpoise, things are brought to a new æquilibrium, there is no rea-  
son, why the gravitation of the water should again change the now regained æquipo-  
ndium. Suppose, in the two scales of a balance there were placed two equally capacious  
and equiponderant phials, whereof one is quite full, and the other almost full; it is evi-  
dent, that the full vessel will keep the scale it leaned upon depressed, and if you gently  
pour in as much water into the unfilled, as the filled has more than it, the scale, that  
was formerly kept raised, will be now depressed, till the beam be brought to be hori-  
zontal; to which posture when it is once brought, the æquilibrium will continue: and  
yet it will not be said, that though the added water, whilst it was filling the glass, de-  
pressed the scale it belonged to, yet it lost its weight, or, which in my sense is all one,  
did not gravitate upon the scale, when the balance was come to an æquilibrium, be-  
cause then this water did no longer depress it. And how much the water in our bubble

does, notwithstanding its immersion, gravitate, would be visible, if, by supposition, it were all annihilated, and no other suffered to supply its room. For then the subjacent part of the imaginary plane being much less pressed, than immediately before, the weight of the collaterally superior water would strongly impel up the bubble, if it were not kept in its place by a proportionable addition of weight to the plummet. Nor should it seem a strange thing, that I should say, that the thirty ounces, lately mentioned, should be counterbalanced partly by the weight in the opposite scale, and partly by the water that fills the immersed bubble; since this notion may be warranted even by the common practice of weighing heavy solids hydrostatically. For if you would, for instance, weigh a lump of copper of nine pounds in common water, the metal, hanging by a horse-hair under water, will need, according to my elsewhere mentioned experiments, either just or near about eight pounds in the opposite scale, to keep the balance horizontal, so that the whole nine pounds, that the lump weighed in the air, is counterpoised partly by the eight pounds newly mentioned in the opposite scale, and partly by the weight, or resistance following from weight, of as much of the water as the copper fills the room of; which, as experience shews, is one pound: and if we should conceive water in a vessel adiaborous, as to gravity and levity, to be substituted in the place of the metalline lump, it would weigh as much as the ninth part of the copper lump weighed in the air, and the same counterpoise of eight pounds would maintain the æquilibrium.

WHAT the learned objector has, at the close of his discourse about the natural and artificial balance, could not without prolixity, and is not here necessary to be dwelt upon; especially since you will see, in what I suppose you have now received from the press, in answer to the ingenious Dr. *More*, what is to be said on that subject, according to my hypothesis. Wherefore, though my learned adversary does in the 152<sup>d</sup> page conclude, "That water cannot weigh in water," and asserts "that the pressure of water is one thing, and water to weigh in water is another;" yet, as I said at first, I conceive much of our difference may be verbal; and, in my sense, when water presses subjacent water, because it does so upon the score of its gravity, it gravitates in water, though it does not pregravitate, that is, actually descend. And since it is in the sense of this last expression, that our author, if I mistake him not, speaks of weighing in water, his conclusion, that water cannot weigh in water, does not contradict me, who affirm not, that water does so weigh in water. Whether we shall agree in all other points of Hydrostaticks, you will easily believe, that I cannot yet tell; though by the expression he is pleased to use in the 146<sup>th</sup> page, to usher in his objection with, it is probable we may. And as to the now-dispatched debate, if I have employed some words in another sense than he, I presume he is so equitable as to consider, that I did not write of these things after having seen this book of his, but some years before; and have since found those expressions justified by the use that eminent writers have thought fit to make of them. And however I am glad, that he has given me this opportunity of clearing my experiment, and declaring by examples, as well as words, the opinion it relates to; especially, if it seems to others, that I omitted to express myself so fully; my design being, as I formerly told you, to convince such adversaries, as I then had met with, by shewing, that the above-recited phenomena of the emersion and sinking of a glass phial depended upon the gravity of the water, and not upon the positive levity of the air.



# NEW EXPERIMENTS

Of the POSITIVE or RELATIVE

## LEVITY OF BODIES UNDER WATER.

IT is obvious even to the vulgar, as well as to philosophers, that if wood, wax, or another body that is lighter in specie than water, and naturally floats upon it, be detained under water, it will, upon removal of that force, emerge to the top. And this it does so readily, and, as it seems, spontaneously, that not only the Peripatetick schools, but the generality of philosophers, both ancient and modern, do, as well as the vulgar, ascribe this ascension of lighter bodies in water to an internal principle, which they therefore call positive levity.

BUT this principle was not always so universally received among philosophers, as in later ages it proved to be; *Democritus*, and several of the ancients, both atomists and others, admitting no absolute, but only a relative or respective levity; which opinion some of the moderns have ingeniously attempted to revive.

BUT, because whatever wit they may have employed in arguing, yet the schools seem to have the advantage in point of experience, the obvious instances given by the Peripateticks having neither been solved by real and practical variations of the same instances, nor counterbalanced by new experiments of a contrary tendency; the importance and difficulty of the subject invited me to attempt, when I was upon hydrostatical trials, whether I could experimentally shew, that whatever becomes of the general question about positive levity, we need not admit it for the true and adequate cause of the emersion of wood, and such lighter bodies, let go under water.

### EXPERIMENT I.

THE instance that is wont to be urged to prove the positive levity of wood in water, seems to me to have been too perfunctorily made to be safely acquiesced in. For even as it is proposed with advantage by a learned foreign mathematician, I cannot think it accurate enough to determine the present controversy: for I will readily allow him to suppose, that in case a flat board, as for instance, a trencher, have its broad surface kept by a man's hand or other competent force upon the horizontal bottom of a tub full of water, if the hand or other body that detained it be removed, it will ordinarily happen, that the trencher will hastily ascend to the surface of the water. But I do not perceive, that a decisive experiment of this kind is easy (not to say possible) to be made

with such materials. For the wood, whereof both the trencher and the bottom of the barrel consists, are supposed to be lighter in specie than water; and to be so, they must be of a porous and not very close texture. To which agrees very well, that the solidier woods, as *lignum vitæ*, Brasil, &c. whose texture is more close and compact, will not float on water, but sink in it: and therefore, if there be not much more care used, than I have yet heard that any experimenter has employed, to bring the surfaces of the trencher, and the bottom of the barrel, to a true flatness, and as much smoothness, as they can be brought to, I shall not think the trial so accurately made, as it might be; not to say, which I suspect, that though it be mentally, yet it is scarce practically possible to bring such porous bodies, as those of the lighter woods, to be fit for such a contact as might be necessary to make the trial accurately. And in case that were actually done, I should be kept from expecting, with my adversaries, the emersion of the trencher, by the experiment by and by to be recited, and by the true reason of it.

I THINK then that the cause why, in ordinary instances, wood, wax, and other bodies specifically lighter than water, being let go at the bottom of a vessel full of that liquor, emerge to the top, is chiefly, that there is no such exquisite congruity and contact between the lowermost superficies of the wood, and the upper surface of the bottom of the vessel, but that the lateral parts of the water, being impelled by the weight of the parts of the same liquor incumbent on them, are made to insinuate and get between the lower parts of the wood and the bottom of the vessel, and so lift or thrust upwards the wood, which bulk for bulk is less heavy than the water that extrudes it.

THAT this is the reason of the emersion or ascension of bodies, lighter in specie than the fluids they swim in, is most consonant to the laws of \* *Hydrostaticks*, as I have elsewhere shown. But whereas the whole force of the argument of those I dispute with, consists in a supposition, that, because the trencher (formerly spoken of) is placed upon the bottom of the barrel, no water can come between to buoy it up, whence they conclude, it must ascend by an internal and positive principle of levity, I thought fit to make the experiment after another, and, if I mistake not, a better manner.

WE took then two round plates of black marble shaped like cheeses, which had those superficies that were to be clapped together ground very flat, and polished very carefully, that the stones being laid one upon the other, might touch in as many of the superficial parts, as the workman could bring them to do; that, whilst they were in that position, the uppermost being taken up, the other would stick to it, and ascend with it. And to keep out the water the better, the internal surfaces were, before they were put together, lightly, and but very lightly, oiled; which did not hinder them from most easily sliding along one another, either forward or backwards, or to the right, or to the left, as long as the contiguous surfaces were kept horizontal.

THESE things being done, a blown bladder, of a moderate size, was fastened to the upper marble, and both of them were let down to the bottom of a tub of water, where, by the help of an easy contrivance, the lower marble was kept level to the horizon. And now the patrons of positive levity would have concluded, that the bladder, being a body, granted to be by vast odds lighter than wood, and being in an unnatural place beneath the surface of the water, should, of its own accord, and with impetuosity, emerge; but I expected a contrary event, because the bladder being tied to the upper marble, so that both of them might in our case be considered as one body, the water could not impel them up, in regard that the close contact of the surfaces of the two marbles kept

\* See the *Hydrostatical Paradoxes*.



the water from being able to insinuate itself between them, and consequently from getting underneath the upper marble, and pressing against the lower superficies of it. And to shew that this was the reason of the bladder's not emerging, I caused one of the bystanders to thrust his arm down to the bottom of the tub, and with his hand to make part of the oiled surface of the upper marble slide off, on any side, from that of the lower, which, by reason of the smoothness and slipperiness of the surfaces, he found most easy to do. But the contact still continuing according to a greater part of the surfaces than was requisite, I bid him yet slide, but by slow degrees, more and more of the upper marbles from the lower, till at length, when, according to his guess, the marbles touched but in one half of their surfaces, the endeavour of the water to extrude the bladder full of air being stronger than the resistance, which the contact, but of part of the surfaces of the stones, was able to make, they were suddenly disjoined, and the bladder was by the extruding water impetuously, as it were, shot up, not only to the top of the water, but a good way beyond it.

With these marbles we made several other experiments of this kind, most commonly letting down the marbles both together; but once or twice at least placing the upper marble under water upon the lowermost already fixed to the bottom of the barrel.

THAT it was not the weight of the upper marble, nor want of lightness, whether positive or relative, of the air included in the bladder, that kept it from ascending, was plain, not only upon the newly-mentioned impetuous emersion of it, upon the disjoining of the marbles, but by this, that the bladder would lift up from the lower parts of the water, not only the upper stone, when it touched not the other, but a weight of seven or eight pounds hanging at it.

AND that a *fuga vacui* was not an adequate cause of the cohesion of the marbles in our experiment, may be argued from this, that whether or no nature do any thing, at any time, out of abhorrence of a vacuum (which may be much disputed); yet, in our case, this abhorrency could not be well pleaded by its assertors, since many of them hold it to be unlimited, and the more modest, to be at least capable of lifting up prodigious weights; whereas, in our experiment, the levity of a bladder, that could not raise ten pounds weight, was sufficient to disjoin the marbles, when they yet touched one another according to half their surfaces.

## EXPERIMENT II.

To shew now, whether it is not rather the gravity and pressure of the water, or other ambient fluid, than the positive levity of a lighter body in specie than it, that makes the immersed body ascend to the surface of the liquor, I devised this experiment:

WE took a bladder, out of which a great part of the included air had been expressed, and tying the neck of it very close, that none of the remaining air might get out, we fastened to it a considerable weight of some very ponderous body, as lead or iron. By the help of this we sunk the bladder to the bottom of a wide mouthed glass, full of water, that the surface of the liquor might be a good deal higher than the upper part of the bladder: this wide mouthed glass we included in a great receiver (whose orifice must be very large to be able to admit such a vessel) which I caused to be carefully cemented on to the engine. The main scope of this experiment was to shew, that though the air included in the bladder was very far from being able, by its absolute levity, to lift up so great a weight, as the bladder was clogged with, yet the same air, continually included in the bladder, would, by its mere expansion, without any new external

external heat, acquire a power of ascending in spite of that weight; which ascension therefore must be attributed to the water, which, according to the laws hydrostatical, ought, *ceteris paribus*, to resist, or buoy up more potently those immerfed bodies, that being lighter in specie, than it, possess the greatest place in it, and hinder the more water from acquiring its due situation: as we see, that among hollow spheres of glass and metal, equally thick and well stopped, there is a much heavier weight requisite to sink a large one than a small one. For the prosecution of this trial, we began to pump the air out of the great receiver; and its pressure upon the surface of the water being thereby more and more lessened (according to what we elsewhere more fully declare) the spring of the included air began by degrees to distend the sides of the bladder, till at length that vessel of air swelling every way, took up so much more room in the water than it did before, that the water was able to lift the bladder and the annexed weight to the top, and detain it there, till we thought fit to let in again some of the excluded air, which forcing that in the bladder to shrink in its dimensions, the weight was presently able to sink it to the bottom.

AND here it may be noted, that if, instead of hanging so great a weight at the neck of the bladder, we fastened but a moderately heavy piece of lead, such as would only serve to sink the bladder, and keep it at the bottom of the water, so that the aggregate of the bladder, air, and metal, was but a little heavier than a bulk of water equal to them; then, upon the first suck or operation of the pump, which could withdraw but a small part of the air in the receiver, the air in the bladder suddenly expanding itself, would forthwith be impetuously extruded by the water, though after some reciprocations it would float in its due position, till upon the return of a little outward air, sometimes as little as we could conveniently let in, it would immediately subside.

BUT this is not so necessary to be insisted on, as it is to take notice, that I foresaw it may be objected, that the ascension of the weight was not effected by the pressure of the water, but by this, that rarity and levity being qualities exceedingly of kin, the great rarefaction of the air might proportionably increase the levity of it, and consequently enable it to perform much greater things than it could do before.

I WILL not here dispute, whether, generally speaking, a body rarefied without heat would, in vacuo, or in a fluid not heavier in specie, than the body when rarefied, merely, by such a greater distance of its parts as may suffice to entitle it to rarefaction, become really heavier or lighter than before. I will not, I say, discuss this question here, where it may serve my turn to satisfy the recited objection by the following experiment.

### EXPERIMENT III.

ABOUT the neck of a conveniently shaped phial capable to hold some few ounces of water, I caused to be carefully tied the neck of a small bladder, whence the air had been diligently expressed, so that the bladder, being very limber of itself, and probably made more so, as well as more impervious to air and water, by the fine oil we had caused it to be rubbed with, lay upon the orifice of the phial like a skin clapped together with many folds and wrinkles.

THIS done, we let down the phial into a conveniently shaped vessel full of water, and the phial, being poised beforehand for that purpose, sunk perpendicularly in the liquor, till the neck of the glass was partly above and partly beneath the surface of the water: then covering the external glass with a large receiver, we caused the air to be pumped out, and as the pressure of that was gradually withdrawn, the air in the float-

ing



ing phial did little by little expand itself into the bladder, and unfolded the wrinkles of it, till at length it became full blown, without altering the erected posture of the glass it leaned upon. But this great expansion being made above the water, and consequently in a medium not heavier than the included air, gave that highly rarefied air no such increase of levity, as enable us to perceive that it made so much as the neck of the glass arise higher in the water than it did before. Nor did we take notice, that the return of the air into the receiver, by reducing the air in the bladder to its former unrarefied estate, made the glass sink deeper than before. But when the experiment was tried with the same glass and bladder, at the bottom of the water, then, upon the pumping out the air, the bladder being dilated under water, was, after a while, carried up to the top, and took up with it about eight or ten ounces, that had been to clog it, fastened to the bottom of the phial.

# NEW EXPERIMENTS

About the PRESSURE of the

## AIR'S SPRING

ON

## BODIES UNDER WATER.

**I** DO not think it were difficult for an intelligent peruser of our physico-mechanical experiments, to find there divers phenomena, whence it may be deduced that bodies under water, though kept by that liquor from the immediate contact of the air, may yet be exposed to its pressure, whether the air act as having a weight, or as a spring. But because not only the vulgar, but philosophers, have been so long and generally possessed with an opinion, that a fluid so little heavy as the air, cannot by its weight act upon a liquor, that is, like water, bulk for bulk, a thousand times heavier than it; and because also it seems yet more strange, that a little air, perhaps not amounting to a scruple or drachm in weight, should in its ordinary state of laxity act considerably upon bodies, which, being covered with water, seem, by the interposition of that liquor, to be fenced from the incumbent air; it may be worth while to add three or four hydrostatical experiments, to confirm a truth that very few are yet acquainted with; and add to the proofs already given of the power of the spring of the air, some of the operations we have discovered it to have upon bodies placed under water.

THERE are two sorts of trials that I shall employ to shew, that a small quantity of inclosed air may, by its pressure (which in our cases must depend upon its spring) have  
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a considerable operation upon bodies under water, notwithstanding the interposition of that liquor.

For this pressure we speak of may be manifested, in the first place, by what it directly and positively operates upon bodies covered with water; and, in the next place, by the things that regularly ensue upon the removal of the inclosed air, or the weakening of its spring.

## EXPERIMENT I.

To begin with the former way of shewing the pressure of the air, I thought it sufficient, in regard of the trials to be referred to the second way, to make the following experiment.

WE took a square glass phial, guessed to be capable of holding between half a pint and a pint of water; the neck of this we luted on carefully and strongly (for else it would have been buoyed up) over the orifice of the small pipe, at which the air passes in our engine out of the receiver into the pump; then whelming over this glass a great receiver, we luted it strongly to the engine (that it might as well keep in the water as keep out the air) and at the top poured in as much water as sufficed to environ the internal receiver (if I may so call it) and cover it to a pretty height. This done, we exactly closed with a turning-key the hole in the great receiver, at which the water had been poured in, that no air might get in or out that way. And lastly, we began to pump out the air contained in the internal receiver; to the end that that air, which by the above-mentioned pipe had communication with the external air, might no longer by its pressure assist the glass to resist the pressure, which the incumbent and inclosed air, by virtue of its spring, constantly exercises upon the subjacent water, and by its intervention upon the sides and bottom of the internal receiver.

AND as we expected not, that this glass by its own single force, should resist the pressure of the air inclosed in the upper part of the great receiver, notwithstanding the interposition of the water, so the event fully justified our conjecture; for at the first exsuction, which could not be supposed to have well emptied the internal glass, this vessel was, by the pressure of the superior air upon the circumstant water, broken into I know not how many pieces. And the same experiment, though with a little slower success, was repeated with a stronger internal glass.

## EXPERIMENT II.

I PROCEED now to the second way of manifesting the pressure of inclosed air upon bodies under water, which is by shewing the phenomena exhibited by those bodies upon the removal or lessening of that pressure.

HAVING squeezed out of a moderately sized bladder the greatest part of its air, we tied the neck of it very close, and then fastening to it a competent weight, we placed it at the bottom of the tallest and largest glass we could cover with our great receiver, that so, though the incumbent air were pumped out, none of the water might be pumped out with it, but still retain the same height above the bladder. Having then poured upon the bladder as much water as would swim a great way above the upper part of it, we covered this glass of water with a great receiver, which being carefully cemented on to



to the engine, the pump was set a work, and as the air, which by its spring pressed upon the surface of the included water, was by degrees pumped out, so the air that was imprisoned in the bladder, did gradually expand itself at the bottom of the water, as if no such liquor had interposed between them otherwise than by its weight, upon whose account it must be allowed to give some little impediment to the expansion of the bladder, in proportion to the height it had above it.

THE event of our experiment was such as was expected, namely, that the immersed bladder was at length full blown, by the dilatation of the air inclosed in it; and by its intumescence made a considerable part of the water run over by the sides of the glass that before contained it all. And when access was given again to the external air, the internal being compressed, the bladder was presently reduced to its wrinkled state.

### EXPERIMENT III.

WE took a small but fine bladder, whose neck was strongly tied up, when it was, by guess, about half full of air; this we put into a short brass cylinder, the lower of whose bases was closed with a brass-plate, and the other left open; this open orifice we afterwards stopped, but not exactly, with a cylindrical plug, that was somewhat less wide than it, and was by a rim at the top hindered from reaching too deep into the cavity of the cylinder, that it might not do mischief to the bladder that lay there beneath it; upon this plug we placed an almost conically shaped weight of lead, and this pile of several things being so placed upon our engine, that we could cover it with a great receiver, we carefully cemented on this vessel, and at the top of it poured in so much water as would serve to fill the vacant part of the brass cylinder, and the cavity of the engine to such a height, that it covered all the leaden weight, which was several inches high, except a rim, which was fastened to the top of it for the convenient removing of it.

ALL this being done, the pump was set to work, and long before we had exhausted the air of the receiver, that, which was inclosed in the lank bladder, had by degrees displayed so vigorous a spring, that it had heaved up the weight that lay upon it to a notable height, and kept it there till the air was let in from without, to assist its being depressed by the leaden weight, which amounted to no less than about 28 pounds.

### EXPERIMENT IV.

THERE remained yet one trial to be made, which, in case it should succeed, seemed likely to appear as great an evidence of the force of the air's spring upon bodies under water, as could be reasonably desired of us; it having been looked upon by many virtuosos, as the considerablest instance of the force of the air's spring, even when no water intervened in the trial.

To satisfy, therefore, our curiosity, we took a copper vessel of a cylindrical shape, and a considerable height; into this, being first almost filled with water, we put a square glass-phial, capable, by guess, to hold nine or ten ounces of water, and exactly stopped with a cork and a close cement; this phial, by a competent weight, was detained at the bottom of the water, from whose upper surface it was considerably distant; then the copper vessel being placed upon the engine, and included in a great receiver well cemented on, the air was by degrees pumped out; but before it was quite exhausted, the glass at

the bottom of the water was, by the spring of the air included in it, burst into many pieces, not without great noise, and a kind of smoke or mist that appeared above the surface of the water.

ANOTHER glass of the same sort had been broken after the same manner in another vessel; but having afforded us no particular phænomenon, I barely mention it, to shew that we made more than one trial of this kind.

THE consequence that will naturally result from the three last experiments, is this, that since barely upon the withdrawing of the pressure of the included air (which was perhaps but very little in quantity) the air residing in the immersed bodies did, by virtue of its spring, expand itself so forcibly as we have recited, and perform notable things, the air above the water must have exercised a very powerful pressure upon the surface of it, since (setting aside the weight of the water, of small moment in our trials) it must have been, at least, equivalent to (and probably much exceeded) that force of the immersed air, whose exercise it was able totally to hinder.

AND from hence it may be easily deduced, that the weight of the atmosphere acts upon bodies under water, notwithstanding that the interposed liquor is, by vast odds, heavier in specie than air; for we have just now proved the pressure of inclosed air (which consists in its spring) upon bodies under water; and it is manifest, that the strength of the spring of this inferior air, we make our trials with, is caused by the weight of the superior air, which bends and compresses those little aerial springy particles, whereof our air consists; so that the weight of the atmosphere being equivalent to the spring of the inferior air (for else it could not compress it as much as it does) must lean upon the surface of the subjacent water, with a force equivalent to the spring of that part of it that is contiguous to the water.

THIS experiment brings into my mind another, that I once made, which, though not properly hydrostatical, yet relating to positive levity, may perhaps be not uselessly added on this occasion; wherefore I shall here subjoin a transcript of the phænomenon that belongs to our present purpose, as it is registered soon after the experiment was made.

[To examine, by a visible experiment, the common doctrine, that a portion of air, by being much dilated, rarefied, or expanded, does acquire a new and proportionable degree of positive levity, I devised to put in practice the following way:

WE took a bladder of a moderate size that was very fine and limber, that it might be the lighter and more easily distended. The most part of the air being squeezed out of the bladder, the neck of it was tied up very close, that no air might get out of it, nor any external air get into it. This limber bladder was hung at one of the scales of a balance, whose beam had been purposely made more than ordinarily short, that the instrument (which yet was ticklish enough) might be suspended, and capable of playing in the cavity of a great receiver, into which we conveyed it, having first carefully counterpoised the bladder with a metalline weight put into the opposite scale.

THIS done, the air was pumped out, and, as that was withdrawn, the bladder was more and more expanded by the spring of the internal air, till at length, when the receiver was well exhausted, it appeared to be quite full. Notwithstanding which great dilatation of the included air, it did not appear by the depression of the opposite scale, to be grown manifestly lighter than it was at first. And the bladder seemed also to retain the same weight, after it had, by the air that was let into the receiver, been compressed into its former wrinkled state.]



## NEW EXPERIMENTS

ABOUT THE

DIFFERING PRESSURE

OF HEAVY

SOLIDS AND FLUIDS.

SINCE not only in vulgar spectators of physico-mechanical experiments, but even among some learned men, it has proved a great impediment to men's freely acquiescing in the doctrine founded on those phenomena, that if the atmosphere could really exercise so great a pressure, as we ascribe to it, it would unavoidably oppress and crush all the bodies exposed to it, and consequently neither other animals nor men would be able to move under so great a load, or subsist in spite of so forcible a compression.

THIS I readily grant to be a plausible objection; but I suppose the force of it will be taken away by the following considerations put together.

AND first, the power of pressing, that we ascribe to the air, is not a thing deduced, as too many other consequences in physick are, from doubtful suppositions or bare hypotheses, but from real and sensible experiments. And therefore since we have clear and positive proofs of the pressure of the air, though we could not explain how men and other animals are not destroyed by it; yet we ought rather to acknowledge our ignorance in a doubtful problem, than deny what experience manifests to be a truth; as is generally practised in treating of the attractive and other powers of the loadstone, which are freely acknowledged, even by those that confess themselves unable to explicate them; though, if experience did not satisfy us of them, they were liable to divers more considerable objections than any that is urged against the pressure of the air.

SECONDLY, but though it be not absolutely necessary that we should answer the above-recited objection otherwise, than by thus declaring that the spring of the air is not to be rejected for it; yet we will endeavour very much to lessen it, if not quite remove the difficulty, before we put an end to the discourse.

I CONSIDER then, thirdly, that they that urge the lately mentioned objection against the great pressure of the air, seem not to be aware that we were conceived and born in places exposed to the pressure of the atmosphere, and therefore how great soever that pressure appeared to be, it ought not to crush us now, since when we were but embryos, or new-born babes, and consequently very much more weak and tender than we now are, we were able to resist it, and not only live, but grow in all dimensions in spite of it.

If there were any place about the moon, or some other of the celestial globes that some learned men fancy to be inhabited, that has no atmosphere or equivalent fluid about it, and where yet men could be generated a new, if one of those men should be supposed to be transported thence, and set down upon our earth, there might be made an experiment fitted for our controversy. In the mean time, I doubt, that since nature is not observed to make things superfluously strong, such a human body being not made to resist any weight or pressure of air, would be of so tender and compressible a make, that it would easily be crushed inwards by our atmospheric pressure. And though we cannot give an instance of this kind, yet we make trials somewhat analogous to it in our pneumatical engine. For when we place water in our receiver, and pump out the air that was above it, there will be generated a multitude of bubbles, some of which, when the air is carefully withdrawn, will be of a strange and scarce credible bigness; these bubbles being generated where the air cannot press upon them, these dimensions are so natural to them, that if the receiver be supposed not to leak, nor other unfriendly accidents to intervene, they would (for aught we know) last a good while; since I have elsewhere shewn, that the spring of highly dilated air did continue for many months, and a bladder would for no less time continue blown and filled in our vacuum by a little air that was left in it, when the ambient air began to be withdrawn from it. And yet the large bubbles above-mentioned, when once the outward air is suffered to come in upon them, are thereby so violently compressed, that in a trice they shrink into dimensions too small to keep them so much as visible; and if I could have succeeded in my attempt of producing such living bodies as I endeavoured (but did not expect) in our vacuum, I suppose the success would have confirmed what I have been saying.

FOURTHLY, but you will tell me, that so great a weight and pressure, as I assign the atmosphere, must needs make a man feel pain, and, if not, otherwise dislocate some of the parts, must, at least, press the whole body inward.

BUT, first, being accustomed to the pressure from our very birth, and even before it, so early and long an accustomedness hinders us from taking notice of it; those pressures only being sensible to us, that are made so by some additional cause, which, by making a new impression, excites us to take notice of it. So we are not sensible of the weight of the cloaths we are accustomed to wear; and so a healthy man is not sensible of the heat in his heart, because it is constant there, and the sentient parts of the heart have been still used to it, whereas that heat oftentimes has been very considerable; and when in living dissections a man puts his finger into the heart of an animal, which probably has a fainter, or at least no stronger degree of heat than a human heart, he will feel in his fingers, accustomed to the air, a manifest degree of heat, if they be but in their usual temper.

2. I have elsewhere proved by experiments, that a cubick inch of air, for instance, has as strong a spring, as suffices to enable it to resist the weight of the whole atmosphere, as far as it is exposed thereunto; for else it would be more compressed than *de facto* it is. And, 3. I have also shewn, that a very little portion of air, though it will much sooner lose its spring by expansion than a greater, yet it will resist further compression as much as a greater. And, 4. I have also shewn, that in the pores of the parts of animals, whether fluid or consistent, as in their blood, galls, urines, hearts, livers, &c. there are included a multitude of aerial corpuscles, as may appear by the numerous bubbles afforded by such liquors, and the swelling or expansion of the consistent parts in our exhausted receiver.

5. To this we may add, that, besides the bones, whose solidity is not questioned, a much greater part of the human body, than is wont to be imagined, does really consist of membranes and fibres, and the coalitions and contextures of these; and that



that these substances are, by the providence of the most wise Author of things, made of a much closer and stronger texture, than those, that have not tried, will be apt to think; as I could make probable by the great force that bladders will endure, and the very great weight that tendons of no great thickness will lift up or sustain, and by other things, that I shall not now insist on. Lastly, there is a far greater difference than men are wont to suspect, between the effects of the pressures made upon bodies by incumbent, or otherwise applied solid weights, and those that they suffer from heavy, but every way ambient fluids; as will appear by the experiments to be mentioned by and by.

From the particulars contained in these considerations, we may be assisted to shew, why it is not necessary, that the pressure of the atmosphere, though as great as we suppose it, should oppress and crush the bodies of men, that live under it; for the solidity of the bones, and the strong texture of the membranes and fibres, and the spring of the aerial particles that abound in the softer, as well as in the fluid parts of bodies, is equivalent to the pressure of as much of the atmosphere as can exercise its pressure against them, and makes the frame of a human body so firm, that it may well resist the pressure of the outward air, without having any part violently dislocated, whilst the external pressure is exercised but by the air, which being but an invironing fluid, presses it equally (as to sense) on every side. And because our bodies have been produced in the atmosphere, and from our very birth exposed, without intermission, to the pressure of it; our continual accustomance to this pressure, and the firmness of their structure, keep us from being sensible of the weight or pressure. And that it was not impertinent for me to mention the firmness of the frame of our bodies on this occasion, I shall manifest by an instance, that will upon another account also be proper for this place.

We know, that multitudes of men have had occasion to pass over high mountains; and besides, that I have been myself upon the *Alps* and *Appennines*, I have enquired of travellers that have visited the Asian and American mountains, and some, that have been upon the top of the Peak of *Teneriffe* itself; but though divers of them took notice of a great difference in the air at the top and bottom, as to some other quality, as coldness and thinness; yet I never met with, nor heard of any that took notice of a difference, as to the weight of air he sustained, or that complained that when he was come down to the foot of the mountain, he felt any greater compression from the air, than at the top. And yet the experiments made, as well by others as by ourselves, sufficiently witness, that on more elevated parts of the earth, which have a less height of the atmosphere incumbent on them, the weight and pressure of the air is not so great as below. And on very high mountains, it is not unlikely that this difference may be very considerable, since, when the Torricellian experiment was made near *Clermont* in *France*, upon the *Puy de Domne* (which is none of the highest mountains in the world, being found, by the ingenious makers of that observation, to be but about 500 fathoms) they found the difference of the mercury, at the top and bottom, to amount to about three inches; and consequently, if the trial had been made with water instead of quicksilver, the difference would have been about three feet and a half in the perpendicular height of the water. And it is very probable, that in much higher mountains, the difference of the mercurial cylinders height, at the top and bottom, may be much greater; and at the bottom of some very deep well or mineral groove, which may, without improbability, be supposed to be placed at, or near the foot of one of these mountains, if we conceive the baricope to be let down, the variation of the height of the mercurial cylinder may be yet much more considerable; and yet we find not, that the diggers in the deepest mines, in mountainous countries, are sensible of being leaned on or compressed by any unusual weight. But not here to build on any thing but matter of fact, it appears by the newly-

named observation, that, when a man was at the bottom of the hill, he had as much greater weight of air leaning upon his head, than he had at the top, as was equal to the height of an imaginary vessel full of water, which having his head for basis, were three feet and a half high; which is so considerable a weight as could not but have been, not only sensible, but very troublesome and uneasy to support. And what has been said of the gravity of a pail of water, that leaned on his head, may be proportionably applied to his shoulders, arms, &c.

WHENCE I think I may infer, that the reason why such a weight was not felt by the man it compressed, was not, that the air that pressed him, was not considerable, but that the pressure was exercised after the uniform manner of fluid bodies.

AND this may suffice to shew, that there is no necessity that the compression of the atmosphere should make it impossible to live in it. But because it is observed, that those that dive to great depths under water, are not oppressed by the great weight of the incumbent water, and the cause of this strange phenomenon is not so easy to be assigned, and therefore has been made one of the two grand arguments whereon the non-gravitation of water in water, and air in air, has been, and still remains founded: I shall here offer something *ex abundanti* towards the solution of that noble and difficult problem.

AND, first, that what is observed by the divers does not evince that water does not weigh in water, I have elsewhere \* proved by such reasons and experiments, as had the good fortune to convince eminently learned men, that were sufficiently prepossessed with the vulgar opinion; and in the same treatise I have given a clear account, why a bucket full of water is not felt considerably heavy, whilst it is under water, in comparison of what it is whilst it is drawn up into the air; which is the other phenomenon, that I freshly intimated the common opinion to be founded on.

NEXT, I do not think it strange, that that follows not, which it is objected should follow from our hypothesis; namely, that a diver should be violently depressed to the bottom of the water, by the weight of so great a pillar of the sea as is placed perpendicularly over his body. For if we imagine a plane so to cut the sea-water, as to pass by the diver's body; then as that part of the plane on which his body leans will be pressed by it, together with the water that is perpendicularly incumbent on it; so all the other parts of the same plane will be pressed by equally tall pillars of water perpendicularly incumbent on them; and consequently, if the man's body were just equiponderant to an equal bulk of water, it and the water, that leans on it, would be sustained by the pressure of the collateral water incumbent on the other parts of the same plane (as may be easily understood by what I have elsewhere † said). And therefore there is no reason why the divers bodies should be more forcibly depressed than its depression is resisted. It is true, that this body will sink, but that is because it is not only, as we lately supposed it, æquiponderant to an equal bulk of water, but heavier than that. But then, since the water, by its gravity and resistance takes off as much of the weight of the diver's body, whilst that is immersed, as a quantity of water equal to it would weigh in the air, the subsiding of the human body by its own weight ought to be but slow, because that being not in specie much heavier than water, it can sink but by virtue of the surplusage of weight that it has above water. And, in effect, I have been informed by swimmers, that in the sea, whose water, by reason of the saltiness, is specifically heavier than the common water, they could hardly dive when they had a mind; the salt-water did so much support them. And having, because I had no conveniencies to make

\* See the Hydrostatical Paradoxes.

† See Appendixes to the Hydrostatical Paradoxes.



trials upon the parts of human bodies, examined the weight of parts of other animals in air and water, I found the overplus of the weight of the animal substances above an equal bulk of water to be but very small. And this may suffice to take off the wonder, why, though water may be admitted to gravitate in water, yet divers are not depressed by that which leans upon them; the endeavour they use to keep themselves from sinking, by striking the resisting water with their arms and legs, easily compensating their weak tendency downwards, which the small surplussage of gravity above-mentioned gives them.

BUT it seems to me far more difficult to render a reason, why those that are a hundred feet beneath the surface of the sea, are not crushed inwards, especially in their chests and abdomens, or at least so compressed as to endure a very great pain.

To clear up or lessen this difficulty, I have two things to offer.

I. I CONFESS, that I am not entirely satisfied about the matter of fact; for I do not yet know, whether it fares alike with the divers in all depths under water; for, according to the answers I obtained from persons, that had been, one of them at the coral-fishing in the *Streights*, and the other at the pearl-fishing near *Manar*, I do not find that the divers are wont to descend to the greatest depths of the sea, which if they did, perhaps they would find a notable difference.

AND in small, or but moderate depths, those that dive without engines usually make such haste, or are so confounded, or have their minds so intent upon their work, that they take not notice of such lesser alterations, as else they might observe, especially they being persons void of curiosity and skill to make such observations. Which I the rather mention, because having met with a learned physician, that living by the sea-side in a hot climate, delighted himself much in diving; and enquiring of him, whether he felt no compression, when he passed out of the air into the water, he answered me, that when he dived nimbly as others use to do, he took not notice of it, but when he let himself sink leisurely into the water, he was sensible of an unusual pressure against his thorax, which he several times observed.

A MAN that gets his living by fetching up goods out of wrecked ships, complained to me, that if with his diving-bell he went very deep into the sea, and made some stay there, he found himself much incommodated; which though he imputed to the coldness of the water, yet by the symptoms he related, I was inclined to suspect that the pressure of it upon the genus nervosum might have an interest in the troublesome effect. And I have been assured by an eminent virtuoso of my acquaintance, that he was lately informed by a person, whose profession it is to fetch up things from the bottom of the sea by the help of a diving-bell, that several times, when he descended to a great depth under the surface of the water, he was so compressed by it, that the blood was squeezed out at his nose and eyes; which relation seems to favour our conjecture, and would much more confirm it, if I were sure that the effect was no way caused by some fermentation or other commotion in the blood itself, occasioned by the great density, or other alterations of the air he breathed in and out, or by some other operation of the ambient medium distinguishable from the compression of the water, though perhaps conjoined with it.

AND on this occasion I remember, that questioning an engineer, who had made use of an engine to go under water, quite differing from the diving-bell; he answered me, that when he came to a considerable depth; he found the pressure so great against the leathern case, wherein he descended, and by that means against his belly and thorax, that he feared it would have spoiled him, which forced him to make haste up again. But this observation, to have much built upon it, should be further enquired into.

THESE

THESE things, and not these only, make me wish that what is felt by those that dive to great depths, and stay at them, might be more heedfully observed by intelligent men, that, being fully informed, what is true in point of fact, we may the better and more cheerfully indagate the reasons.

IN the mean while, taking things as they are thought to appear, I shall propose two things towards the solution of our difficulty; namely, the firmness of the structure of a human body, and the uniformity of the pressure made by fluids.

OF the first of these I shall add but little to what has been already said, where I spoke of the resistance made by our bodies to the compression of the atmosphere; only shall here take notice, that whereas the membranes are very thin parts, and therefore seem unfit to make any great resistance; we have tried, that if a piece of fine bladder were fastened to the orifice of a brass-pipe, of about an inch in diameter, we could not, by drawing the air from beneath it, make the weight of the atmosphere break the bladder, though the weight were perhaps equivalent to an erected cylinder of water, of the wideness of the orifice, and about thirty feet high, and were indeed such, that divers men that laid their hands on the orifice, when the air was pumped out from beneath, complained, that they were not able to lift off their hands again, till some of the air was re-admitted.

BUT the main thing I shall propose; towards the solving of the difficulty we are considering, is the uniformity wherewith fluid bodies press upon the solid ones that are placed in them. And because I remember not to have met with experiments purposely made to shew, how this sort of pressure is more easy to be resisted than that of solids against solids, I shall subjoin the following trials.

## EXPERIMENT I.

IN the short cylinder of brass above-mentioned, we put a fine bladder tied so close at the neck, that none of the air (whereof it was about half full) could probably get out. Which we did, to the end that the hen-egg, we were to bed in it, might lie soft, and have its sides almost covered with the limber and flaccid bladder and contained air; this done, we covered the remaining part of the egg with another bladder, that nothing that was hard might come to bear immediately upon the shell; then we put the wooden plug into the cylinder, and a weight upon the plug, which is to be done very slowly and warily, lest the quick descent of the weight should make the plug break the egg it leans on. Lastly, the cylinder thus fitted, being covered with a large receiver, and the air being drawn out, that air, which was tied up in the bladders, by degrees expanded itself so strongly, as to lift up the plug and the incumbent weight to a pretty height, and keep it there, till the external air was re-admitted.

Now since it will be readily granted, and appears by divers experiments elsewhere related, that the air in such cases expands itself vigorously every way, it appears by the recited trial, that it pressed against the egg with the same force that it pressed proportionably against the bottom of the plug, and that force was more than sufficient to lift up the weight, which (together with the plug) amounted to about thirty pounds, and yet the egg being taken out, appeared perfectly whole and no way harmed; whereas, upon the same egg, if I mistake not, or at least another of the same kind, laying warily a while after small weights, one upon another, the egg was crushed to pieces by about four pounds weight. This experiment, though it seemed considerable to those that saw it, and may prevent an objection, for which reason I here mention it; yet will appear in no



way strange to them, that consider that the weight of the atmosphere which the egg supported before it was put into the cylinder, was more than equivalent to such a pressure of the air, as may suffice to lift up the plug; wherefore I thought fit to make further trials of a differing nature.

## EXPERIMENT II.

WE took a glass-bubble of about an inch and half in diameter, which we caused to be blown at the flame of a lamp, that it might be far more thin and easy to break than the thinnest phials that are wont to be blown in the glasser's furnaces. This bubble we included between bladders, as we did the egg in the former experiment; and then having warily put the plug into the cylinder, so as it might press upon the bladder that invironed the glass, we leisurely put the weights upon the plug, till they, together with the plug, amounted to thirty pounds or more, which being removed, the plug was taken out, and the glass-bubble, though it were extraordinarily thin, perhaps no thinner than fine white paper, was taken out whole.

## EXPERIMENT III.

BUT lest the great resistance of so thin a glass, which yet was not hermetically sealed, should be ascribed to the sphericness of its figure, we employed, instead of it, the shell of an egg, whence by a hole, made at one end of it, the yolk and white had been taken out. This empty and imperfectly closed shell we handled, as we did the glass-bubble in the former experiment; and, notwithstanding the great leaden weight, that leaned by the intervention of the plug upon the soft body that environed it, it was taken out, not only uncrushed together, but, for aught we could perceive, without the least crack.

## EXPERIMENT IV.

AND to shew, that what we observed about the nature of the compression of fluid bodies will hold as well in water as air, though it seemed difficult to make the trial with the accommodations we then had, we thought upon the following expedient.

INTO a limber bladder, almost full of water, we put a hen egg, and tying the neck very strait, that nothing might get in or out, we so placed the bladder in the brass cylinder, that the egg might not be immediately touched by any thing that was hard; then putting the plug into the cylinder, we warily and leisurely heaped upon it flat-bottomed weights of lead conveniently shaped, till they amounted (if both I and another misremember not) to about seventy-five pounds; notwithstanding all which, the egg was taken out sound and uncracked; and probably might have supported a much greater pressure, if we had been furnished with more weights of a commodious figure to heap upon it.

If we compare with this what was noted at the close of the first experiment, about the breaking of an egg with four pounds weight, when no fluid body was interposed, it will be obvious to conclude how great a difference there is between the resistance that a body may make to the pressure of solid bodies, that bear hard against some parts, and not against others; and its resistance to others that compress it uniformly, or in all

places alike. For though it be denied, and that, I think, upon very insufficient grounds, that bodies under water are pressed by the incumbent water, because, as it is pretended, the elements gravitate not in their proper place; yet this objection cannot be pretended to take place in our last experiment, where the main thing that leaned upon the water which surrounded the egg, being not a pillar of homogeneous water, but a great and solid weight of lead, the included egg must, by the intervention of the water, have been compressed. Nor were eggs the only bodies we endeavoured to crush after this manner, the trial having been also made upon a substance more soft, and of a very irregular shape.

To apply this now to divers, when they are at a moderate depth under water; it seems not improbable that the structure of their bodies should be robust enough, not to be violated by the pressure of the incumbent, and otherwise ambient water. For we have seen by the former experiment, and especially by the last recited, that a body, easy to be broken inwards by an incumbent solid weight, will remain entire and unaltered, in point of figure, under a very much greater weight, that compresses it after the manner of an ambient fluid. And though it would seem to many, that even in our supposition, the thorax being, as they think it, a kind of empty space in the body, the ribs and muscles ought, by the weight of the water, to be crushed into the great cavity intercepted between them; yet it is to be considered on the other side, that the air contained in the chest, especially when its spring is increased by those accidental causes that may take place, when men are deep under water, particularly the preternatural heat, which the want of the usual respiration is apt to produce, will very much help the chest to resist the pressure, as they will easily grant, that have tried the resistance that air makes, to be considerably compressed under water, the difficulty of farther compressing it still encreasing, as in springs it ought to do, the more it is compressed. And I further observe, that the structure of the thorax is much more firm than men are wont to suppose; as appears by the very great solid weights that some men do, for gain, or to shew their strength, suffer to be laid on their breasts, without receiving any mischief thereby. And if I should admit, that at great depths the water had some little compressive operation upon the chest; yet that can be no other than the pressing the parts a little inwards, and that the structure of the thorax itself, fitted by nature for constriction and dilatation (as may appear in vehement takings in and blowings out of the air) may admit with small inconvenience. To which purpose I recal to mind, what I lately mentioned concerning the physician that found his thorax somewhat compressed when he leisurely dived; as also what I have \* elsewhere delivered concerning a tad-pole, which swimming in water, that was strongly compressed by an external force, seemed, thorough the glass that contained the water, to be somewhat lessened in bulk, and yet not killed, nor sensibly crushed, notwithstanding its great tenderness. And if there were parts of a human body that were of a texture too weak, and too disproportionate to the rest, I think it possible, that this compression inwards might be great enough to be very sensible to the divers. For having purposely enquired of a certain man, whose trade it was to fetch up goods out of ships cast away, by the help of a diving instrument, he told me, that when he was at a considerable depth under water, as about ten or twelve fathoms, he found, suitably to my conjecture, so great a pressure against the drums or thin membranes of his ears, which were not sufficiently counterpressed from within, as put him to a great deal of pain, till he had found some contrivances to lessen the inconvenience. Nor was this man the only diver that has complained of this troublesome pressure, which seems to

\* In the Appendix to Hydrost. Paradox.



argue, that, at least at great depths under water, the firmness of the structure of a man's body does concur with the uniformity of the fluid's pressure, to keep him from being hurt by the incumbent, and otherwise ambient air.

BUT I shall now say no more of the problem about divers, since (besides that the matter of fact is not yet, in my opinion, accurately enough stated and determined) the true solution of it is not necessary to give a reason, why the weight of the air, a fluid so much lighter than water, should not oppress nor crush the bodies of animals; though what has been already said, about the resistance of bodies under water, may serve very much to confirm the reasons I proposed, why we that live in the atmosphere are not sensibly compressed, much less oppressed by its weight.

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## SOME OBSERVATIONS

ABOUT

## SHINING FLESH,

Both of VEAL and of PULLET,

AND THAT

Without any sensible PUTREFACTION in those Bodies.

First published in the *Philosophical Transactions*, N<sup>o</sup> 89, p. 5108,  
for December 16, 1672.

**Y**ESTERDAY, when I was about to go to bed, an amanuensis of mine, accustomed to make observations, informed me, that one of the servants of the house, going upon some occasion into the larder, was frightened by something of luminous, that she saw (notwithstanding the darkness of the place) where the meat had been hung up before. Whereupon, suspending for a while my going to rest, I presently sent for the meat into my chamber, and caused it to be placed in a corner of the room capable of being made considerably dark, and then I plainly saw, both with wonder and delight, that the joint of meat did, in divers places, shine like rotten wood or stinking fish; which was so uncommon a sight, that I had presently thoughts of inviting you to be a sharer in the pleasure of it. But the late hour of the night did not only make me fear

to give you too unseasonable a trouble, but being joined with a great cold I had got that day by making trial of a new telescope, you saw, in a windy place, I durst not sit up long enough to make all the trials that I thought of, and judged the occasion worthy. But yet, because I effectually resolved to employ the little time I had to spare, in making such observations and trials as the accommodations I could procure at so inconvenient an hour would enable me, I shall here give you a brief account of the chief circumstances and phenomena that I had opportunity to take notice of.

1. THEN I must tell you, that the subject we discourse of was a neck of veal, which, as I learned by enquiry, had been bought of a country-butcher on the Tuesday preceding.

2. IN this one piece of meat I reckoned distinctly above twenty several places, that did all of them shine, though not all of them alike, some of them doing it but very faintly.

3. THE bigness of these lucid parts was differing enough, some of them being as big as the nail of a man's middle finger, some few bigger, and most of them less. Nor were their figures at all more uniform, some being inclined to a round, others almost oval, but the greatest part of them very irregularly shaped.

4. THE parts that shone most, which it was not so easy to determine in the dark, were some grisly or soft parts of the bones, where the butcher's cleaver had passed; but these were not the only parts that were luminous; for by drawing to and fro the medulla spinalis, we found that a part of that also did not shine ill; and I perceived one place in a tendon to afford some light; and lastly, three or four spots in the fleshy parts, at a good distance from the bones, were plainly discovered by their own light, though that were fainter than in the parts above-mentioned.

5. WHEN all these lucid parts were surveyed together, they made a very splendid shew; but it was not so easy, because of the moistness and grossness of the lump of matter, to examine the degree of their luminousness, as it is to estimate that of glow-worms, which being small and dry bodies, may be conveniently laid in a book, and made to move from one letter or word to another. But by good fortune, having by me the curious transactions of this month, I was able so to apply that flexible paper to some of the more resplendent spots, that I could plainly read divers consecutive letters of the title.

6. THE colour that accompanied the light was not in all the same, but in those which shone liveliest, it seemed to have such a fine greenish blue, as I have divers times observed in the tails of glow-worms.

7. BUT notwithstanding the vividness of this light, I could not, by the touch, discern the least degree of heat in the parts whence it proceeded; and having put some marks on one or two of the more shining places, that I might know them again, when brought to the light, I applied a sealed weather-glass, furnished with tinted spirit of wine, for a pretty while, and could not satisfy myself, that the shining parts did at all sensibly warm the liquor; but the thermoscope, though good in its kind, being not fitted for such nice experiments, I did not build much upon that trial.

8. NOTWITHSTANDING the great number of lucid parts in this neck of veal, yet neither I, nor any of those that were about me, could perceive, by the smell, the least degree of stink, whence to infer any putrefaction; the meat being judged very fresh and well-conditioned, and fit to be dressed.

9. THE floor of the larder, where this meat was kept, is almost a story lower than the level of the street, and it is divided from the kitchen but by a partition of boards, and



is furnished but with one window, which is not great, and looks towards the street, which lies northward from it.

10. THE wind, as far as we could observe it, was then at south-west, and blustering enough. The air by the sealed thermoscope appeared hot for the season. The moon was passed its last quarter. The mercury in the barometer stood at  $29\frac{1}{10}$  inches.

11. WE cut off with a knife one of the luminous parts, which proved to be a tender bone, and being of about the thickness of a half crown piece, appeared to shine on both sides, though not equally; and that part of the bone, whence this had been cut off, continued joined to the rest of the neck of veal, and was seen to shine, but nothing near so vividly as the part we had taken off did before.

12. To try, whether I could obtain any juice or moist substance from this, as I have several-times done from the tails of glow-worms; I rubbed some of the softer and more lucid parts (which I caused to be purposely cut off) as dexterously as I could, upon my hand, but I did not at all perceive any luminous moisture was thereby imparted; though the flesh seemed, by that operation, to have lost some of its light.

13. I CAUSED also a piece of shining flesh to be compressed betwixt two pieces of glass, to try how well the contexture of it would resist that external force; but I did not find the light to be thereby extinguished during the short time I could allot to the experiment.

14. BUT supposing, that high rectified spirit of wine might so alter the contexture of the body it permeated, as to destroy its faculty of shining, I put a luminous piece of veal into a crystalline phial, and pouring on it a little pure spirit of wine, that would have burned all away, after I had shaken them together, I laid by the glass, and in about a quarter of an hour, or less, I found that the light was vanished.

15. BUT water would not so easily quench our seeming fires; for having put one of them into a China cup, and almost filled it with cold water, the light did not only appear, perhaps undiminished, through that liquor, but above an hour after was vigorous enough not to be eclipsed, by being looked upon at no great distance from a burning candle, that was none of the smallest; and probably the light would have been seen much longer if we could have afforded to watch out its duration.

16. WHILST these things were doing, I caused the pneumatical engine to be prepared in a room without fire (that the experiment might be tried in a greater degree of darkness); and having conveyed one of the largest luminous pieces into a small receiver, we caused the candles to be put out, and the pump to be plied in the dark; but the diminution of light, after the pump seemed to have been employed for a competent while, appeared so inconsiderable (whether because our eyes had leisure to be fitted to that dark place, or for what other cause soever) that I began to suspect that the instrument having been managed in the dark, had leaked all the while. Wherefore causing the lights to be brought in, and a mercurial gage to be put into the receiver, when we were sure that this glass was well cemented on to the engine, the candles being removed, the pump was set a-work again; and then opening my eyes, which I had kept closed against the light of the candles, I could perceive, upon the gradual withdrawing of the air, a discernible and gradual lessening of the light, which yet was never brought quite to disappear (as I long since told you, the light of rotten wood and glow-worms had done) or to be so near vanishing as one would have expected; though, upon the bringing in of the candles again, it appeared by the gage, that the pump had been diligently applied. But the room being once again darkened; by the hasty increase of light that had disclosed itself in the veal, upon this letting in of the air to the exhausted receiver, it appeared more manifestly than before, that the decrement, though but slowly made,

made, had been considerable. This trial we once more repeated with a not unlike success; which, though it convinced us, that the luminous matter of our included body was more vigorous, or tenacious than that of most other shining bodies, yet it left us some doubts that the light would have been much more impaired, if not quite made to vanish, if the subject of it could have been kept long enough in our exhausted receiver; but the unseasonable time of the night reducing me at length to go to bed, I could not stay to prosecute this, or any other trial.

17. ONLY, whilst I was undressing, this further observation occurred, that supposing there might be, in the same larder, more joints of the same veal than one, ennobled with this shining faculty, it was found, that a leg of veal, which was caused to be brought into my chamber, had some shining places in it; though they were but very few, and faint, in comparison of those that were conspicuous in the above mentioned neck.

18. WHAT further phaenomena this morning might have afforded me, I cannot tell, having been hastily called up, before day, for a niece, that I am very justly, and exceedingly concerned for; who was thought to be upon the point of death, and whose almost gasping condition had too much affected and employed me, to leave me any time for philosophical entertainments, that require a calm, if not a pleased mind. Only this I took notice of, because the observation could not cost me a minute of an hour, that whilst they were bringing me candles for to rise by, I looked upon a clean phial, that I had laid upon the bed by me, after a piece of our luminous veal had been included in it, and found it to shine vividly at that time, which was between four and five of the clock this morning; since when I have made no one observation or trial.

#### P O S T S C R I P T .

19. NEAR two days after I had made the fore-mentioned observations, those horrid symptoms of my niece's disease, that had so much alarmed the physicians, and me, being, through God's goodness, considerably abated, I began to resume the thoughts of our shining veal; and though having, in the hurry I was in, forgotten to take any order about it, I found it was already disposed of; yet the piece I lately mentioned to have been included in a phial, being preserved in it, I looked upon it the third day (inclusively) after we had first observed the meat it was cut off from to be luminous; and I found it to shine in the dark as vigorously as ever. The fourth day its light was also conspicuous; so that I was able, in a dark corner of the room, to shew it, even in the day-time, to three or four very ingenious physicians, all of them, save one, members of the Royal Society; and, I presume, I need not remind you, that the following night, I invited you to be a spectator of it, though before that time the light had begun to decay, and the offensive smell to grow somewhat strong: which seems to argue, that the disposition, upon whose account our veal was luminous, may very well consist both with its being, and not being in a state of putrefaction, and consequently is not likely to be derived merely from the one, or the other. The fifth day, in the morning, looking upon it when I awaked, and before the curtains were opened, it seemed to shine better, than it had done the day preceding. The same night also, it was manifest enough, though not vivid, in the dark. When I awaked the sixth day in the morning, after the sun was risen, I could, within the curtains, perceive a glimmering light. But the seventh day, which was yesterday, I could not, late at night, discern any light at all.

You



You saw too much in what a condition I was, when you did me the favour to visit me, to expect that I should presume to entertain you with any speculations about the cause of these unusual apparitions of light. It is true indeed, that in some notes I formerly mentioned to you, I endeavoured to make it probable, that whether light depend upon a particular kind of impulse, propagated through a transparent medium, or upon a diffusion of extremely little parts from the luminous body, or upon the action of some other corporeal agent; whatever the efficient be, the effect is produced in a mechanical way. But though I had these papers by me, yet, to determine what peculiar kind of motions or other operations nature really employed in the production of a light, which seemed not clearly, by what I shall presently note, referable either to the particular and settled constitution of the animals, whose flesh shined (as in our glow-worms, and some American flies) or to that intestine and unusual motion of the parts that causes or accompanies putrefaction in rotten wood, or fishes; since, upon the first and liveliest appearance of the light, there was not any (at least that could be taken notice of by the senses); to determine this, I say, it seemed to me so difficult a task, that I shall willingly leave the solution of such abstruse phenomena, as some of ours, unattempted; especially since I may, God permitting, make an historical mention of them the day after to-morrow, at the meeting of the Royal Society; where, I doubt not, much more, and more to the purpose, will be said, and considered, than I have vanity to think myself capable of offering. Only, for the prevention of some needless conjectures, to which, without this previous advertisement, one might upon plausible grounds indulge, I shall, in the mean while add, and conclude with one observation more, which may possibly take off our thoughts from striving to deduce the shining of our veal from the peculiar nourishment, or constitution, or properties, of that individual calf, whose flesh, &c. was luminous. For, having several nights sent purposely into the larder, to observe, whether any veal, since brought thither, or any other meat, did afford any light, a negative answer was always brought me back; save at one time, which happened to be within less than forty-eight hours of that, at which the luminousness of the veal had been first taken notice of; for at this time there was, in the same larder, a conspicuous light seen in a pullet that hung up there, which having caused to be brought up into a darkened place in my chamber, in the night-time, I perceived four or five luminous places; which were not indeed near so large as those of the veal, but were little less vivid than they. All of these I took notice to be either upon, or near the rump; and that which appeared most like a spark of fire, shone at the very tip of that part. Yet was not this fowl mortified, nor at all ill scented, but so fresh, that the next day I found it very good meat. But whether this may reasonably lead to a suspicion, that the peculiar constitution of the air in that larder, and at that time, may as well deserve to be taken into consideration, as the peculiar nature of the animals, whose flesh did shine, is a question, that I, who have scarce time to name it, must not presume to do any more than name. And therefore, as soon as I have begged your pardon for this tedious though hasty scribble, I shall, without ceremony, subscribe myself, &c.

A

## NEW EXPERIMENT

CONCERNING

An Effect of the varying Weight of the ATMOSPHERE upon some Bodies in the Water; suggesting a Conjecture, that the very Alterations of the Air, in point of Weight, may have considerable Operations, even upon Men's Sickness or Health.

First published in the *Philosophical Transactions*, N<sup>o</sup> 91. p. 5156,  
for February 24, 1672-3.

**T**HOUGH many things have, by ingenious men, been already observed, as to the power and operations of the atmosphere's weight upon liquors that are exposed to it in Torricellian tubes, or other vessels, closed at one end, and near the top, either empty or unfilled with any visible body; yet men seem not to have much enquired what effects the very variation of this weight of the atmosphere may have on the liquors which it presses, in other vessels than such as baroscopes and pumps. And yet when I remember, how much of air appears by our engine to be invisibly harboured in the pores, not only of water, but of the blood, serum, urine, gall, and other juices of the human body; and that (as I have elsewhere experimentally shewn) the pressure of the atmosphere, and the spring of the air, work upon liquors, and on bodies immersed in those liquors, as well as upon solid ones, immediately exposed to the air, I am prone to suspect that the very alterations of the atmosphere, in point of weight, may, in some cases, have some not contemptible operations, even upon men's sickness or health; as when the ambient air, for instance, grows suddenly very much lighter than it was before, or than it was wont to be, the spirituous and aerial particles that are plentifully harboured in the mass of blood, will naturally swell that liquor, and so may distend the greater vessels, and not a little after the celerity and manner of the circulation of the blood by the capillary arteries and veins. By which alteration, that divers changes may happen in the body, will not seem improbable to those that know in general, how important a thing the manner of  
the



the circulation of the blood may be there, though, as to its particular effects, I leave them to the speculation of physicians; and shall only add, that to keep this conjecture of mine (for I propose it as no other) from seeming as groundless as extravagant, I will annex an experiment, that you will not perhaps dislike, just as I find it registered among some of my loose papers.

I CAUSED to be blown, at the flame of a lamp, three small round glass bubbles, about the bigness of hazel-nuts, and furnished each of them with a short and slender stem, by whose means they were so nicely poised in water, that a very small change of weight would make them either emerge, if they but lightly leaned on the bottom of the vessel, or sink, if they floated on the top of the water.

THIS being done at a time, when the atmosphere was of a convenient weight (and such a season is not ordinarily difficult to be chosen within some reasonable time, to him that wants neither attention nor a good baroscope) I put them in a wide-mouthed glass, furnished with common water, and leaving them in a quiet place, where yet they were frequently in my eye, and were suffered to continue many weeks, or some months, I observed, as I expected, that sometimes they would be at the top of the water, and remain there for divers days, or perhaps weeks; and sometimes would fall to the bottom, and after having continued there for some time, longer or shorter, they would again emerge. And though sometimes, especially if I removed the vessel that contained them to a southern window, they would rise to the top, or fall to the bottom of the water, according as the air was hot or cold; yet it was not difficult to distinguish these motions from those produced by the varying gravity of the atmosphere. For when the beams of the sun, or heat of the ambient air, by rarefying the air included in the bubbles, made that air drive out some of the water, and consequently made the whole bubble, consisting of glass, air, and water, somewhat lighter than a bulk of water equal to it, though the bubble did necessarily swim as long as the included air was thus rarefied, yet when the absence of the sun, or any other cause, made the air lose its adventitious warmth, there would ensue a condensation of the air again, and thereupon an intrusion of more water (to succeed the air) into the glass, and consequently a sinking of the bubble; and this would commonly happen at night, if it did not happen sooner. But when it was upon the account of the varying weight of the atmosphere that the bubbles either rose or fell, it appeared by the baroscope, that the atmosphere was so heavy, or so light, that they ought to do so. Inasmuch, that I divers times predicted, whether I should find the mercury in the baroscope high or low, by observing the situation and posture of the bubbles; and consulting that instrument, it verified my conjectures. And though, whilst the atmosphere was not too considerably either light or heavy, the changes of the air, as to heat or cold, would, as I was saying, place the bubbles sometimes at the top, and sometimes at the bottom of the water, within the compass of a day; yet, if the atmosphere were either very heavy, or very light, the bubbles would continue at the bottom, or at the top of the water for many days together, in case the atmosphere did not in all that time change its gravity. And I remember, that I did, for curiosity's sake, when the quicksilver was high in the baroscope, put the glass two or three days in a south window about noon, and for a good while after, and that in sun-shining weather; and yet even then the bubbles did not emerge, though it appeared by a good sealed weather-glass, which I kept in the same window, that the ambient air was much warmer than at other times, when I had observed the bubbles to keep at the top of the water.

N.B. 1. It being very difficult to poise several bubbles precisely, as well one as another, I thought it not strange, that all the three bubbles did not constantly (though for

the most part they did) rise and fall together; but sometimes two of them, and now and then, though seldom, one alone would sink or emerge, when the change of the weight of the atmosphere was not considerable enough to operate sensibly upon the rest; and of such instances, I have had opportunity to observe one or two within these last three days; and therefore it is not amiss to poise a greater number of bubbles together, that after trial made of all, the fittest may be chosen. Which advertisement will appear the more proper, because of what is to be added in the following note.

2. I HAVE observed it sometimes to happen, that a bubble that floated when it was first poised, would, after a while, subside without any manifest cause; or if it were made to sink by such a cause, it would continue at the bottom of the water, though that cause were removed; which difficult phenomenon seeming to depend upon a kind of imbibition made of certain particles of an ærial nature, by the water, the consideration of it belongs to another place, not to this; where it may suffice, that the experiment did sometimes actually answer expectation, as that above-related did; wherein my main drift is to shew, that since, as the atmosphere was heavier or lighter, it is capable to work upon bodies under water, so as to procure their sinking, or their emersion; the air, though a fluid a thousand times lighter, must lean or press upon the water itself, by whole intervention it produces these effects; which confirms what I elsewhere teach, that the atmosphere is incumbent, as a heavy body, upon the terraqueous globe.

3. BESIDES the other circumstances, upon whose account this experiment may fail of success, the season of the year, wherein it is tried, may, for aught I know, be considerable. For which reason I shall here add this advertisement, that I choose, but do not confine myself, to make my trials about the beginning of the spring, as a time wherein notable alterations of the air, as well to weight as to other things, are the likeliest to be frequent.





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E S S A Y S

OF THE

STRANGE SUBTILTY,  
GREAT EFFICACY,  
DETERMINATE NATURE

OF

E F F L U V I U M S.

To which are annexed

New Experiments to make FIRE and FLAME ponderable :

TOGETHER WITH

A Discovery of the PERVIOUSNESS of GLASS.

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AN ADVERTISEMENT to the READER.

IT is hoped the reader will not think it strange, not to meet with, in the following papers, a more close and uniform contexture of the passages that make them up, if he be seasonably informed of the rise and occasion of penning them, which was this. The author having many years ago written an essay about an experiment he made of nitre, by whose phenomena he endeavoured to exemplify some parts of the corpuscular philosophy, especially the production of qualities ; he afterwards threw together divers occurring thoughts and experiments, which he supposed might be employed by way of notes, to prove or illustrate those doctrines, and especially those that concerned the qualities of bodies ; and among these, observing those that are called occult, to be subjects

uncultivated enough (at least, in the way that seemed to him proper) he proposed to handle them more largely than most of the rest; and in order to that design he judged it almost necessary to premise some considerations and experimental collections about the nature and power of effluvia, about the pores of bodies and figures of corpuscles, and about the efficacy of such local motions as are wont either to be judged very faint, or to be passed by unheeded. For he had often looked upon these three doctrines of effluvia, of pores and figures, and of unheeded motions, as the three principal keys to the philosophy of occult qualities. But having hereupon made such collections, as upon review appeared too large to pass for notes on so short a text, he was induced to draw them \* into the form (they now appear in) of essays; but he would not put himself to the trouble of doing it, with care to keep them from retaining much of their first want of exact method and connexion. Nor was the author solicitous to finish them up, in regard that his other studies and occasions made him perceive, that in what he had designed about occult qualities, he had cut himself out more work, than probably he should, during many years, have opportunity to set upon in earnest, and complete. And in this condition these papers lay for divers years (as is well known to several that saw them, or even transcribed some of them) and might have continued to do so, if the author had not been induced to let them come abroad, partly by considering, that though the subjects (however he handled them) were as well important as curious, yet he did not find himself prevented by others in what he had to publish about them; and partly by the references he had made to them in some other papers, that he had promised his friends, wherein several things here delivered are vouched, and others supposed. And because the notes concerning the porosity of greater bodies, and the figurations of minute particles, together with the paper about unregarded motions, having been long laid aside among other neglected papers, were some of them missing, and others so misused, that they could not easily be made ready to accompany those that now come abroad; the author, that he might keep this book from having its dimensions too disproportionate, was content to add to the thickness of it, by subjoining one of those little tracts, that lay by him, concerning flame, because of the affinity betwixt the preceding doctrine about effluvia in general, and experiments that shew, in particular, the subtilty and efficacy of those of fire and flame. And though to that tract itself there belong another, designed to examine, whether the matter of what we call the sun-beams, may be brought to be ponderable; yet supposing this hitherto cold and wet summer to be like to be as unfriendly to the trials to be made with burning-glasses, as of late years some other summers have proved, he was easily prevailed with, not to make those experiments that were ready, wait any longer for those that probably will not in a short time be so; especially since those that now come abroad, have no dependency upon the others.

\* And some that were published anno 1669, under the title of the Atmospheres of Consistent Bodies.



OF THE

## STRANGE SUBTILTY

OF

## EFFLUVIUMS.

## C H A P. I.

**W**HETHER we suppose, with the ancient and modern atomists, that all sensible bodies are made up of corpuscles, not only insensible, but indivisible; or whether we think with the Cartesians, and (as many of that party teach us) with *Aristotle*; that matter, like quantity, is indefinitely, if not infinitely divisible; it will be consonant enough to either doctrine, that the effluvia of bodies may consist of particles extremely small. For if we embrace the opinion of *Aristotle*, or *Des Cartes*, there is no stop to be put to the subdivision of matter into fragments still lesser and lesser. And though the Epicurean hypothesis admit not of such an interminate division of matter, but will have it stop at certain solid corpuscles, which, for their not being further divisible, are called atoms, *ἄτομοι*; yet the assertors of these do justly think themselves injured, when they are charged with taking the motes, or small dust, that fly up and down in the sun-beams, for their atoms; since, according to these philosophers, one of those little grains of dust, that is visible only when it plays in the sun-beams, may be composed of a multitude of atoms, and exceed many thousands of them in bulk. This the learned *Gassendus*, in his notes on *Diogenes Laertius*, makes probable by the instance of a small mite, which, though scarce distinctly discernible by the naked eye, unless when it is in motion, does yet, in a good microscope, appear to be a complete animal, furnished with all necessary parts; which I can easily allow, having often in cheese-mites very distinctly seen the hair growing upon their legs. And to the former instance I might add, what I have elsewhere told you of, a sort of animals far lesser than cheese-mites themselves, namely those that may be oftentimes seen in vinegar. But what has been already said may suffice for my present purpose, which is only to shew that the wonderful minuteness I shall hereafter ascribe to effluvia, is not inconsistent with the most received theories of naturalists. For otherwise, in this essay, the proofs I mean to employ, must be taken, not *à priori*, but *à posteriori*. And the experiments and observations I shall employ on this occasion, will be chiefly those that are referrible to one of the following heads.

1. THE strange extensibility of some bodies, whilst their parts yet remain tangible.
2. THE multitude of visible corpuscles that may be afforded by a small portion of matter.
3. THE smallness of the pores, at which the effluvia of some bodies will get in.

4. THE

4. THE small decrement of bulk, or weight, that a body may suffer by parting with great store of effluvia.

5. THE great quantity of space that may be filled, as to sense, by a small quantity of matter, when rarefied or dispersed.

BUT though to these distinct heads I shall design distinct chapters, yet you must not expect to find the instances solicitously marshalled, but set down in the order they occurred to me; such a liberty being allowable in a paper, where I pretend not to write treatises, but \* notes.

## C H A P. II.

AMONG many things that are gross enough to be the objects of our touch, and to be managed with our hands, there are some that may help us to conceive a wonderful minuteness in the small parts they consist of.

I do not remember what *Cardan*, and since him, another writer, have delivered about the thinness and slenderness to which gold may be brought. And therefore, without positively assenting to, or absolutely rejecting what may have been said about it by others, I shall only borrow, on this occasion, what I have mentioned on † another, upon my own observation; namely, that silver, whose ductility and tractility are very much inferior to those of gold, was, by my procuring, drawn out to so slender a wire, that, when we measured it, which was somewhat troublesome to do, with a long and accurate measure, we found, that eight yards of it did not yet fully counterpoise one grain; so that we might add a grain more without making the scale, wherein it was put, manifestly preponderate, notwithstanding the tenderness of the balance. Whence we concluded, that a single grain of this wire amounted to twenty-seven feet, that is, three hundred and twenty-four inches. And since experience informs us, that half an English inch can, by diagonal lines, be divided into one hundred parts, great enough to be easily distinguished, even for mechanical uses, it follows, that a grain of this wire-drawn silver may be divided into sixty-four thousand eight hundred parts, and yet each of these will be a true metalline, though but slender and short cylinder, which we may very well conceive to consist yet of a multitude of minuter parts. For though I could procure no gilt wire near so slender as our newly-mentioned silver wire; yet I tried, that some, which I had by me, was small enough to make one grain of it fourteen feet long; at which rate an ounce did amount to a full mile, consisting of one thousand geometrical paces of five feet a piece, and seven hundred and twenty feet over and above. And if now it be permitted to suppose the wire to have been, as in probability it might have been, further drawn out to the same slenderness with the above-mentioned silver wire, the instance will still be far more considerable; for in this case, each of those little cylinders, of which sixty-four thousand eight hundred go to the making of one grain, will have a superficial area, which, except at the basis, will be covered with a case of gold; which is not only separable from it by a mental operation, but perhaps also by a chymical one. For I remember, that from very slender gilt wire, though I could get none so slender as this of mere silver, I did, more than once, for curiosity's sake, so get out the silver, that the golden films, whilst they were in a liquor that plumped them up, seemed to be solid wires of gold; but when the liquor was withdrawn, they appeared, as indeed they were,

\* This Essay was designed to be but a part of the author's notes upon his Essay about Saltpetre.

† In a paper about Improbable Truths.



to be oblong, and extremely thin and double membranes of that metal, which, with an instrument that had been delicate enough, might have been ripped open, and displayed, and been made capable of further divisions and sub-divisions. To this I shall add, that each of the little silver cylinders I lately spoke of, must not only have its little area, but its solidity; and yet I saw no reason to doubt, but that it might be very possible, if the artificer had been so skilful and willing, as I wished, to have drawn the same quantity of metal to a much greater length, since even an animal substance is capable of being brought to a slenderness much surpassing that of our wire, supposing the truth of an observation of very credible persons, critical enough in making experiments, which, for a confirmation, and an improvement of our present argument, I shall now subjoin. An ingenious gentlewoman of my acquaintance, wife to a learned physician, taking much pleasure to keep silk-worms, had once the curiosity to draw out one of the oval cases (which the silk-worm spins, not, as it is commonly thought, out of its belly, but out of the mouth, whence I have taken pleasure to draw it out with my fingers) into all the filken wire it was made up of, which, to the great wonder, as well of her husband, as herself, who both informed me of it, appeared to be, by measure, a great deal above three hundred yards, and yet weighed but two grains and a half; so that each cylindrically shaped grain of silk may well be reckoned to be at least one hundred and twenty yards long.

ANOTHER way, I remember, I also employed to help men, by the extensibility of gold, the better to conceive the minuteness of the parts of solid bodies.

WE took six beaten leaves of gold, which we measured one by one with a ruler purposely made for nice experiments, and found them to have a greater equality in dimensions, and to be nearer true squares than could be well expected; the side of the square was in each of them exactly enough three inches and  $\frac{2}{3}$  or  $\frac{1}{4}$ , which number being reduced to a decimal fraction, viz.  $\frac{31}{100}$ , and multiplied by itself, affords  $\frac{91}{10000}$  for the area, or superficial content of each square leaf; and this multiplied by 6, the number of the leaves amounts to  $\frac{546}{10000}$  square inches, for the area of the six leaves. These, being carefully weighed in a pair of tender scales, amounted all of them to one grain and a quarter; and so one grain of this foliated gold was extended to somewhat above fifty inches; which differed but about a fifth part from an experiment of the like nature, that I remember I made many years ago in a pair of exact scales; and so small a difference may very well be imputed to that of the pains and diligence of the gold-beaters, who do not always work with equal strength and skill, nor upon equally fine and ductile gold.

Now if we recal to mind what I was lately saying, of the actual divisibility of an inch into an hundred sensible parts, and suppose an inch so divided, to be applied to each side of a square inch of the leaf-gold newly mentioned, it is manifest, that by subtle parallel lines, drawn between all the opposite points, a grain of gold must be divisible into five hundred thousand little squares, very minute indeed, but yet discernible by a sufficient sharp-sighted eye. And if we suppose an inch to be divided into two hundred parts, as I lately told you it was in a ruler I employ, then, according to the newly-recited way, the number of the squares, into which a single grain is capable of being divided, will amount to no less than two millions.

THERE is yet another way that I took to shew, that the extensibility, and consequently the divisibleness of gold, is probably far more wonderful than by the lately mentioned trial it appears.

For this purpose, I went to a great refiner whom I used to deal with for purified gold and silver, and enquired of him how many grains of leaf-gold he was wont to allow to an ounce of silver, when it was to be drawn into gilt wire as slender as an hair. To this

he answered me, that eight grains was the proportion he allowed to an ounce, when the wire was to be well gilt; but if it were to be more slightly gilt, six grains would serve the turn. And to the same purpose I was answered by a skilful wire-drawer. And I remember, that desiring the refiner to shew me an ingot of silver, as he did at first gild it; he shewed me a good fair cylindrical bar, whereon the leaf-gold that overlaid the surface, did not appear to be, by odds, so thick as fine Venetian paper; and yet comparing this with gilt wire, which I also desired to see, the wire appeared to be the better gilt of the two; possibly, because the gold, in passing through the various holes, was by the sides of them not only extended, but polished, which made it look more vividly than the unpolished leaves that gilded the ingot.

So that, if we suppose an ounce of the gilt wire formerly mentioned to have been gilt with six grains of leaf-gold, it will appear, by an easy calculation, that at this rate one ounce of gold, employed on gilding wire of that slenderness, would reach between ninety and an hundred miles. But if now we further suppose, as we lately did, that the slender silver wire, mentioned at the beginning of this chapter, were gilt; though we should allow it to have (because of its exceeding slenderness) not, as the former, six grains, but eight grains of leaf-gold to an ounce of silver, it must be acknowledged, that an hollow cylinder, or sheath of gold, weighing but eight grains, may be so stretched, that it will reach to no less than sixty times as much in weight of silver wire, as it covers [I said sixty times, for so often is eight contained in four hundred and eighty, the number of grains in an ounce]; and consequently a grain of that wire having been found to be twenty-seven feet long, the ounce of gold would reach to seven hundred seventy-seven thousand six hundred feet, that is, an hundred fifty-five miles and above a half. And if we yet further suppose this superficial, or hollow cylinder of gold, to be slit all along, and cut into as slender lists or thongs as may be, we must not deny that gold may be made to reach to a stupendous length. But we need not this last supposition to make what preceded it an amazing thing; which yet, though it be indeed stupendous, and seem incredible, ought not at all to be judged impossible, being no more than what, upon the suppositions and observations above laid down, does evidently follow.

### C H A P. III.

AFTER what has been said of the minuteness of tangible objects, it will be proper to subjoin some instances of the smallness of such as yet continue visible. But, in regard these corpuscles are singly too little to have any common measure applied to any of them, we must make an estimate of their minuteness by the number of those, into which a small portion or fragment of matter may be actually divided, the multitude of these being afforded by so inconsiderable a quantity of matter, sufficiently declaring, that each of them, in particular, must be marvelously little.

AMONG the instances, where the smallness of bodies may be deduced from what is immediately the object of sight, it may not be unfit to take notice of the evaporation of water, which though it be granted to consist of gross particles, in comparison of the spirituous and odoriferous ones of divers other liquors, as of pure spirit of wine, essential oils of spices, &c. yet to shew, that a small quantity of it may be dispersed into a multitude of manifestly visible corpuscles, I thought upon, and more than once tried the rarefaction of it into vapours by help of an æolipile, wherein, when I made the experiment the last time, I took the pains to register the event as follows:

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WE put an ounce of common water into an æolipile, and having put it upon a chafing-dish of coals, we observed the time, when the streams of vapours began to be manifest. This stream was, for a good while, impetuous enough, as appeared by the noise it made, which would be much encreased, if we applied to it, at a convenient distance, a kindled brand, in which it would blow up the fire very vehemently. The stream continued about a quarter of an hour, sixteen minutes, or better, but afterwards the wind had pauses and gusts for two or three minutes before it quite ceased. And by reason of the shape of the æolipile (which being framed chiefly for other purposes, was not so convenient for this) a great portion of the vapours condensed in the upper part of it, and fell down in drops; so that supposing that they also had come out in the form of wind, and the blast had not been intermitted toward the latter end, I guessed it might have continued uninterruptedly eighteen or twenty minutes. Note, that applying a measure to the smoke, that came out very visible in a form almost conical, where it seemed to have an inch or more in diameter, it was distant from the hole of the æolipile about twenty inches; and five or six inches beyond that, though it were spread so much, as to have four or five inches in diameter, yet the not uniform, but still-cohering clouds, which was the form, wherein the vapours appeared, were manifest and conspicuous.

AFTER the rarefaction of water, when it is turned into vapours, we may consider that of fewel, when it is turned into flame; to which purpose, I might here propose several trials, as well of our own as others, about the prodigious expansion of some inflammable bodies upon their being actually turned into flame. But in this place to mention all these, would perhaps too much intrench upon another paper; and therefore I shall here propose to your consideration but one instance, and that very easy to be tried, of which I find this account among my *adversaria*.

HAVING oftentimes burnt spirit of wine, and also oil in glass lamps, that for certain uses were so made, that the surface of the liquor was still circular, it was obvious to observe, how little the liquor would subside by the waste, that was made of it, in about half a quarter of an hour. And yet if we consider, that the naked eye, after some exercise, may, as I have often tried, discern the motions of a pendulum, that swings fast enough to divide a single minute of an hour into two hundred and forty parts, and consequently half a quarter of an hour into one thousand eight hundred parts; if we also consider, into how many parts of the time employed by a pendulum, the vibrations, slow enough to be discernible by the eye, may be mentally subdivided; and if we further consider, that, without intermission, the oil is preyed upon by an actual flame, and the particles of it do continually furnish a considerable stream of shining matter, that with a strange celerity is always flying away; we may very well conceive, that those parts of flame into which the oil is turned, are stupendously minute, since, though the wasting of the oil is in its progress too slow to be perceived by the eye, yet it is undoubted, that there is a continual decrement of the depth of the oil, the physical surfaces whereof are continually and successively attenuated and turned into flame; and the strange subtily of the corpuscles of flame would be much the stronger argued, if we should suppose, that instead of common oil the flame were nourished by a fewel so much more compact and durable, as is that inflammable substance made of a metalline body, of whose lastingness I have elsewhere made particular mention\*, after having taught the way of preparing it.

\* In some papers about Flame.

HAVING in a pair of tender scales carefully weighed out half a grain of good gunpowder, we laid it on a piece of tile, and whelmed over it a vessel of glass (elsewhere described, and often mentioned) with a brass plate to cover the upper orifice of it. Then having fired the gunpowder, we observed that the smoke of it did opacate, and, as to sense, so fill the whole cavity of the glass, though its basis were eight inches, its perpendicular height above twenty inches, and its figure far more capacious, than if it were conical; and this smoke, not containing itself within the vessel, issued out at two or three little intervals, that were purposely left between the orifice of the vessel and the plate that lay upon it. This cover we then removed, that we might observe how long the smoke would continue to ascend; which we found it would do for about half a quarter of an hour, and during near half that time, viz. the three first minutes, the continually ascending smoke seemed to be, at its going out, of the same diameter with the orifice at which it issued; and it would ascend sometimes a foot, sometimes half a yard, sometimes two feet or more into the air, before it would disperse and vanish into it.

Now if we consider that the cavity of this round orifice was two inches in diameter, how many myriads of visible corpuscles may we easily conceive thronged out at so large an outlet, in the time above-mentioned, since they were continually thrusting one another forwards? and into so many visible particles of smoke must we admit, that the half grain of powder was shattered, beside those multitudes, which, having been turned into actual flame, may probably be supposed to have suffered a comminution, that made them become invisible. And though I shall not attempt so hopeless a work, as to compute the number of these small particles, yet to make an estimate, whereby it would appear to be exceeding great, I thought fit to consider, how great the proportion was between the spaces, that to the eye appeared all full of smoke, and the dimensions of the powder, that was resolved into that smoke. Causing then the glass to be filled with common water, we found it to contain above two-and-twenty pints of that liquor, and causing one of those measures to be weighed, it was found to weigh so near a pound (of sixteen ounces) that the computation of the whole water amounted to at least one hundred and sixty thousand grains, and consequently three hundred and twenty thousand half grains. To which if we add, that this gunpowder would readily sink to the bottom of water, as being (by reason of the salt-petre and brimstone, that make up at least six parts of seven of it) in specie heavier than it, and in likelihood twice as heavy (for it is not easy to determine it exactly) we may probably guess the space to which the smoke reached, to exceed five hundred thousand times that which contained the unfired powder; and this, though the smoke, being confined in the vessel, was thereby kept from diffusing itself so far, as by its streaming out it seemed likely, that it would have done.

To these instances from inanimate bodies, I shall subjoin one more taken from animals. Whereas then men have with reason wondered, that so small a body as a cheese-mite, which by the naked eye is oftentimes not to be taken notice of, unless it move (if even then it be so) should, by the microscope, appear to be an animal furnished with all necessary parts; whereas this, I say, has given just occasion to conclude, that the corpuscles that make up the parts of so small an animal, must themselves be extremely small; I think the argument may be much improved by the following consideration. Those that have had the curiosity to open from time to time eggs, that are sat upon by a hatching hen, cannot but have observed, how small a proportion, in reference to the bulk of the whole egg, the chick bears; when that which the excellent *Harvey* calls *punctum saliens*, discloses the motion of the heart, and the colour of the blood; and that



that even about the seventh or eighth day, the whole chick now visibly formed bears no great proportion to the whole egg, which is to supply it with aliment, not only for its nourishment, but speedy growth for many days after.

To apply this now to the matter in hand ; having several times observed, and shewn to others, that cheese-mites themselves are generated of eggs, if we conceive, that in these eggs, as in ordinary ones, the animal at its first formation bears but a small proportion to the bulk of the whole egg, the remaining part being to suffice for the food and growth of the embryo probably for a pretty while ; since, if an ingenious person, that I desired to watch them, did not misinform me, they used to be about ten or twelve days in hatching ; this whole egg itself will be allowed to be but little, in reference to the mite it came from, how extremely and unimaginably minute may we suppose those parts to be, that make up the alimental liquors, and even the spirits, that passing through the nerves, or analogous parts, serve to move the limbs and sensories of but, as it were, the model of such an animal, as, when it rests, would not, perhaps, itself, to the naked eye be so much as visible ; and in which we may presume the nobler sort of stabler parts to be of an amazing slenderness, if we consider, that, though in other hairy animals, the optick, or some other of the larger nerves do, I know not how many times, in thickness and circuit, surpass a hair of the same animal ; yet in a cheese-mite, though none of the largest of those creatures, we have divers times manifestly seen, as is before intimated, single hairs that grow upon the legs.

ANOTHER way there is, that I employed to give men cause to think, that the invincible effluvia of bodies, that wander through the air, may be strangely minute ; and this was by shewing how small a fragment of matter may be resolved into particles minute enough to associate themselves in such numbers with a fluid so much more dense than air, as water is, as to impart a determinate colour to the whole liquor. What I did with cochineal in prosecution of this design, my experiments about colours may inform you ; but I shall now relate the success of an attempt made another way, for which perhaps some of your friends, the chymists, will thank me ; though I was not solicitous to carry on the experiment very far with gold, not because I judged that less divisible into a number of coloured particles, but because I found, as I expected, that the paleness of the native colour of the gold may make it in the end less conspicuous, though, if I had then had by me a menstruum, as I sometimes had, that would dissolve gold blood-red, perhaps the experiment with gold would have surpassed that, which it is now time I should begin to relate, as soon as I have hinted to you by the way, that, for variety's sake, I made a trial with copper calcined *per se*, that I might not be accused of having omitted to employ a metal, whose body chymists suppose to be much opened by calcination. And though the event were notable, even in comparison of that of the experiment made with cochineal, yet my conjectures inclined me much to prefer the way described in the following account.

WE carefully weighed out in a pair of tender scales one grain of copper not calcined, but barely filed ; and because, as we made choice of this metal for its yielding in most menstrooms a blue, which is a deep and conspicuous colour, we also chose to make a solution, not in aqua fortis, or aqua regis, but the spirit of sal armoniack (as that is an urinous spirit) having found by former trials, that this menstruum would give a far deeper solution than either of the others. This lovely liquor, of which we used a good proportion, that all the copper might be thoroughly dissolved, we put into a tall cylindrical glass of about four inches in diameter, and by degrees poured to it of distilled water, which is more proper in this case than common water, which has oftentimes an inconvenient saltishness, till we had almost filled the glass, and saw the colour grow

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somewhat pale, without being too dilute to be manifest; and then we warily poured this liquor into a conical glass, that it might be the more easy to fill the vessel several times to the same height. This conical glass we filled to a certain mark four times consecutively, weighing it, and the liquor too, as often in a pair of excellent scales purposely made for statical experiments, and which, though strong enough to weigh some pounds in each scale, would, when not too much loaden, turn with about one grain. These several weights of the glass, together with the contained liquor, we added together, and then carefully weighing the empty glass again, we deducted four times its weight from the above-mentioned sum, and thereby found the weight of the liquor alone, to be that, which reduced to grains, amounted to 28,534, so that a grain of copper, which is not full half so heavy in specie as fine gold, communicated a tincture to 28,534 times its weight.

BUT now, if you please to take notice, that the scope of my experiment was to shew, into what a number of parts one grain of copper might be divided; you will allow me to consider, as I did, that this multitude of parts must be estimated by the proportion, not so much in weight as in bulk, of the tinging metal to the tinged liquor; and consequently, since that divers hydrostatical trials have informed me, that the weight of copper to the weight of water of the same bulk is *proximè* as nine to one, a grain-weight of copper is in bigness but the ninth part of as much water as weighs a grain; and so the formerly-mentioned number of the grains of water must be multiplied by nine, to give us the proportion between the tinging and tinged bodies, that is, that a single grain of copper gave a blueness to above 256,806 parts of limpid water, each of them as big as it. Which, though it may seem stupendous, and scarce credible, yet I thought fit to prosecute the experiment somewhat farther, by pouring all the liquor out of the tall cylindrical glass into another clean vessel, whence filling the conical glass twice, and emptying it as often into the same cylindrical glass, the third time I filled the conical glass with colourless distilled water, and pouring that also into the cylindrical glass, we found the mixed liquor to have yet a manifest, though but a pale blueness. And lastly, throwing away what was in the cylindrical glass, we poured into it, out of the same conical glass, equal parts of distilled colourless water, and of the tinged liquor we had formerly set a-part in the clean vessel; and found that though the colour were very faint and dilute, yet an attentive eye could easily discern it to be blueish; and so it was judged by an intelligent stranger, that was brought in to look upon it, and was desired to discover of what colour he thought it to be. Whereby it appears, that one grain of copper was able to impart a colour to above double the quantity of water above-mentioned.

THIS experiment I have allowed myself to be the longer and more particular in relating, both because I know not that any such has been hitherto either made or attempted, and because it will probably gratify your chymists, that love to have the tinctures of metals believed very diffusive; and because if circumstances were not added, it would seem to you as well incredible, as perhaps it does seem stupendous, that a portion of matter should be able to impart a conspicuous colour to above 256,806 times its bulk of water, and a manifest tincture to above 385,200 (for so it did, when the proportion of the tinged part to the whole mixture made of it, and the untinged part, was as 2 to 31) and a faint, but yet discernible and distinguishable colour, to above five hundred and thirteen thousand six hundred and twenty times its bulk of water.



## C H A P. IV.

It were easy for me (*Pyroph.*) to give you several instances, to shew, that the effluvia of liquors may get in at the pores of bodies, that are reputed of a close texture; but I shall at present forbear to mention such examples, not only because they belong to another place\*, where I take notice of them, but because many such would not seem so remarkable, nor be so considerable to our present purpose, as a few taken from bodies that are not fluid.

AND first, it is delivered by writers of good credit, that several persons (for the experiment does not hold in all) by barely holding for some time dried cantharides in their hands, have been put to much pain at the neck of the bladder, and have had some other parts ministering to the secretion of urine sensibly injured. That this is true, I am induced to believe, by what I have elsewhere related to you of the unwelcome experiment I had of the effect of cantharides applied but outwardly to my neck, and that unknown to me, upon the urinary passages; and that these operations are due to material effluxes, which, to get into the mass of blood, must pass through the pores of the skin, you will not, I presume, put me to prove.

SCALIGER Exercit. 186. relates, that in *Gascony*, his country, there are spiders of that virulency, that if a man treads upon them to crush them, their poison will pass through the very soles of his shoes. Which story, notwithstanding the reputation of the author, I should perhaps have left unmentioned, because of a much stranger about spiders, which he relates in the same section, but that I met with one that is analogous in the diligent *Piso's* late history of *Brasil*; where, having spoken of another venomous fish of that country, and the antidotes he had successfully used to cure the hurts it inflicts, he proceeds to that fish the natives call *Amoreatim*, of one kind whereof, called by the Portugals *Peixe Sola*, his words are these; *Que mira sanè efficacia non solum manum vel levissimo attritu, sed & pedem, licet optimè calceatum, piscatoris incautè pisciculum conterentis, paralyti & stupore afficit, instar torpedinis Europeæ, sed minus durabili.* Lib. 5. cap. 14.

WHAT I shall ere long have occasion to tell you of the power of the *Torpedo*, and some other animals, to affect the hand and arm of him that strikes them, seems applicable to the matter under consideration: for, though their affecting the striker at a distance may very well be ascribed to the stupefactive, or other venomous exhalations, that expire (and perhaps are as it were darted) from the animal irritated by the stroke, and are breathed in together with the air they infect; yet their benumbing, or otherwise affecting the arm that struck them, rather than any other part, seems to argue, that the poisonous steams get in at the pores of the skin of the limb, and so stupefy, or otherwise injure, the nervous and muscular parts of it.

OTHER examples belonging to this section may be referred hither from divers other places in these papers about occult qualities, and therefore I shall only add here, that most remarkable proof, That some emanations, even of solid bodies, may be subtil enough to get through the pores, even of the closest bodies; which is afforded us by the effluvia of the loadstone, which are by magnetical writers said to penetrate, without resistance, all kind of bodies. And though I have not tried this in all sorts, yet having tried it in metals themselves, I am apt to think, the general rule admits of very few exceptions, especially, if that can be fully made out, which is affirmed about the perviousness of

\* A Discourse of pores of bodies, and figures of corpuscles.

glafs to the effluxions of the loadstone. For, not only glafs is generally reputed to be as close a body as any is, but (which weighs more with me) I have by trials purposely made, had occasion to admire the closeness of very thin pieces of glafs. But the reason why I just now exprest myself with an *If*, was, because I was not entirely satisfied with the proof wont to be acquiesced in, of the perviousness of glafs; namely, that in dials and sea-compasses, that are covered with plates of glafs, the needle may be readily moved to and fro by a loadstone held over it. For these plates being commonly but fastened on with wax, or at best with cement, a sceptick may pretend, that the magnetical effluvia pass not through the glafs, but through that much more pervious matter that is employed to secure the commissures, only from the access of the air. To put then the matter past doubt, I caused some needles to be hermetically sealed up in glafs-pipes, which being laid upon the surface of water (whereon, by reason of the bigness of the cavities, they would lightly float) the included needles did not only readily feel the virtue of an externally applied loadstone (though but a weak one) but complied with it so well, that I could easily, by the help of the needle, lead, without touching it, the whole pipe, this was shut up in, to what part of the surface of the water I pleased. And I also found that by applying a better loadstone to the upper part of a sealed pipe, and a needle in it, I could make the needle leap up from the lower part, as near to the loadstone, as the interposed glafs would give it leave.

BUT I thought it would be more considerable, to manifest that the magnetical effluvia, even of such a dull body as the globe of the earth, would also penetrate glafs. And though this seem difficult to be tried, because no ordinary loadstone, nor any iron touched by it, was to be employed to work on the included iron; yet I thought fit to attempt it after this manner: I took a cylindrical piece of iron, of about the bigness of one's little finger, and between half a foot and a foot long (for I had formerly observed, that the quantity of unexcited iron furthers its operation upon excited needles) and having hermetically sealed it up in a glafs pipe but very little longer than it, I supposed that if I held it in a perpendicular posture, the magnetical effluvia of the earth, penetrating the glafs, would make the lower extremum of the iron answerable to the north pole; and therefore having applied this to the point of the needle in a dial, or sea-compass, that looked towards the north (for authors mean not all the same thing by the northern pole of a needle, or loadstone) I presumed it would, according to the laws magnetical (elsewhere mentioned) drive it away, which accordingly it did. And having for farther trial inverted the included iron (so that the end, which was formerly the lowermost, was now the uppermost) and held it in a perpendicular posture, just under the same point of the needle, that extreme of the iron-rod, which before had driven away this point, being by this inversion become, in a manner, a south-pole did, according to the same laws) attract it: by which sudden change of poles, merely upon the change of situation, it also appeared, that the iron owed its virtue only to the magnetism of the earth, not that of another loadstone, which would not have been thus easily alterable. And this experiment I the more particularly relate, because this is not the only place where I have occasion to make use of it.

#### C H A P. V.

ANOTHER proof of the great subtily of effluvioms may be taken from the small decrement of weight or bulk, that a body may suffer by parting with great store of such emanations.

THAT



THAT bodies, which infused in liquors impregnate them with new qualities suitable to those of the immersed bodies, do so by imparting to them somewhat of their own substance, will, I presume, be readily granted by those that conceive not how one body should communicate to another a solitary and naked quality, unaccompanied by any thing corporeal to support and convey it. But I would not have you think, *Pyrophilus*, that the only matter of fact I have to countenance this notion, is that experiment, which has convinced divers chymists and physicians, otherwise not friends to the corpuscular philosophy, that medicines may operate without any consumption of themselves. For though divers of these, some of them learned men, have confidently written, that glass of antimony, and crocus metallorum, being either of them infused in a great proportion of wine, will make it vomitive; and if that liquor be poured off, and new be poured on, every new portion of such liquor will be impregnated with the same virtue, and this though the liquor be changed a thousand times, and yet the antimonial glass or crocus will continue the same, as well in weight as virtue; and though thence some of them, especially chymists, argue, that some metals work without imparting any thing substantial, but only, as *Helmont* speaks of some of his arcana, by irradiation: yet, I confess, I have some doubts, whether the experiment have been competently tried, and shall not fully acquiesce in what has been said, till some skilful experimenter deliver it upon his own trial, and acquaint us too, with what instruments, and what circumspection he made it. For besides that the ingeniousest physicians I have questioned about it, acknowledged the taste, and sometimes the colour of the wine, to be altered by the infused mineral, I could not acquiesce in the affirmation of an ordinary chymist, or apothecary, or even physician, if he should barely aver that he had weighed an antimonial medicine before it was put to infuse, and after the infusion ended, and observed no decrement of weight. For I have had too much experience (as I elsewhere mention) of the difficulty of making exact statical trials; not to know that such scales as are wont to be employed by chymists and apothecaries in weighing drugs, are by no means fit to make trials with the nicety, which that I am speaking of requires: it being easy, even with the better sort of such inaccurate scales, especially if they be not suspended from some fixed thing, but held with the hand, to mistake half a grain, or a grain; and perhaps a greater quantity, and at least more than by divers of the experiments of this essay appears necessary to be spent upon the impregnating of a considerable proportion of liquor, with corporeal effluvioms. Besides that if, when the beaten crocus, or glass, be taken out of the wine to be weighed again, the experimenter be not cautious enough to make allowance for the liquor that will adhere to the medicament, it is plain that he may take notice of no decrement of weight, though there may be really effluvioms of the mineral amounting to several grains, imbibed by the liquor. And though he be aware of this, and dry the powder, yet it is not so easy, even for a skilful man, to be sure that none of the more viscous particles of the liquor stick to the mineral, and being sensible upon the balance, though not to the eye or hand, repair the recess of those emetick corpuscles, that diffused themselves into the menstruum. And the sense of these difficulties put me upon the attempting to make so noble an experiment with excellent scales, and the care that it deserves: but, after a long trial, an unlucky accident frustrated at last my endeavours. But though, till competent relators give us an account of this matter upon their own trial, and repeat the infusion very much oftener, than, for aught I find, any man has yet done, I must not acquiesce in all that is said of the impregnation of wine, or other liquors by antimonial glass and crocus metallorum; yet, that after divers repeated infusions, the mineral substance should not be sensibly diminished in bulk or virtue, may well suffice to make this instance, though not the only or chief that may be

be brought for our purpose, yet a pertinent one to it. For, that there is a powerful emetick quality imparted to the liquor, is manifest by experience; and that the mineral does not impart this virtue, as it were, by irradiation, but by substantial effluxion, seems to me very probable; not only because I conceive not, how this can be done otherwise, but because, as it is noted above, the wine does oftentimes change colour by being kept a competent time upon the mineral, as if it drew thence a tincture; and even when it is not discoloured, I think it unsafe to conclude, that the menstruum has not wrought upon it. For I have kept good spirit of vinegar for a considerable time, upon finely powdered glass of antimony made *per se*, without finding the spirit to be all tinged, though it is known, that antimonial glass is soluble in spirit of vinegar, as mine afterwards appeared to be, by a longer digestion in the same liquor. But there may be a great number of minute particles dissolved in the menstruum before they be numerous enough to change the colour of it. And with this agrees very well what is observed, that though too great a quantity of the prepared antimony be put into the liquor, yet it will not be thereby made too strongly emetick. For the wine, being a menstruum, will, like other menstrooms, be impregnated but to a certain measure, without dissolving the overplus of the matter that is put into it; and Mars, which is a harder and heavier body than glass of antimony, is itself in part soluble in good Rhenish, or other white wine (and that in no long time) and sometimes even in water.

I do not therefore reject the emetick infusion, as unfit to have a place in this chapter, but till the experiment have been a little more accurately made, I think it inferior, as to our purpose, to some of the instances to be met with in the next chapter, and perhaps also to that mentioned by *Helmont*, and tried by more than one of my acquaintance, concerning the virtue of killing worms, that mercury imparts to the water or wine, wherein it has been long enough infused, or else for a while decocted. Though quicksilver given in substance is commended as an effectual medicine against worms, not only by many professed \* spagirists, but by divers † methodists of good note. And though some other things, chymical and philosophical, keep me from being of their opinion, who think, that in this case the mercury impregnates the liquor, as it were, by irradiation, rather than in a corporeal manner; yet the eye does not perceive, that even limpid water takes any thing from clean and well-purged mercury, which we know; that divers corrosive liquors themselves will not work upon.

To this instance I must add one, that is yet freer from exceptions, which is, that having for curiosity sake suspended in a pair of exact scales, that would turn with a very small part of a grain, a piece of ambergris bigger than a walnut, and weighing betwixt an hundred and six score grains, I could not in three days and a half, that I had opportunity to make the trial, discover, even upon that balance, any decrement of weight in the ambergris; though so rich a perfume, lying in the open air, was like in that time to have parted with good store of odoriferous steams. And a while after suspending a lump of assa foetida five days and a half, I found it not to have sustained any discernible loss of weight, though, in spite of the unfavourable cold weather, it had about it a neighbouring atmosphere replenished with foetid exhalations. And when twelve or fourteen hours after, perhaps upon some change of weather, I came to look upon it, though I found, that in that time the æquilibrium was somewhat altered, yet the whole lump had not lost half a quarter of a grain; which induced me to think, that there may perhaps be steams discernible even by our nostrils, that are far more subtil than the odorous exhalations of

\* As Quercetanus, Libavius, Zabata, Burggravius.

† As Vidius, Paræus, Cæsalpinus, &c.



spices themselves. For having, in very good scales, suspended in the month of *March* an ounce of nutmegs, it lost in about six days five grains and a half. And an ounce of cloves, in the same time, lost seven grains and five-eighths.

You will perhaps wonder, why I do not prefer, to the instances I make mention of in this chapter, that, which may be afforded by the loadstone, that is acknowledged continually to emit multitudes of magnetical steams without decrement of weight. But though I have not thought fit to pass this wholly under silence, yet I forbear to lay so much stress on it, not only because my balances have not yet satisfied me about the effluvia of loadstones (for I take them not all to be equally diffusive of their particles) but because I foresee it may be doubted, whether loadstones, like odorous bodies, do furnish afresh of their own, all the corpuscles that from time to time issue from them; or, whether they be not continually repaired, partly by the return of the magnetical particles to one pole, that sallied out of the other; and partly by the continued passage of magnetical matter, supplied by the earth, or other mundane bodies, which make the pores or channels of the loadstone their constant thorough-fares.

I doubt not but it will make it more probable, that a small quantity of matter being scattered into invisible effluvia, may be exceedingly rarefied and expanded, if it can be made appear, that this little portion of matter shall, for a considerable time, emit multitudes of visible parts, and that in so close an order among themselves, as to seem in their aggregate but one entire liquor, endowed with a stream-like motion, and a distinct superficies, wherein no interruption is to be seen, even by an eye placed near it. To devise this experiment, I was induced, by considering, that hitherto all the total dissolutions, that have been made of pigments, have been in liquors naturally cold, and consisting probably of much less subtil, and certainly of much less agitated parts, than that fluid aggregate of shining matter, that we call flame; whereas I argued, that if one could totally dissolve a body composed of parts so minute as those of a metal, into actual flame, and husband its flame so, as that it should not immoderately waste, I should thereby dissolve the metal in a far more subtil menstruum than our common water, or aqua fortis, or aqua regis, or any other known menstruum I have yet employed. And consequently, the attenuation and expansion of the metal in this truly igneous menstruum would much surpass, not only what happens in ordinary metalline solutions, but possibly also what I have noted in the third chapter of this essay, about the strange diffusion of copper dissolved in spirit of urine and water. In prosecution of this design, I so prepared one single grain of that metal, by a way that I elsewhere teach, that it was dissolved in about a spoonful of an appropriated menstruum. And then having caused a small glass lamp to be purposely blown to contain this liquor, and fitted it with a socket and wick, we lighted the lamp, which, without consuming the wick, burnt with a flame large enough, and very hot, and seemed to be all the while of a greenish blue, as if it were but a finer and shining solution of copper. And yet this one grain of prepared metal tinged the flame, that was from moment to moment produced, during no less than half an hour and six minutes. And now if we consider, that in this flame there was an uninterrupted succession of multitudes of coloured particles newly extricated, and flying off in every of those many parts wherein a minute of time may either actually or mentally be divided; and if we consider flame as a light and very agitated body, passing with a stream upwards through the air, and if we also consider the quantity of liquor, that would (as I shall by and by tell you) run through a pipe of a much lesser diameter than that flame, within the compass of the fore-mentioned time: what a quantity of the streaming fluid, we call flame; if it could have been preserved, and collected into one body, may we suppose, would ap-

pear to have issued out of one grain of copper in the space of thirty-six minutes; and what a multitude of metalline corpuscles may we suppose to have been supplied for the tinging of that flame, during so long a time? since a cylindrical stream of water falling but through a very short pipe of glass, constantly supplied with liquors, did pass at such a rate, that though the aqueous cylinder seemed more slender by half, or perhaps by two-thirds, or better than the flame, yet we estimated, by the help of a minute-watch, and a good pair of scales, that, if I had had conveniencies to let it run long enough, the water effluxed in thirty-six minutes, the time of the flame's duration, would have amounted to above nine gallons, or, reckoning a pint of water to contain a pound of sixteen ounces, seventy-two pounds.

## C H A P. VI.

THE last sort of instances I shall propose to shew the strange subtily of effluvia, is of such as discover the great quantity of space that may, by a small quantity of matter, when rarefied or dispersed, be either filled as to sense, or, at least, made (as they speak) the sphere of its activity.

To manifest this truth, and thereby as well confirm the foregoing chapter, as make out what is designed in this, I shall endeavour to shew, and help your imagination to conceive, how great a space may be impregnated with the effluxions of a body, oftentimes without any sensible, and oftener without any considerable decrement in bulk, or weight, of the body that affords them. And in order to this, though I shall not pretend to determine precisely how little the substances I am to instance in, would waste upon the balance, because you will very easily see, they are not that way to be examined; yet I presume you will as easily grant, that the decrement of weight would be but inconsiderable, since, of such light substances, the loss even of bulk is so; which last clause I shall now attempt to make good, by setting down some observations, partly borrowed from the writings of approved physicians, and partly, that my friends and I have made about the durable evaporation of such small particles of the effluxions of animals, as are actually not to be discerned by the eye to have any of those things sticking to them, which are so very long in flying successively away.

It is wont to be somewhat surprizing to men of letters, when they first go a hawking with good spaniels, to observe with how great sagacity those dogs will take notice of, and distinguish by the scent, the places where partridges, quails, &c. have lately been. But I have much more wondered at the quick scent of an excellent setting-dog, who, by his way of ranging the fields, and his other motions, especially of his head, would not only intimate to us the kinds of game, whose scent he chanced to light on, but would discover to us where partridges have been, though perhaps without staying in that place, several hours before, and assist us to guess how long they had been gone before we came.

I HAVE had strange answers given me in *Ireland*, by those who make a gain, if not an entire livelihood, by killing of wolves in that country (where they are paid so much for every head they bring in) about the sagacity of that peculiar race of dogs they employ in hunting them; but not trusting much to those relators, I shall add, that a very sober and discreet gentleman of my acquaintance, who has often occasion to employ blood-hounds, assures me, that if a man have but passed over a field, the scent will lie, as they speak, so as to be perceptible enough to a good dog of that sort for several hours after. And an ingenious hunter assures me, that he has observed, that the scent



of a flying and heated deer will sometimes continue upon the ground from one day to the next following.

AND now we may consider these three things; first, that the substance left upon the grass, or ground, by the transient tread of a partridge, hare, or other animal, that does but pass along his way, does probably communicate to the grass, or ground, but some of those effluxions that transpire out of his feet, which being small enough to escape the discernment of the eye, may probably not amount to one grain in weight, or perhaps not to the tenth part of it. Next, that the parts of fluid bodies, as such, are perpetually in motion, and so are the invisible particles that swim in them, as may appear by the dissolution of salt or sugar in water, and the wandering of aqueous vapours through the air, even when the eye perceives them not. And thirdly, that though the atmosphere of one of these small parcels of the exhaling matter we are speaking of may oftentimes be exceeding vast, in comparison of the emittent body, as may be guessed by the distance at which some setters, or blood-hounds, will find the scent of a partridge, or deer; yet in places exposed to the free air, or wind, it is very likely, that these steams are assiduously carried away from their fountain, to maintain the fore-mentioned atmosphere for six, eight, or more hours, that is, as long as the scent has been observed to lie, there will be requisite a continual recruit of steams succeeding one another: and, that so very small a portion of matter, as that, which we were saying the *fomes* of these steams may be judged to be, being sensibly to impregnate an atmosphere incomparably greater than itself, and supply it with almost continual recruits, we cannot but think that the steams it parts with, must be of an extreme, and scarce conceivable minuteness.

AND we may further consider, that the substances which emit these steams, being such as newly belonged to animals, and were, for the most part, transpired through the pores of their feet, must be in likelihood a far more evaporable and dissipable kind of bodies, than minerals or adult vegetables, such as gunpowder is made of; so that if the grains of gunpowder emit effluvioms capable of being, by some animals, perceived at a distance by their smell, one may probably suppose, that the small grains of this powder may hold out very many times longer to supply an atmosphere with odorable steams, than the corpuscles left on the ground by transient animals.

Now though it be generally agreed on, that very few birds have any thing near so quick a sense of smelling, as setting-dogs, or blood-hounds, yet, that the odour of gunpowder, especially when assisted by the steams of the caput mortuum of powder formerly fired in the same gun, may by fowls be smelt at a notable distance, particularly when the wind blew from me towards them, I often persuaded myself I observed, especially as to crows, when I went a shooting; and was confirmed in that opinion, both by the common tradition, and by sober and ingenious persons much exercised in the killing of wild-fowl, and of some four-footed beasts.

I HAD forgotten to take notice of one observation of the experienced *Julius Palmarius*: whence we may learn, that beasts may leave upon the vegetables, that have touched their bodies for any time, such corpuscles, as though unheeded by other animals, may, when eaten by them, produce in them such diseases as the infected animals had. For this author writes, in his useful tract *De morbis contagiosis*, that he observed horses, beesves, sheep, and other animals, to run mad upon the eating of some of the straw on which some mad swine had lain.

AND now to resume and prosecute our former discourse, you may take notice, that the effluvia, mentioned to have been smelt by animals, are, though invisible, yet big enough to be the objects of sense; so that it is not improbable, that among the steams,

that no sense can immediately perceive, there should be some far more subtil than these, and consequently capable of furnishing an atmosphere much longer, without quite exhausting the effluviating matter that afforded them.

Lib. VI.  
Obfer. 22

FORESTUS, an useful author, recites an example of pestilential contagion long preserved in a cobweb.

ALEXANDER BENEDICTUS writes also, that at *Venice* a flock-bed did for many years harbour a pestiferous malignity to that degree, that when afterwards it came to be beaten, it presently infected the by-standers with the plague.

Lib. IV.  
de Feb.  
cap. 3.

AND the learned *Sennertus* himself relates, that in the year one thousand five hundred and forty-two, there did in the city of *Uratiflavia*, vulgarly *Breslaw*, where he afterwards practised physick, die of the plague, in less than six months, little less than six thousand men, and that from that time the pestilential contagion was kept folded up in a linen cloth about fourteen years, and at the end of that time being displayed in another city, it began a plague there, which infected also the neighbouring towns, and other places.

Lib. III.  
Con. 17.

TRINCAVELLA makes mention of a yet lastinger contagion, which occasioned the death of ten thousand persons, that lay lurking in certain ropes, with which, at *Jussinopolis*, those, that died of the plague, had been let down into the graves.

BUT though none of these relations should, to some criticks, appear scarce credible, it may be objected, that all these things, wherein this contagion resided, were kept close shut up, or at least were not exposed to the air. Wherefore having only intimated, that the exception, which I think is not irrational, would, though never so true, but lessen the wonder of these strange relations, without rendering them unfit for our present purpose, I shall add, that though it is the opinion of divers learned physicians, that the matter harbouring contagion cannot last above twenty, or a few more days, if the body it adheres to be exposed to the free air and the wind; and though I am not forward to deny, that their judgment may hold in ordinary cases; yet I must not deny neither, that a contagion may sometimes happen to be much more tenacious, and obstinate: of

Lib. IV.  
de Peste.

which I shall give but that one, almost recent instance, observed by the learned *Diemerbroek*, in his own apothecary, who having but removed with his foot, from one side to the other of a little arbour in his garden, some straw, that had lain under the pallet, on which near eight months before a bed had lain, wherein a servant of the apothecary's, that recovered, had been sick of the plague; the infectious steams presently invaded the lower part of his leg, and produced a pungent pain and blister, which turned to a pestilential carbuncle, that could scarce be cured in a fortnight after, though, during that time, the patient were neither feverish, nor, as to the rest of his body, ill at ease. This memorable instance, together with some others of the like kind, that our author observed in the same city of *Nimeguen*, obtained, not to say extorted, even from him, this confession; which I add, because it contains some considerable, and not yet mentioned circumstances of the recited case: *Hoc exemplo medicorum doctrina de contagio in semite latente satis confirmatur. Mirum tamen est, hoc contagium tanto tempore in praedicto stramine potuisse subsistere, utpote quod tota hyeme ventis & pluviis* (he adds in another place) *nivibus & frigori, expositum fuisset.*

AND now I will shut up this chapter with an instance, that some will think, perhaps, no less strange than any of the rest; which is, that though they that are skilful in the perfuming of gloves, are wont to imbue them with but an inconsiderable quantity of odoriferous matter, yet I have by me a pair of *Spanish* gloves, which I had by the favour of your fair and virtuous sister (*F.*) that were so skilfully perfumed, that partly by her, partly by those that presented them her



as a rarity, and partly by me, who have kept them several years; they have been kept about eight or nine and twenty years, if not thirty, and they are so well scented, that they may, for aught I know, continue fragrant divers years longer. Which instance if you please to reflect upon, and consider, that such gloves cannot have been carried from one place to another, or so much as uncovered, as they must often have been, in the free air, without diffusing from themselves a fragrant atmosphere, we cannot but conclude those odorous steams to be unimaginably subtil, that could for so long a time issue out, in such swarms, from a little perfumed matter lodged in the pores of a glove, and yet leave it richly stocked with particles of the same nature; though, especially by reason of some removes, in which I took not the gloves along with me, I forgot ever since I had them, to keep them so much as shut up in a box.

OF THE

G R E A T E F F I C A C Y

O F

E F F L U V I U M S.

C H A P. I.

**T**HEY that are wont, in the estimates they make of natural things, to trust too much to the negative informations of their senses, without sufficiently consulting their reason, have commonly but a very little and slight opinion of the power and efficacy of effluvioms: and imagine that such minute corpuscles (if they grant that there are such) as are not, for the most part of them, capable to work upon the tenderest and quickest of senses the sight, cannot have any considerable operation upon other bodies. But I take this to be an error, which, as it very little becomes philosophers, so it has done no little prejudice to philosophy itself, and perhaps to physick too. And therefore though the nature of my design at present did not require it, yet the importance of the subject would invite me to shew, that this is as ill-grounded as prejudicial a supposition.

AND indeed if we consider the subject attentively, we may observe, that though it be true, that, *ceteris paribus*, the greatness of bodies doth, in most cases, contribute to that of their operation upon others, yet matter or body being, in its own precise nature, an unactive or moveless subject, one part of the mass acts upon another, but upon the account of its local motion, whose operations are facilitated and otherwise diversified by the

the shape, size, situation, and texture both of the agent and of the patient. And therefore if corpuscles, though very minute, be numerous enough, and having a competent degree of motion, even these small particles, especially if fitly shaped, when they chance to meet with a body, which the congruity of its texture disposes to admit them at its pores, and receive their either friendly or hostile impressions, may perform such things in the patient, as visible and much grosser bodies, but less conveniently shaped and moved, would be utterly unable on the same body to effect.

AND that you may with the less difficulty allow me to say, that the effluvioms of bodies, as minute as they are, may perform considerable things, give me leave to observe to you, that there are at least six ways, by which the effluvioms of a body may notably operate upon another; namely, 1. By the great number of emitted corpuscles. 2. By their penetrating and pervading nature. 3. By their celerity, and other modifications of their motion. 4. By the congruity and incongruity of their bulk and shape to the pores of the bodies they are to act upon. 5. By the motions of one part upon another, that they excite or occasion in the body they work upon, according to its structure. And, 6. by the fitness and power they have to make themselves be assisted, in their working, by the mere catholic agents of the universe. And though it may perhaps be sufficiently proved, that there are several cases wherein a body that emits particles, may act notably upon another body, by this or that single way, of those I have been naming; yet usually the great matters are performed by the association of two, three, or more of them, concurring to produce the same effect. Upon which score, when I shall in the following paper refer an instance, or a phenomenon, to any one of the forementioned heads, I desire to be understood as looking upon that but as the head to which it chiefly relates, without excluding the rest.

#### C H A P. II.

TAKING those things for granted, that have, I hope, been sufficiently proved in the former tract about the subtilty of effluvioms, I suppose it will readily be allowed, that the emanations of a body may be extremely minute; whence it may be rightly inferred, that a small portion of matter may emit great multitudes of them.

Now, that the great number of agents may in many cases compensate their littleness, especially where they act, or resist *per modum unius*, as they speak, men would perhaps the more easily grant, if they took notice to this purpose of some familiar instances.

WE see, that not only lesser land-floods, that overflow the neighbouring fields, but those terrible inundations, that sometimes drown whole countries, are made by bodies singly so small and inconsiderable as drops of rain, when they continue to fall in those multitudes we call showers.

So the aggregates of such minute bodies as grains of sand, being heaped together in sufficient numbers, make banks, wherewith greatest ships are sometimes split, nay, and serve in most places for bounds to the sea itself.

AND though a single corn of gunpowder, or two or three together, are not of force to do much mischief, yet two or three barrels of those corns, taking fire together, are able to blow up ships and houses, and perform prodigious things.

BUT instead of multiplying such instances, afforded by bodies of small indeed, but yet visible bulk, I shall (as soon as I have intimated, that the above-mentioned drops of rain themselves consist of convening multitudes of vapours most commonly invisible in their ascent) endeavour to make out what was proposed, by two or three instances drawn from the operations of invisible particles.



And first, we see, that though aqueous vapours be looked upon as the faintest and least active effluvia, that we know of; yet when multitudes of them are in rainy weather dispersed thorough the air, and are thereby qualified to work on the bodies exposed to it, their operations are very considerable, not only in the dissolution of salts, as sea-salt, salt of tartar, &c. and in the putrefactive changes they produce in many bodies, but in the intumescence they cause in oak and other solid woods; as appears by the difficulty we often find in and before rainy weather, to shut and open doors, boxes, and other wooden pieces of work, that were before fit enough for the cavities they had been adjusted to.

I MIGHT here urge, that though the strings of viols and other musical instruments are sometimes strong enough to sustain considerable weights, yet if they be left screwed to their full tension (as it frequently happens) they are oftentimes, by the supervening of moist weather, made to break, not without impetuosity and noise. But it may suit better with my present aim, if I mention on this occasion (what I elsewhere more fully take notice of) being desirous to try, what a multitude even of aqueous steams may do, I caused a rope, that was long, but not thick, and was in part sustained by a pulley, to have a weight of lead so fastened to the end of it, as not to touch the ground, and after the weight had leisure allowed it to stretch the cord as far as it could, I observed, that in the moist weather the waterish particles that did invisibly abound in the air, did so much work upon and shorten the rope, as to make it lift up the hanging weight, which was, if I misremember not, about an hundred pounds.

THE invisible steams issuing out of the walls of a newly plaistered or whited room, are not sensibly prejudicial to those that do but transiently visit it, or make but a very short stay in it, though there be a charcoal-fire in the chimney; but we have many instances of persons, that by lying for a night in such rooms, have been the next morning, or sooner, found dead in their beds, being suffocated by the multitude of the noxious vapours emitted during all that time.

AND here I think it proper to observe, that it may much assist us to take notice of the multitude of effluvia, and make us expect great matters from them, to consider that they are not emitted from the body, that affords them, all at once, as hail-shot out of a gun, but issue from it, as the vaporious winds do out of an æolipile well heated, or waters out of a spring-head in continued streams, wherein fresh parts still succeed one another; so that though as many effluxions of a body as can be sent out at one time, were numerous enough to act but upon its superficial parts, yet the emanation of the next minute may get in a little farther, and each smallest portion of time supplying fresh recruits, and perhaps urging on the steams already entered, the particles may at length get into a multitude of the pores of the invaded body, and penetrate it to the very innermost parts.

### C H A P. III.

I COME now to shew, in the second place, that the subtil and penetrating nature of effluvia may, in many cases, co-operate with their multitude in producing notable effects; and that there are effluvia of a very piercing nature, though we shall not now enquire upon what account they are so, we may evince by several examples. For not only the invisible steams of good aqua-fortis and spirit of nitre do usually in a short time, and in the cold, so penetrate the corks wherewith the glasses that contained them, were stopped, as to reduce them into a yellow pap; but also the emanations of mercury have been sometimes found in the form of coagulated, or even of running mercury, in the heads

or

or very bones of those gilders, or venereal patients, that have too long, or too unadvisedly, been exposed to the fumes of it, though they never took quicksilver in its gross substance. Chymists too, often find in their laboratories, that the steams of sulphur, antimony, arsenick, and divers other minerals, are able to make those stagger, or perhaps strike them down, that without a competent wariness unlute the vessels, wherein they had been distilled or sublimed; of which I have known divers sad examples. And of the penetrancy, even of animal steams, we may easily be persuaded, if we consider how soon in many plagues the contagious, though invisible exhalations are able to reach the heart, or infect other internal parts; though in divers of these cases the blood helps to convey the infection, yet still the morbidick particles must get into the body before they can infect the mass of blood. And in those stupefactions that are caused at a distance by the torpedo, the parts mainly affected seem to be the nervous ones of the hand and arm, which are of the most retired and best fenced parts of those members. And there is a spirit of sal armoniack, that I make to smell to, whose invisible steams, unexcited by heat, are of so piercing a nature, that not only they will powerfully affect the eyes and nostrils, and throats, and sometimes the stomachs too (yet without proving vomitive) of the patients they invade, but also when a great cold has so clogged the organs of smelling, that neither sweet nor stinking odours would at all affect them, these piercing steams have not only in a few minutes both made themselves a way, and, which is more, so opened the passages, that soon after the patient has been able to smell other things also. And by the same penetrating spirit, a person of quality was, some time since, restored to a power of smelling, which he had lost for divers years (if he ever had it equally with other men) I could easily subjoin examples of this kind, but they belong to other places. And here I shall only add, that the steams of water itself, assisted by warmth, are capable of dissolving the texture of even hard and solid bodies, that are not suspected to be saline; as appears by the philosophical calcination (as chymists call it) wherein solid pieces of hartshorn are brought to be easily friable into powder, by being hung over waters, whilst their steams rise in distillation, and without the help of furnaces. The exhalations that usually swim every night in the air, and almost every night fall to the ground in the form of dews (which makes them be judged aqueous) are in many places of the torrid zone of so penetrating a nature, that, as eye-witnesses have informed me, they would, in a very short time, make knives rust in their sheaths, and swords in their scabbards, nay, and watches in their cases, if they did not constantly carry them in their pockets. And I have known even in *England* divers hard bodies, into which the vapours swimming in the air have insinuated themselves so far as to make them friable throughout. But of the penetration of effluvia, I have given, in several places, so many instances, that it is not necessary to add any here. And therefore to shew, that, as I intimated at the beginning of this chapter, the penetrancy and the multitude of effluvia may much assist each other, I shall now subjoin, that we must not for the most part look upon effluvia, as swarms of corpuscles that only beat against the outides of the bodies they invade, but as corpuscles, which by reason of their great and frequently recruited numbers, and by the extreme smallness of their parts, insinuate themselves in multitudes into the minute pores of the bodies they invade, and often penetrate to the innermost of them; so that, though each single corpuscle, and its distinct action, be inconsiderable, in respect of the multitude of parts that compose the body to be wrought on; yet a vast multitude of these little agents working together upon a correspondent number of the small parts of the body they pervade, they may well be able to have powerful effects upon the body that those parts constitute; as, in the case mentioned in the former chapter, the rope would not probably have been enabled to raise so great a weight, though a vehement wind



wind had blown against it, to make it lose its perpendicular straightness, but a vast multitude of watery particles, getting by degrees into the pores of the rope, might, like an innumerable company of little wedges, so widen the pores, as to make the threads or splinters of hemp, the rope was made up of, swell, and that so forcibly, that the depending weight could not hinder the shortening of the rope, and therefore must of necessity be raised thereby. And I have more than once known solid, and even heavy mineral bodies burst in pieces by the moisture of the air, though we kept them within-doors carefully sheltered from the rain.

C H A P. IV.

THAT the celerity of the motion of very minute bodies, especially conjoined to their multitudes, may perform very notable things, may be argued from the wonderful effects of fired gunpowder, *aurum fulminans*, of flames, that invisibly touch the bodies they work on, and also whirlwinds, and those streams of invisible exhalations and other aerial particles we call winds. But because instances of this sort suit not so well with the main scope of this tract, I shall not insist on them, but subjoin some others, which, though less notable in themselves, will be more congruous to my present design. That the corpuscles, whereof odours consist, swim to and fro in the air, as in a fluid vehicle, will by most, I presume, be granted, and may be easily proved. But I have elsewhere shewn that the motion of the effluvia of some sufficiently odorous bodies has too little celerity to make a sensible impression on the organs of smelling, unless those steams be assisted to beat more forcibly upon the nostrils by the air, which hurries them along with it, when it enters the nostrils in the form of a stream, in the act of inspiration. And I have by familiar observation of hunters, fowlers, and partly of my own, made manifest, that setting-dogs, hounds, crows, and some other animals, will be much more affected with scents, or the odorous effluvia of partridges, hares, gunpowder, &c. when the wind blows from the object towards the sensory, than when it fits the contrary way, which way soever the nostrils of the animal be obverted, so the air be imbued with the odorous steams; and consequently the difference seems to proceed from this, that when the nostrils are obverted to the wind, the current of the air drives the steams forcibly upon the sensory, which otherwise it does not.

THAT there is a briskness of motion requisite, and more than ordinarily conducive to electrical attractions, may be argued from the necessity that we usually find by rubbing amber, jett, and other electrical bodies, to make them emit those steams, by which it is highly probable their action is performed; and though I have elsewhere shewn that this precedent rubbing is not always necessary to excite all electrical bodies; yet in those that I made to attract without it, it would operate much more vigorously after attrition; which I conceive makes a reciprocal motion amongst the more stable parts, and does thereby, as it were, discharge and shoot out the attracting corpuscles; whose real emission, though it may be probably argued from what has been already said, seems more strongly provable by an observation that I made many years ago, and which I have been lately informed to have been long since made by the very learned *Fabri*. The observation was this; that if, when we took a vigorously excited electrick, we did, at a certain nick of time (which circumstances may much vary, but was usually almost as soon as the body was well rubbed) place it at a just distance from a suspended hair, or other light body, or perhaps from some light powder; the hair, &c. would not be attracted to the electrick, but driven

away from it, as it seemed, by the briskly moving steams that issue out of the amber or other light body.

THIS argument I could confirm by another phænomenon or two, of affinity with this, if I should not borrow too much of what I have elsewhere noted about the history of electricity.

I KNOW a certain substance, which, though made by distillation, does in the cold emit but a very mild and inoffensive smell, but when the vessel that holds it is heated, though no separation of constituent principles appear to be thereby made (the body being in all usual trials homogeneous) the effluvioms will be so altered, that I remember a virtuoso, that, to satisfy his curiosity, would needs be smelling to it, when it was heated, complained to me, that he thought the steams would have killed him, and that the effluvioms of spirit of sal armoniack itself were nothing near so strong and piercing as those.

AND even among solid bodies, I know some, which, though abounding much in a substance, wherein some rank smells principally reside, yet (if they were not chafed) were scarce at all sensibly odorous; but upon the rubbing of them a little one against the other, the attrition making them, as it were, dart out their emissions, would in a minute or two make them stink egregiously.

AND as the celerity of motion may thus give a vigour to the emanations of bodies, so there may be other modifications of motion that may contribute to the same thing, and are not to be wholly neglected in this place. For as we see that greater bodies do operate differently, according to such and such modifications; as there is a great difference between the effects of a dart, or javelin, so thrown, as that its point be always forwards, and the same weapon, if it be so thrown, that during its progressive motion the extremes turn about the center of gravity, or some inward parts, as it happens, when boys throw sticks to beat down fruit from the tops of trees; so there is little doubt to be made, but, that in corpuseles themselves, it is not all one, as to their effects, whether they move with, or without rotation, and whether in such or such a line, and whether with, or without undulation, trembling, or such a kind of consecution; and in short, whether the motion have, or have not this or that particular modification; which, how much it may diversify the effects of the bodies moved, may appear by the motion that the aerial particles are put into by musical instruments. For though the effects of harmony, discord, and peculiar sounds, be sometimes very great, not only in human bodies, but, as we shall shew in the following tract, in organical ones too; the whole efficacy of musick, and of sounds, that are not extraordinary loud and different, seems, as far as it is ascribable to sonorous bodies, to depend upon the different manners of motion wherinto that air is put, that makes the immediate impression on our organs of hearing.

#### C H A P. V.

I SHOULD now proceed to shew, how the celerity and other modes, that diversify the motion of effluvioms, may be assisted to make them operative by their determinate sizes and figures, and the congruity or incongruity which they may have upon that score, with the pores of the grosser bodies they are to work on; but I think it not fit to entrench upon the subject of another \* tract, where the relation between the figure of corpuseles, and the pores of grosser bodies, is amply enough treated of. And therefore I shall only, in this place, take notice of those effects of lightning, which seem referrible, partly to the

\* Of the Pores of Bodies, and Figures of Corpuseles.



celerity and manner of appulse, and partly to the distinct sizes and shapes of the corpuscles that compose the destructive matter, and to the peculiar relation between the particles of that matter, and the structure of the bodies they invade. I know, that many strange things that are delivered about the effects of what the Latins call *fulmen*, which our English word lightning does not adequately render, are but fabulous; but there are but too many that are not so; some of which I have been an eye-witness of, within less than a quarter of an hour after that the things happened. And though it be very difficult to explicate particularly many of these true phenomena, yet it seems warrantable enough to argue from them, that there may be agents so qualified, and so swiftly moved, that notwithstanding their being so exceedingly minute, as they must be, to make up a flame, which is a fluid body, they must, in an imperceptible time, pervade solid bodies, and traversing some of them, without violating their texture, burn, break, melt, and produce other very great changes in other bodies, that are fitted to be wrought on by them. And of this, I must not forget to mention this remarkable instance; that a person curious enough to collect many rarities, bringing me one day into the study, where he kept the choicest of them, I saw there, among other things, a fine pair of drinking-glasses, that were somewhat slender, but extraordinarily tall; they seemed to have been designed to resemble one another, and made for some drinking entertainment. But before I saw them, that resemblance was much lessened by the lightning, that fell between them in so strange a manner, that, without breaking either of them, that I could perceive, it altered a little the figure of one of them, near the lower part of the cavity; but the other was so bent, near the same place, as to make it stand quite awry, and give it a posture that I beheld not without some amazement. And I cannot yet but look upon it as a very strange thing, and no less considerable to our present purpose, that nature should, in the free air, make of exhalations, and that such as probably, when they ascended, were invisible, such an aggregate of corpuscles, as should, without breaking such frail bodies as glasses, be able in its passage thorough them, that is, in the twinkling of an eye, to melt them; which to do is wont, even in our reverberatory furnaces, to cost the active flames a pretty deal of time.

AND this calls into memory, that upon a time, hearing not far off from me such a clap of thunder, as made me judge and say, that questionless some of the neighbouring places were thunder-struck, I sent presently to make inquiry; which having justified my conjecture, I forthwith repaired to the house, where the mischief was done by something, which those that pretended to have seen it coming thither, affirmed to be like a flame, moved very obliquely. To omit the hurt, that seemed to have been done by a wind, that accompanied it, or was perhaps produced by it, to divers persons and cattle; that, which makes me here mention it, was, that observing narrowly what had happened in an upper room, where it first fell, I saw, that it had, in more than one place, melted the lead in its passage, though that possibly outlasted not the twinkling of an eye, without breaking to pieces the glass casements, or burning, that I took notice of, either the bed, or hangings, or any other combustible household-stuff; though, near the window, it had thrown down a good quantity of solid substance of the wall, through which it seemed to have made its passage in or out. And that which made me the less scruple to mention this accident, is, that having curiously pried into the effects of the *fulmen*, not only in that little upper room, but in other parts of the house, beneath whose lowermost parts it seemed to have ended its extravagant course, I could not but conclude, that if so be it were the same *fulmen*, it must have more than once gone in and out of the house, and that the line of its motion was neither straight, nor yet reducible to any curve or mixed line, that I had met with among mathematicians; but that, as I then told some of my

friends, it moved to and fro in an extravagant manner, not unlike the irregular and wriggling motion of those fired squibs, that boys are wont to make by ramming gun-powder into quills. But about thunder, more perhaps elsewhere. I shall here only add, that whereas it is a known tradition, which my own observations heedfully made seem now and then to confirm, that vehement thunder, if beer be not very strong, will usually (for I do not say always) sour it in a day or two; if this degeneration be not one of the consequences of the great and peculiar kinds of the concussions of the air that happens in loud thunder (in which case, the phenomenon will belong to the next discourse) the effect may probably be imputed to some subtil exhalations diffused thorough the air, which, penetrating the pores of the wooden vessels, whose contexture is not very close, imbue the liquor with a kind of acetous ferment: which conjecture I should think much confirmed by a trial, it suggested to me, if I had made it often enough to rely upon it. For considering that the pores of glass are straight enough to be imperious (for aught I have yet observed) to the steams or spirituous parts of sulphur, as well as to other odorous exhalations, I thought it worth trying whether there be any sulphureous steams or other corpuscles diffused thorough the air in time of thunder, that would not be too gross to get in at such minute pores as those of glass. And accordingly, having hermetically sealed up both beer and ale apart, I kept them in summer-time, till there happened a great thunder, a day or two after which the beer, which we drank, that was good before, being generally complained of, as soured by the thunder, I suffered my liquors to continue at least a day or two longer, that the souring steams, if any such there were, might have time enough to operate upon them, and then breaking the glasses, I found not that the liquors had been soured, though we had purposely forbore to fill the glasses, to facilitate the degeneration of the liquors. Perhaps it will be pardonable, on this occasion, to mention a practice, which is usual in some places where I have been, and particularly employed by a great lady, that is, a great housekeeper and is very curious and expert in divers physical observations; for, talking with her about the remedies of the souring of beer, and other drinks, by thunder, which is sometimes no small prejudice to her, she affirmed to me that she usually found the practice I was mentioning, succeed; and, that before the then last great thunder, of which I had observed the effects upon beer, she preserved hers by putting, at a convenient distance, under the barrels, chafing-dishes of coals, when she perceived that the thunder was like to begin, which practice, if it constantly succeed, may put one a considering, whether the fire do not, by rarefying the air, and discharging the sulphureous or other steams, by altering them, or by uniting with them the exhalations of the coals, or by some such kind of way, render ineffectual these souring corpuscles, which perhaps require a determinate bulk and shape, besides their being crowded very many of them together, to have their full operation on barrelled liquors. But these things are but mere conjectures, and therefore I proceed.

#### C H A P. VI.

THE fifth way, whereby effluvia may perform notable things, is the motion of one part upon another, that they may excite or occasion in the body they work on, according to its structure.

I SHALL, in the following tract, have occasion to say something of the motions into which the internal parts of inanimate bodies may put one another; but the examples now produced are designed to manifest the efficacy that effluvia may, on the newly-mentioned accounts, have on organical and living bodies. To which instances, it would  
yet



yet be proper to premise, that even inanimate and solid bodies may be of such a structure, as to be very much alterable by the appropriated effluvioms of other bodies, as may be instanced in the power, that I have known some vigorous loadstones to have, of taking away, in a trice, the attractive virtue of an excited needle, or giving a verticity directly contrary to the former, without so much as touching it.

AND we may pertinently take notice of the attractive virtue of the loadstone, as that which may afford us an eminent example of the great power of a multitude of invisible effluvioms, even from bodies that are not great, upon bodies that are inorganic or liveless; for, taking it for granted, what both the Epicureans, Cartesians, and almost all other corpuscularian philosophers agree in, that magnetism is performed by corporeal emissions, we may consider, that these passing unresistedly thorough the pores of all solid bodies, and even glass itself, which neither the subtlest odours, nor electrical exhalations are observed to do, seem to be almost incredibly minute, and much smaller than any other effluvioms, though themselves too small to be visible; and yet these so incomparably little magnetical effluvioms proceeding from vigorous loadstones, will be able to take up considerable quantities of so ponderous a body as iron; insomuch, that I have seen a loadstone, not very great, that would keep suspended a weight of iron, that I could hardly lift up to it with one arm; and I have seen a little one, with which I could take up above eighty times its weight. And these effluvia do not only for a moment fasten the iron to the stone, but keep the metal suspended as long as one pleases.

THIS being premised, I come now to observe, that the chief effects of effluvia belonging to the fifth head are wrought upon animals, which, by virtue of their curious and elaborate structure, have their parts so connected and otherwise contrived, that the motions or changes that are produced in one, may have, by the consent of parts, a manifest operation upon others, although perhaps very distant from it, and so framed, as to declare their being affected by actions that seem to have no affinity at all with the agents that work upon the part first affected.

I HAVE shewn at large, in another \* treatise; that a human body ought not to be looked upon merely as an aggregate of bones, flesh, and other consistent parts, but as a most curious, and a living engine, some of whose parts, though so nicely framed, as to be very easily affected by external agents, are yet capable of having great operations upon the other parts of the body they help to compose. Wherefore, without now repeating what is there already delivered, I shall proceed to deliver such effects as are wrought on human bodies by these effluvioms, without any immediate contact of the bodies that emit them.

AND, first, not to mention light, because its being, or not being a corporeal thing, is much disputed, even among the moderns; it is plain, that our organs of smelling are sensibly affected by such minute particles of matter as the finest odours consist of. Nor do they always affect us precisely as odours, since we see, that many persons, both men and women, are by smells, either sweet or stinking, put into troublesome headaches.

IF it were not almost ordinary, it would be more than almost incredible, that the smell of a pleasing perfume should presently produce, in a human body, that immediately before was well and strong, such faintnesses, swoons, loss of sensible respiration, intumescence of the abdomen, seeming epilepsies, and really convulsive motions of the limbs, and I know not how many other frightful symptoms, that, by the unskilful, are often taken for the effects of witchcraft, and would impose upon physicians themselves, if

\* The Usefulness of Experimental Philosophy.

their

their own, or their predecessors experience did not furnish them with examples of the like phenomena produced by natural means. Those symptoms manifest what the consent of parts may do in a human body; since even morbid odours, if I may so call them, by immediately affecting the organs of smelling, affect so many other parts of the *genus nervosum*, as oftentimes to produce convulsive motions, even in the extreme parts of the hands and feet.

NOR is the efficacy of effluvioms confined to produce hysterical fits, since these invisible particles may be able, and sometimes as suddenly, as perfumes are wont to excite them, to appease them; as I have very frequently, though not with never-failing success, tried, by holding a spirit, I usually make of sal armoniack, under the nostrils of hysterical persons. My remedy did not only often recover, in a trice, those whose fits were but ordinary, but did, more than once, somewhat to the wonder of the by-standers, relieve, within a minute or two, persons of differing ages, and constitutions that were suddenly fallen down by fits, that the by-standers judged epileptical, but I, hysterical.

I ATTRIBUTE the good and evil operations of the fore-mentioned steams, rather in general to the consent of the parts that make up the *genus nervosum*, than to any hidden sympathy or antipathy betwixt them and the womb, not only for other reasons, not proper to be insisted on here, but because I have known odours have notable effects even upon men. I know a very eminent person, a traveller, and a man of a strong constitution, but considerably sanguine, who is put into violent head-achs by the smell of musk. And I remember, that one day being with him, and a great many other men of note, about a publick affair, a man that had a parcel of musk about him, having an occasion to make an application to us, this person was so disordered by the smell, which to most of us was delightful, that, in spite of his civility, he was reduced to make us an apology, and send the perfumed man out of the room; notwithstanding whose recess, this person complained to me, a good while after, of a violent pain in his head, which I perceived had somewhat unfitted him for the transaction of the affair, whereof he was to be the chief manager. I know another person, whose happy muse hath justly made him many admirers, that is subject to the head-ach upon so mild a smell as that of damask-roses, and sometimes even of red roses; insomuch, that walking one day with him in a garden, whose alleys were very large, so that he might easily keep himself at a distance from the bushes, which bore many of them red roses, he abruptly broke off the discourse we were engaged in, to complain of the harm the perfume did his head, and desired me to pass into a walk that had no roses growing near it.

If it were not for the sex of the person, I could relate an instance that would be much more considerable, of the operation of roses. For I know a discreet lady, to whom their smell is not displeasing (for she answered me, that it was not so at all) but so hurtful, that it presently makes her sick, and would make her swoon, if not seasonably prevented; and she told me, that being once at a court, in which she was a maid of honour, though she herself did not know whence it came, she found herself extremely ill on a sudden, and ready to sink down for faintness; but being then in discourse with a person, whose high quality she paid her profound respect to, her civility, that kept her from complaining, or withdrawing, might have been dangerous, if not fatal to her, had not the prince, who was speaking with her, and who knew her antipathy to roses, taken notice, that her face grew strangely pale, and was covered with a cold sweat. For thereby presently guessing what might be the cause, which the sick lady herself did not, she asked aloud, whether some body had not brought roses (which were then in season) into the bed-chamber, which question occasioned a speedy withdrawing of a lady that stood at a distance off, and had about her roses, which were not seen by the patient, who was by this means preserved



preserved from falling into a swoon, though not from being for a while very much discomposed.

BUT this you may tell me was the case of a woman who complained her malady affected her heart, not her head. Wherefore returning to what I was speaking of before I mentioned her, I shall proceed to tell you, that as odours may thus give men the head-ach, so I have often found the smell of rectified spirit of sal armoniack to free men, as well as women, from the fits of that distemper; and that sometimes in so few minutes, that the persons relieved could scarcely imagine they could so quickly be so.

To which I shall not add the trials that I have successfully made upon myself, because being, thanks be to God, very seldom troubled with that distemper, the occasions I have had of making them have not been many. And though I have not always found so slight a remedy to work the desired cure, yet that it does it often, even in men, is sufficient to shew the efficacy of sanative effluvioms.

Now to manifest that steams do not operate only upon hysterical women, or persons subject to the head-ach, I will add some instances of the effects they may produce upon other persons and parts.

It is but too well known an observation, that women with child have been often made to miscarry by the stink of an ill-extinguished candle, though perhaps the smoke ascending from the snuff were dissipated into the invisible corpuscles, a good while before it arrived at the nostrils of the unhappy woman; and what violent and straining motions abortions are frequently accompanied with, is sufficiently known already.

I THINK I have elsewhere mentioned, that a gentleman of my acquaintance, a proper and lusty man, will be put into the fits of vomiting, by the smell of coffee boiled in water. I shall therefore rather mention that I know a physician, who having been, for a long time, when he was young, frequently compelled to take electuarium lenitivum, one of the gentlest and least unpleasant laxatives of the shops, conceived such a dislike of it, that still, as himself has complained to me, if he smell to it, as he sometimes happens to do in apothecaries shops, it will work (now and then for several times) upwards and downwards with him.

I KNOW another very ingenious person of the same faculty, that has been a traveller by sea and land, who has complained to me, that the smell of the grease of the wheels of a hackney-coach, though it do but pass by him, is wont to make him sick, and ready to vomit.

EVERY body knows that smoke is apt to make mens eyes water, and excite, in the organs of respiration, that troublesome and vehement commotion we call coughing. But we need not have recourse at all to visible fumes, for the production of the like effects; since we have often observed them, and repeated sneezings, to boot, to proceed from the invisible steams of spirit of sal armoniack, when phials containing that liquor, though they were perhaps but very small, were approached too hastily, or perhaps, too near to the nostrils.

AND, because, in most of the foregoing instances, the chief effects seem to be wrought, by the consent of parts, on the *genus nervosum*, and the action of one of them upon the other, and thereby upon several other parts of the body, I will subjoin a remarkable instance of the operation of a mild and grateful odour upon the humours themselves, and that in a man.

A FAMOUS apothecary, who is a very tall and big man, several times told me, that though he was once a great lover of roses, yet having had occasion to employ great quantities of them at a time, he was so altered by their steams, that now, if he come among the rose-bushes, the smell does much discompose him. And the odour of roses  
(I mean

(I mean incarnate-roses, which we commonly call damask-roses, though they be not the true ones) makes such a colliquation of humours in his head, that it sets him a coughing, and makes him run at the nose, and gives him a sore throat; and by an affluence of humours makes his eyes sore; insomuch, that during the season of roses, when quantities of them are brought into his house, he is obliged, for the most part, to absent himself from home.

## C H A P. VII.

ONE may shew on this occasion, that as there might be considerable things performed by effluvioms, as they make one part of a living engine work upon another by virtue of its structure, so the action of such invisible agents may, in divers cases, be much promoted by the fabrick and laws of the universe itself, upon this account, that, by the operation of effluvia upon particular bodies, they may dispose and qualify those bodies to be wrought upon, which before they were not fit to be, by light, magnetisms, the atmosphere, gravity, or some other of the more catholick agents of nature, as the world is now constituted. But not to injure another tract, I shall conclude this, when I shall have taken notice, that in the instances hitherto produced there has been a visible local distance between the body that emits streams, and that on which they work. But if I thought it necessary, it were not difficult to shew that one might well enough refer to the title of this tract divers effects of bodies, that are applied immediately to ours; such as are blood-stones, cornelians, nephritick stones, lapis Malacensis, and some amulets, and other solid substances, applied by physicians outwardly to our bodies. For in these applications the gross body touches but the skin, and the great effects which I elsewhere relate myself to have sometimes (though not often, much less always) observed to have followed upon this external contact or near application, may reasonably be derived from the subtle emanations that pass thorough the pores of the skin to the inward parts of the body; as is evident in those, who by holding cantharides in their hands, or having them applied to some remote external part, have grievous pains produced in their urinary parts, as it has happened to me, as well as to many others. And to the insinuation of these minute corpuscles that get in at the pores of the skin, seems to be due the efficacy of some medicines that purge, vomit, resolve the humours, or otherwise notably alter the body, being but externally applied; of which I could here give several instances, but that they belong more properly to another place, and are not necessary in this, where it may suffice to name the notorious power that mercurial ointments, or fumes, either together or apart, have of producing copious salivations, to shew, in general, that both the streams and the emanations of outwardly-applied medicinal bodies, may have some great effects on human ones.



OF THE  
DETERMINATE NATURE  
OF  
EFFLUVIUMS.

C H A P. I.

THE effluvia of bodies, *Pyrophilus*, being for the most part invisible, have been wont to be so little considered by vulgar philosophers, that scarce vouchsafing to take notice of their existence, it is no wonder that men have not been solicitous to discover their distinct natures and differences. Only *Aristotle*, and, upon his account, the schools have been pleased to think that the two grand parts of our globe do sometimes emit two kinds of exhalations or steams; the earthy part affording those that are hot and dry, others that are (not as many of his disciples mistake him to have taught cold and moist, but) hot and moist \*, which they usually call vapours, to discriminate them from the fumes or exhalations, though otherwise, in common acceptance, those appellations are very frequently confounded.

Lib. I.  
Meteor.  
cap. 3. & 4.

BUT, though the *Aristotelians* have thus perfunctorily handled this subject, it would not become corpuscularian philosophers, who attribute so much as they do to the insensible particles of matter, to acquiesce in so slight and jejune an account of the emanations of bodies. And since we have already shewn, that besides the greater and more simple masses of terrestrial and aqueous matter newly mentioned, there are very many mixed bodies that emit effluvia, which make, as it were, little atmospheres about divers of them, it will be congruous to our doctrine and design to add in this place, that besides the slight and obvious differences taken notice of by *Aristotle*, the steams of bodies may be almost as various as the bodies themselves that emit them; and that therefore we ought not to look upon them barely under the general and confused notion of smoke, or vapours, but may probably conceive them to have their distinct and determinate natures, oftentimes, though not always suitable to that of the bodies from whence they proceed.

AND, indeed, the newly-mentioned divisions of the schools give us so slight an account of the emanations of bodies, that, methinks, it looks like such another, as if one should divide animals into those that are horned, and those that have two feet; for, besides that the distinction is taken from a difference that is not the considerablest, there are divers animals, as many four-footed beasts and fishes, that are not comprized in it;

\* Cap. 3. "Ἐστὶ γὰρ ἀπὸ τοῦ γῆς καὶ πυρὸς καὶ ἀέρος καὶ ὕδατος καὶ ὁσίων."

and each member of the division comprehends I know not how many distinct sorts of animals, whose differences from one another are many times more considerable, than those that constitute the two supreme genuses, the one having bulls and goats, and rhinoceroses, and deer, and elks, and certain sea-monsters, whose horns I have seen; and the other genus comprising also a greater variety, namely, a great part of four-footed beasts, and, besides men, all the birds (for aught we know) whether of land or water. And as it would give us but a very slender information of the nature of an elk, or an unicorn, to know that it is an horned beast; or of the nature of a man, an eagle, or a nightingale, to be told that it is an hornless beast; so it will but very little instruct a man in the nature of the steams of quicksilver, or of opium, to be told, that they are vapours hot, or rather cold, and moist; or of the steams of amber, or cantharides, or cinnamon, or tobacco, to be told, that they are hot and dry. For besides that there may be effluvioms, which, even by their elementary qualities, are not of either of these two supreme genuses (for they may be cold and dry, or cold and moist) these qualities are often far from being the noblest, and consequently those, that deserve to be most considered in the effluvioms of this, or that body; as we shall by and by have occasion to manifest.

## C H A P. I.

AND here it may not be improper to mention an experiment, that, I remember, I divers years since employed to illustrate the subject of our present discourse.

I CONSIDERED then, that fluid bodies may be of very unequal density, and gravity, as is evident in quicksilver, water, and pure spirit of wine; which, notwithstanding their great difference in specifick gravity, may yet agree in the conditions requisite to fluid bodies. Therefore presuming, that by what I could make appear visible in one, what happens analogically in the other, may be ocularly illustrated, I took some ounces of roch-alum, and as much of fine saltpetre. I took some ounces of each, because, if the quantity of the ingredients be too small, the concoagulated grains will be so too, and the success will not be so conspicuous. These being dissolved together in fair water, the filtrated solution was set to evaporate in an open-mouthed glass, and being then left to shoot in a cool place, there were fastened to the sides, and other parts of the glass, several small crystals, some octoedrical, which is the figure proper to roch-alum, and others of the prismatical shape of pure saltpetre; besides some other saline concretions, whose being distinctly of neither of these two shapes, argued them to be concoagulations of both the salts. And this we did, by using such a degree of celerity in evaporating the liquor, as was proper for such an effect. For, by another degree, which is to be employed, when one would recover the salts more distinctly and manifestly, the matter may, as I found by trial, be so ordered, that the aluminous salt may, for the most part, be first coagulated by itself, and then, from the remaining liquor, curiously shaped crystals of nitre may be copiously obtained.

TRIALS like this we also made with other salts, and particularly with sea-salt, and with alum and vitriol; the phenomena of which you may meet with in their due places. For the recited experiment may, I hope, alone serve to assist the imagination to conceive, how the particles of bodies may swim to and fro in a fluid (which the air is) and though they be little enough to be invisible, may, many of them, retain their distinct and determinate natures, and their aptness to cohere upon occasion; and others may, by their various occurrences and coalitions, unite into lesser corpuscles, or greater bodies, differ-



differing from the more simple particles that composed them, and yet not of indeterminate, though compounded figures.

C H A P. III.

THESE things being premised, we may now proceed to the particular instances of the determinate nature of effluvioms; and these we may not inconveniently reduce to the three following heads, to each of which we shall assign a distinct chapter; the first of these I shall briefly treat of in this third chapter, and treat somewhat more largely of the others in the two following.

IN the first place then, that the effluvioms of many bodies retain a determinate nature oftentimes in an invisible smallness, and oftener in such a size, as makes them little enough to fly or swim in the air, may appear by this, that these effluvia being, by condensation, or otherwise, re-united, they appear to be of the same nature with the body that emitted them. Thus in moist weather, the vapours of water, that wander invisibly through the air, meeting with marble-walls, or pavements, or other bodies, by their coldness and other qualifications fit to condense and retain them, appear again in the form of drops of water; and the same vapours return to the visible form of water, when they fall out of the air in dews or rains.

QUICKSILVER itself, if it be made to ascend in distillation with a convenient degree of fire, will almost all be found again in the receiver in the form of running mercury. Which strange and piercing fluid is in some cases so disposed to be stripped of its disguises, and re-appear in its own form, that divers artificers, and especially gilders, have found, to their cost, that the fumes of it need not be, as in distillation, included in close vessels to return to their pristine nature, mercury having been several times found in the heads, and other parts of such people, who have, in tract of time, been killed by it, and sometimes made to discover itself during the lives of those, that dealt so much in it; of which I elsewhere give some instances. Wherefore I shall only observe at present, that it is a common practice, both among gilders, and some chymists, that, when they have occasion to make an amalgam, or force away the mercury from one by the fire, they keep gold in their mouths, which, by the mercurial fumes that wander through the air, will now and then, by that time it is taken out of their mouths, be turned white almost, as if it had been silvered over.

A MASS of purified brimstone being sublimed, the ascending fumes will condense into what the chymists call *stones Sulphuris*, which is true sulphur of the same nature with that formerly exposed to sublimation; and may readily, by melting, be reduced into such another mass.

AND to give you another like example of dry bodies, I tried, that by subliming good camphire in close vessels, it would all, as to sense, be raised into the upper vessel, or part of the subliming glass, in the form of dry camphire, as it was before.

NAY, though a body be not by nature, but art, compounded of such differing bodies, as a metal and another mineral, and two or three salts; yet, if upon purification of the mixture from its grosser parts, the remaining and finer parts be minute enough and fitly shaped, the whole liquor will ascend, and yet in the receiver altogether recover its pristine form of a transparent fluid, composed of differing saline and mineral parts. This is evident in the distillation of what chymists call butter, or oil of antimony, very well rectified. For this liquor will pass into the receiver diaphanous and fluid, though, besides the particles of the sublimate (which is itself a factitious compounded body) it

abounds

abounds with antimonial corpuscles, carried over and kept invisible by the corroding salts; whatever *Angelus Sala*, and those chymists that follow him, have affirmed to the contrary, as might be easily here proved, if this were a fit place to do it in.

I FOUND by enquiring of an ingenious person, that had an interest in a tin mine, that I was not deceived in guessing, that tin itself, though a metal, whose ore is of a very difficult fusion, and which I have by itself kept long upon the cupel without finding it to fly away, would yet retain its metalline nature in the form of fumes or flowers. For this experienced gentleman answered me, that divers times they would take great store of a whitish sublimate from the upper part of the furnaces or chimnies, where they brought their ore to fusion or wrought further upon it; and that this sublimate, though perhaps elevated to the height of an ordinary man, would, when melted down, afford at once many pounds of very good tin. On which occasion I shall add, that I have myself, more than once, raised this metal in the form of white corpuscles by the help of an additament, that did scarce weigh half so much as it.

#### C H A P. IV.

THE second way, by which we may discover the determinate nature of effluvioms, is, by the difference that may sometimes be observed in their sensible qualities. For these effluvioms, that are endowed with them, proceed from the same sort of bodies, and yet those afforded by one kind of bodies being in many cases manifestly differing from those that fly off from another, this evident disparity in their exhalations argues their retaining distinct natures, according to those of the respective bodies whence they proceed.

I WILL not now stay to examine, whether in the steams, that are made visibly to ascend from the terrestrial globe by those grand agents and usual raisers of them, the sun, and the agitation of the air, the eye can manifestly distinguish the diversity of colours: but in some productions of art, such different colours may be discovered in the exhalations, even without the application of any external heat to raise them. For, when spirit of nitre, for example, has been well rectified, I have often observed, that even in the cold the fumes would play in the unfilled part of the stopped phials it was kept in, and appear in it of a reddish colour, and if those vessels were opened, the same fumes would copiously ascend into the air, in the form of a reddish or orange-tawny smoke; spirit, or oil of salt also, if it be very well dephlegmed, though it will scarce in the cold visibly ascend in the empty part of a phial, whilst it is kept well stopped; yet, if the free air be allowed access to it, it will, in case it be sufficiently rectified, fly up in the form of a whitish fume. But this is inconsiderable, in comparison of what happens in a volatile tincture of sulphur I have elsewhere taught you to make with quick-lime. For, not only upon a slight occasion, the vacant part of the phial will be filled with white fumes, though the glass be well stopped; but upon the opening the phial these fumes will copiously pass out at the neck, and ascend into the air in the form of a smoke, more white than perhaps you ever saw any. And both this and that of the spirit of saltpetre do, by their operation, as well as smell, disclose what they are; the latter being of a nitrous nature (as is confessed) and the former, of a sulphureous: in so much, that having, for curiosity's sake, in a fitly shaped glass, caught a competent quantity of the ascending white fumes, I found them to have conveined into bodies transparent and geometrically figured, wherein it was easy to discover, by their sensible qualities, that there were store of sulphureous particles mixed with the saline ones. That the

liquors



liquors of vegetables, distilled in *balneo*, or in water, are not wont to retain any thing of the colour of the bodies that afforded them, is a thing easy to be observed in distillations made without retorts or the violence of the fire. But it may be worth while to make trial, whether the essential oil of wormwood ascend coloured like the plant whence it is first drawn over with water in the alembic, or rectified in *balneo*. For I forgot to take notice of it, when upon some particularities, I observed in that plant, my curiosity led me to find, that not only in the first distillation in a copper alembic, tinned on the inside, the oil came over green, but by a rectification purposely made in a glass vessel, the purified liquor was not deprived of that colour.

THE mention of these essential oils, as chymists call those that are drawn in alembics, leads me to tell you, that though these liquors be but effluvia of the vegetables they are distilled from, condensed again in the receiver into liquors; yet, as subtil as they are, many of them retain the genuine taste of the bodies, whence the heat elevated them; as you will easily find, if you will taste a few drops of the essential oil of cinnamon, for example, or of wormwood dissolved by the intervention of sugar, or spirit of wine, in a convenient quantity of water, wine, or beer. For by this means you have the natural taste of this spice or herb. And wormwood is a plant, whose effluvia do so retain the nature of the body that parts with them, that I must not forbear to alledge here an observation of mine, that may shew you, that it is possible, though not usual, that even without the help of the fire, the expirations of a body may communicate its taste. For, among other things that I had occasion to observe about some quantity of wormwood laid up together, I remember I took notice, and made others do the like, that coming into a room where it was kept, not only the organs of smelling were powerfully wrought upon by the corpuscles, that swarmed in the air, but also the mouth was sensibly affected with a bitter taste. Perhaps you will scarce think it worth while, that after this instance I should add, that I found the expirations of amber, kept a while in pure spirit of wine, taste upon the tongue like amber itself, when I chewed it between my teeth. But I chuse to mention this instance, because it will connect those lately mentioned with another sort, very pertinent to our present purpose. For, the expirations that I have obtained from amber, both with pure spirit of wine, and a more piercing menstruum, did manifestly retain in both these liquors a peculiar smell, with which I found it to affect the nostrils, when, for trial's sake, I excited the electrical faculty of amber by rubbing. And as for odours, it is plain, that the essential oils of chymists, well drawn, do many of them retain the peculiar and genuine scent of the spices or herbs that afforded them. And that these odours do really consist of, or reside in certain invisible corpuscles, that fly off from the visible bodies, that are said to be endowed with such smells, I have elsewhere proved at large; and it may sufficiently appear from their sticking to divers of the bodies they meet with, and their lasting adhesion to them.

OTHER examples may be given of the settled difference of effluvioms directly perceivable by human organs of sense, as dull as they are; which last expression I add, because I scarce doubt, but that, if our sensories were sufficiently subtil and tender, they might immediately perceive in the size, shape, motion, and perhaps colour too of some now invisible effluvioms, as distinguishable differences, as our naked eyes in their present constitution see, between the differing sorts of birds, by their appearances, and their manner of flying in the air, as hawks, and partridges, and sparrows, and swallows. To make this probable I will not urge, that in fine white sand, whose grains by the unassisted eye are not wont to be distinguished by any sensible quality, I have often observed in an excellent microscope, a notable disparity as to bulk, figure, and sometimes as to colour:

colour : and that in small cheese-mites, which the naked eye can very scarcely discern, so far is it from not discovering any difference between them, one may (as was noted in the last essay) plainly see, besides an obvious difference in point of bigness, many particular parts, on whose accounts, the structure of those moving points may difference them from each other. And I have sometimes seen a very evident disparity, even in point of shape, between the very eggs of these living atoms, as a poet would perhaps stile them. But these kinds of proofs (as I was saying) I shall forbear to insist on, that I may proceed to countenance my conjecture by the effects of the effluvioms, that are properly so called, upon animals.

AND first, though the touch be reckoned one of the most dull of the five senses, and be reputed to be far less quick in men, than in divers other animals ; yet the gross organs of that may, in men themselves, even by accident, be so disposed, as to be susceptible of impressions from effluvia : of this, in another paper, I give some instances. And I know not, whether divers of the presages of weather, to be observed in some animals, and the aches, and other pains, that in many crazy and wounded men, are wont to fore-run great changes of weather, do not often (for I do not say always) proceed, at least in part, from invisible, and yet incongruous effluxions, which, either from the subterranean parts, or from some bodies above ground, do copiously impregnate the air. And, on this occasion, it will not be impertinent to mention here what an experienced physician, being (if I much misremember not) the learned *Diemerbroek*, relates concerning himself, who having been infected with the plague by a patient, that lay very ill of it, though by God's blessing, which he particularly acknowledges, upon a slight, but seasonable remedy, he was very quickly cured, and that without the breaking of any tumor ; yet it left such a change in some parts of his body, that he subjoins this memorable passage, *Ab illo periculo ad contagiosos mibi appropinquant in emunctis successit dolor, vix fallax pestis indicium.*

About  
Cosmical  
Suspensions.

Two or three other observations of the like nature you meet with in another of my papers. And I shall now add, that I know an ingenious gentlewoman (wife to a famous physician) who was of a very curious and delicate complexion, that has several times assured me, that she can very readily discover, whether a person, that comes to visit her in winter, came from some place where there is any considerable quantity of snow ; and this she does, as she tells me, not by feeling any unusual cold (for if the ground be frozen, but not covered with snow, the effect succeeds not) but from some peculiar impression, which she thinks, she receives by the organs of smelling. I might add, that I know also, as I may have formerly told you, a very ingenious physician, who falling into an odd kind of fever, had his sense of hearing thereby made so very nice and tender, that he very plainly heard soft whispers, that were made at a considerable distance off, and which were not in the least perceived by the healthy by-standers, nor would have been by him before his sickness. Which sickness I mention as the thing, that gave his organs of hearing this preternatural quickness, because, when the fever had quite left him, he was able to hear but at the rate of other men. And I might tell you too, that I know a gentleman of eminent parts and note, who, during a distemper he had in his eyes, had his organs of sight brought to be so tender, that both his friends, and himself also, have assured me, that when he waked in the night, he could for a while plainly see and distinguish colours, as well as other objects, discernible by the eye, as was more than once tried, by pinning ribbands, or the like bodies, of several colours, to the inside of his curtains in the dark. For if he were awakened in the night, he would be able to tell his bedfellow, where those bodies were placed, and what colour each of them was of.



I HAVE mentioned these instances only to shew you, that if our sensories were more delicate and quick, they would be sufficiently affected by objects, that, as they are generally constituted, make no impressions at all upon them. For otherwise I know, that the species (as they call them) both of sounds and colours are not held by many of the moderns (from whom in that I dissent not) to be so much corporeal effluvioms, trajected through the medium, as peculiar kinds of local motion conveyed by it. Therefore, I shall now confirm the conjecture I would countenance, by the discrimination made by the organs of other animals, of such effluvia, as to us men are not only invisible, but sensible. And therefore, partly to strengthen what I delivered, and partly to confirm what I am now discoursing of, it will not be impertinent to subjoin two or three relations, that I had from persons of very good credit, whom I thought likely to make me no unsatisfactory returns to my questions, about things they were very well versed in.

A PERSON of quality, to whom I am near allied, related to me, that to make a trial, whether a young blood-hound was well instructed (or as the huntmen call it, made) he caused one of his servants, who had not killed, or so much as touched any of his deer, to walk to a country-town, four miles off, and then to a market-town, three miles distant from thence; which done, this nobleman did, a competent while after, put the blood-hound upon the scent of the man, and caused him to be followed by a servant or two, the master himself thinking it also fit to go after them to see the event; which was, that the dog, without ever seeing the man he was to pursue, followed him by the scent to the above-mentioned places, notwithstanding the multitude of market people, that went along in the same way, and of travellers, that had occasion to cross it. And when the blood-hound came to the chief market-town, he passed through the streets, without taking notice of any of the people there, and left not, till he had gone to the house, where the man, he sought, rested himself, and found him in an upper room, to the wonder of those that followed him. The particulars of this narrative, the nobleman's wife, a person of great veracity, that happened to be with him when the trial was made, confirmed to me.

ENQUIRING of a studious person, that was keeper of a red deer park, and versed in making blood-hounds, in how long time, after a man or deer had passed by a grassy place, one of those dogs would be able to follow him by the scent? he told me, that it would be six or seven hours: whereupon an ingenious gentleman, that chanced to be present, and lived near that park, assured us both, that he had old dogs of so good a scent, that if a buck had the day before passed in a wood, they will, when they come where the scent lies, though at such a distance of time after, presently find the scent and run directly to that part of the wood where the buck is. He also told me, that though an old blood-hound will not so easily fix on the scent of a single deer, that presently hides himself in a whole herd, yet if the deer be chased a little till he be heated, the dog will go nigh to single him out, though the whole herd also be chased. The above-named gentleman also affirmed, that he could easily distinguish, whether his hounds were in chase of a hare, or a fox, by their way of running, and their holding up their nose higher than ordinary, when they pursue a fox, whose scent is more strong. These relations will not be judged incredible by him, that reflects on some of the instances that have already, in the foregoing essay, been given of the strange subtilty of effluvia: to which I shall now add, that I remember, that to try, whether I could, in some measure, make art imitate nature, I prepared a body of a vegetable substance, which, though it were actually cold, and both to the eye and touch dry, did for a while emit such determinate and piercing, though invisible, exhalations, that having, for trial's  
fake,

fake, applied to it a clear metalline plate, and that of none of the very softest kind neither, for about one minute of an hour, I found, that though there had been no immediate contact between them, I have purposely interposed a piece of paper to hinder it, yet there was imprinted on the surface of the plate a conspicuous stain of that peculiar colour, that the body, with whose steams I had imbued the vegetable substance, was fitted to give a plate of that mixed metal. And though it be true, that in some circumstances, the lately-mentioned instances about blood-hounds have a considerable advantage of this I have now recited, yet that advantage is much lessened, not to say counterbalanced, by some circumstances of our experiment. For, not to repeat, that the emittent body was firm and cold, the effect produced by the effluvium, that guided the setting-dog, was wrought upon the sensory of a living and warm animal; and such an one, whose organs of smelling are of an extraordinary tender constitution above those of men, and other animals, and probably the impression was but transient; whereas, in our case, the invisible steams of the vegetable substance wrought upon a body, which was of so strong and inorganical a texture, as a compounded metal, though it were fenced by being lapped up in paper, notwithstanding which these steams invaded it in such numbers, and so notably, as to make their operation on it manifest to the eye, and considerably permanent too; since, coming to look upon the plate after the third day, I found the induced colour yet conspicuous, and not like suddenly to vanish.

HITHERTO, in this chapter, I have argued from the constant and settled difference of the sensible qualities of effluvioms, that they do not always lose their distinct natures, when they seem to have lost themselves by vanishing into air. But before I dismiss this subject, I must consider an objection, which I know may be made against the opinion we have been countenancing. For it may be alledged, that there may be many cases, wherein the effluvioms of bodies are, in their passage through the air, sensibly altered, or do affect the organs of sense, otherwise than each kind of them apart would do: nor is this difficulty altogether irrational. For it seems consonant enough to experience, that some such cases should be admitted; and therefore, in the foregoing discourse I have, where I thought it necessary, forbore to express myself in such general and absolute terms, as otherwise I might have done. But as for such cases, as I have insisted upon, and many more I shall now represent, that the objected alterations need not hinder, but that effluvioms, at their first parting from the bodies, whence they take wing, if I may so speak, may retain as much of the nature of those bodies, as we have ascribed to them; since the subsequent change may very probably be deduced from the combinations, or coalitions, of divers steams associating themselves in the air, and acting upon the sensory, either altogether and conjointly, or at least so near it, that the sense cannot perceive their operations as distinct. This I shall elucidate, but not pretend to prove, by what happens in sounds and tastes. For if, by way of instance, in a musical instrument, two strings tuned to an eighth, be touched together, they will strike the ear with a sound, that will be judged one, as well as pleasing, though each of the trembling strings make a distinct noise, and the one vibrates as fast again as the other. And if, into oil of tartar *per deliquium* you drop a due proportion of spirit of nitre, and exhale the superfluous moisture, the acid and alcalzate corpuscles, that were so small as to swim invisibly in those liquors, will convene into nitrous concretions, whose taste will be compounded of, but very differing from, both the tastes of the acid and tartarous particles; which particles may yet, for the most part, by a skilful distillation, be divorced again. And so, if to a strong solution of pot-ashes, or salt of tartar, you put as much in weight of sal armoniack, as there is of either of those fixed salts contained in the liquor; you may, besides a subtil urinous spirit, that will easily come over in the distil-



distillation, obtain a dry *caput mortuum*, which is almost totally a compounded salt, differing enough from either of the ingredients, especially the alcalizate, as well in taste, as in some other qualities: this salt, freed from its fæces, being that diuretick salt, I several years ago gave quantities of some to chymists and physicians, from the most of whom I received great thanks, accompanied with the more acceptable accounts of the very happy success they had employed it with, though usually but in a small dose, as from six, eight, or ten grains to a scruple. But this being mentioned only upon the by, I shall proceed to tell you, that since I intimated to you already, that I would mention examples of sounds and tastes, only to illustrate what I have been delivering; I shall now add some instances by way of proof, of the coalition and resulting change of steams in the air. It is easily observable in some nosegays, where the differing flowers happen to be conveniently mixed, that in the smell afforded by it, at a due distance, the odours of the particular flowers are not perceived, but the organ is affected by their joint action, which makes on it a confused, but delightful impression. And so, when in a ball of pomander, or a perfumed skin, musk, and amber, and civet, and other sweets, are skilfully mixed, the coalition of the distinct effluvia of the ingredients, that associate themselves in their passage through the air, produce in the sensory one grateful perfume, resulting from all those odours. But if you take spirit of fermented urine, and spirit of wine, both of them phlegmatick, and mix them together, they will incorporate like wine and water, or any other such liquors, without affording any dry concretions. But if you expose them in a convenient vessel but to the mild heat of a bath, or lamp, the ascending particles will associate themselves, and adhere to the upper part of the glass in the form of a white but tender sublimate, consisting both of urinous and vinous spirits, associated into a mixture, which differs from either of the liquors, not only in consistence, taste, and smell, but in some considerable operations performable by this odd mixture; which this is not the place to take further notice of. And if spirit of salt, and spirit of nitre be, by distillation, elevated in the form of fumes, so ordered, as to convene into one liquor in the receiver, this liquor will readily dissolve crude gold, though neither the spirit of nitre alone, nor that of salt, would do so.

And that you may have an ocular proof of the possibility of the distinctness and subsequent commixture of the steams in the air, I shall now add an experiment, which I long since devised for that purpose, and which I soon after shewed to many curious persons, most of whom appeared somewhat surprized at it. The experiment was, that I took two small phials, the one filled with spirit of salt, but not very strong, the other with spirit of fermented urine, or of sal armoniack very well rectified: these phials being placed at some distance, and not being stopp'd, each liquor afforded its own smell, at a pretty distance, by the steams it emitted into the air, but yet these steams were invisible. But when these phials (which should be of the same size) came to be approached very near to each other, though not so as to touch; as when the two liquors are put together in the form of liquors, they will notably act upon one another; so their respective effluvia meeting in the air, would, answerably to the littleness of their bulk, do the like, and by their mutual occurrences, become manifestly visible, and appear moving in the air like a little portion of smoke, or of a mist, which would quickly cease, if either of the phials were removed half a foot, or a foot from the other. And I remember, that, to add to the oddness of the phenomenon, I sometimes made a drop of the spirit of salt hang at the bottom of a little stick of glass, or some other convenient body, and held this drop thus suspended in the orifice of a phial, that had spirit of sal armoniack in it, and was furnished with a somewhat long neck; for by this means,

happened as I expected, that the ascending urinous particles, though invisible before, invading plentifully the acid ones of the drop, produced a notable smoke, which, if the drop were held a little above the neck of the glass, would most commonly fly upwards to the height of a foot, or half a yard: but if the drop were held somewhat deep within the cavity of the neck, a good part of the produced smoke would oftentimes fall into the cavity of the phial, which was left in great part empty, sometimes in the form of drops, but usually in the form of a slender and somewhat winding stream, of a white colour, that seemed to flow down just like a liquor from the depending drop, till it had reached the spirit of sal armoniack; upon whose surface it would spread itself like a mist. But this only upon the by. As for the main experiment itself, it may be, as I have found, successfully tried with other liquors than these; but it is not necessary, in this place, to give an account of such trials; though perhaps, if I had leisure, it might be worth while to consider, whether these coalitions of differing sorts of steams in the air, and the changes resulting thence of their particular precedent quantities, may not assist us to investigate the causes of divers sudden clouds and mists, and some other meteorological phenomena, and also of divers changes that happen in the air, in reference to the coming in and ceasing of several either epidemical, or contagious diseases, and particularly the plague, that seem to depend upon some occult temperature and alterations of the air, which may be copiously impregnated by the differing subterranean (not to add here, fideral) effluvioms, that not unfrequently ascend into it, or otherwise invade it, with pestiferous, or other morbidick corpuscles, and sometimes with others of a contrary nature, and sometimes too, perhaps, neither the one sort of steams, which may be supposed to have imbued the air, is in itself deleterious; nor the other salutary, but becomes so upon their casual coalition in the air. You will perhaps think this conjecture of the resulant of pestilential steams the less improbable, if I here add that odd observation, which was frequently made in the formerly mentioned plague at Nimeguen, by a physician so judicious as Diemerbroek, whose words are these; *Illud notatu dignum sepius observavimus, nempe in illis aedibus, in quibus nulla adhuc pestis erat, si linteamina, sordida aquâ & sapone nostrate (ut in Belgio moris est) illic lavarentur, eo ipso die, vel interdum postridie, duos tres-ve simul peste correptos fuisse, ipsique ægri testabantur fatorem aquæ saponatæ illis primam & maximam alterationem intulisse. Hoc ipsum quoque in meo ipsius hospitio infelix experientia docuit, in quo post lota linteamina statim gravem alterationem perceperunt plerique domestici, & proxime sequenti nocte tres peste correpti, ac brevi post mortuæ fuere.* I omit the instances he further sets down to confirm this odd phenomenon, of which, though perhaps some other cause may be divided, yet, that I lately assigned, seems at least a probable one, if not the most probable; since, as it is manifest by daily experience, that the smell occasioned by the washing of foul linen with soap commonly used in the Netherlands, produces not the plague; so, by our learned author's observation it appears, either, that there were not yet any pestilential effluvioms in the air of those places, which, on the occasions of those washings, became infected, or at least, that by the addition of the fetid effluvia of the soapy water, those morbidick particles, that were dispersed through the air before, had not the power to introduce a malignant constitution into the air, and to act as truly pestilential, till they were enabled to do so, by being associated with the ill-scented effluvia of the soap.

WHETHER also salutary, and, if I may so call them, alexipharmical corpuscles may not be produced in the air by coalition, might be very well worth our enquiry: especially if we had a competent historical account of the yearly ceasing of the plague at *Grand Cayro*. For, as I have elsewhere noted out of the learned *Prosper Alpinus*, who practised physick there, and, as I have also been informed by some of my acquaintance, who

Tract. de  
Peste,  
Lib. II.  
cap. 3.

visited



visited that vast city, that almost in the midst of summer, as soon as the river begins to rise\*, the plague has its malignity suddenly checked, even as to those that are already infected, and soon after ceases; so if other circumstances contradict not, one might guess, that this strange phenomenon may be chiefly occasioned by some nitrous, or other corpuscles, that accompany the overflowing Nile, and by associating themselves with what *Hippocrates* somewhere calls *voresq; ἀνορροίας*, disable them to produce their wonted pernicious effects. To which hypothesis suits well what is delivered by more than one traveller into *Egypt*, and more particular by our ingenious countryman Mr. *George Sandys*, who not only takes notice, that about the time of the overflowing of *Nilus*, whose abounding with nitre has been observed even by the antients, there is a certain moistening emanation diffused thorough the air. To prove, says he†, speaking of the overflowing of *Nilus*, that it proceedeth from a natural cause, this one, though strange, yet true experiment will suffice. Take of the earth of *Egypt* adjoining to the river, and preserve it carefully, that it neither come to be wet nor wasted, weigh it daily, and you shall find it neither more nor less heavy until the seventeenth of *June*, at which day it beginneth to grow more ponderous, and augmenteth with the augmentation of the river, whereby they have an infallible knowledge of the state of the deluge, proceeding without doubt from the humidity of the air, which having a recourse through all passable places, and mixing therewith, increaseth the same, as it increaseth in moisture.

THAT these sanative steams perform their effects merely because they are moist, I presume naturalists will scarce pretend; but that they may be of such a nature, as by their coalition with the morbifick corpuscles, to encrease their bulk and alter their figure, or precipitate them out of the air, or clog their agility, or pervert their motions, and, in a word, destroy all, or some at least, of those mechanical affections, which made those corpuscles pestilential: that, I say, these antidotal vapours (if I may so call them) may have these effects upon those that formerly were morbifick, and that so there may result from the association of two sorts of particles, whereof one was of a highly noxious nature, a harmless mixture, might here be made probable by several things; but that I hope what I have lately recited about the coalitions of the effluvia of spirit of salt, and of urine (liquors known to be highly contrary to each other) is not already forgotten by you.

AND the experiment, with which I am to conclude this essay, will, perhaps, make you think it possible, that the pestiferous steams, that have already passed out of the air, and invaded, but not too much vitiated, the bodies of men, may have their malignity much debilitated by the supervening of these antidotal particles. For in that experiment you will find, that the steams emitted into the air from the liquor there described, though that were actually cold, were able to reach, and manifestly to operate (and that probably by way of precipitation) upon corpuscles, that were fenced from them by the interposition of other bodies, not more porous than those of living men. Whether the fume or sulphur, which by many is extolled to prevent the infection of the air, do, by its acid, or other particles, disarm, if I may so speak, the pestilential ones, I have not now time to enquire: no more than whether in *Ireland*, and some few other countries, that breed or brook no poisonous animals, that hostility may proceed, at least, in great part, from

\* The plague, which here miserably rageth, upon the first of the flood doth instantly cease, inasmuch, as when five hundred die at *Cairo* the day before, which is nothing rare (for the sound keep company with the sick, holding death fatal, and, to avoid them, irreligion) not one doth die the day following, says Mr. *Sandys* in his travels, Lib. II.

† Mr. *Sandys* in the book above-cited.

the peculiar nature of the soil, which both from its superficial or deeper parts, constantly supplies the air with corpuscles destructive to venomous animals. And some other particulars, that may be pertinently enough considered here, you may find treated on in other papers. And therefore at present I shall only intimate in a word, that having purposely made a visible and lasting stain on a solid body barely by cold effluvia, I did, by the invisible and cold steams of another body, make, in two or three minutes, a visible change in the colour of that stain.

AND as for the other part of the conjecture, viz. that meteo's may sometimes be produced by the occurrences of subterranean effluvia, some of them of one determinate nature, and some of another, I think I could, to countenance it, give you divers instances of the plentiful impregnation of the air at some times, and in some places, with steams of very differing natures, and such as are not so likely to be attracted by the heat of the sun, as to be sent up from the subterranean regions, and sometimes from minerals themselves. But for instances of this kind, I shall, for brevity sake, refer you to another paper\*, where I have purposely treated of this subject, and particularly shewn, that though usually the effluvioms that come from under-ground, are ill-scented, yet they are not always so; and also, that sulphureous exhalations, even from cold, and, for the most part, aqueous liquors, may retain their determinate nature in the air, and act accordingly upon solid bodies themselves, to whose constitution those effluvia chance to be proportionate.

BUT one memorable story, not mentioned in that discourse, is too much to our present purpose to be here omitted, especially having met with it in so approved an author as the experienced *Agricola*, who having mentioned out of antient historians the raining of white and red liquors, which they took (erroneously, I doubt not) for milk and blood, subjoins, *† Ut autem majorem fidem habeamus annalium monumentis facit res illa decantata, quæ patrum memoriâ* (in another place he specifies the year our Lord) *in Suevia accidit; aer enim ille stillavit guttas, quæ lineas vestes crucibus rubris quasi sanguineis imbebant.* Which I the rather mention, because it does not only prove what I alledge it for; but may keep what is lately and very credibly reported to have happened in divers places of the kingdom of *Naples*, soon after the fiery eruption of *Vesuvius*, from being judged a phenomenon either altogether fabulous (as doubtless many have thought it) or a prodigy without all example, as is presumed, even by those that think it not miraculous. And to this I add, that it will be the less improbable, that the more agile corpuscles of subterranean salts, sulphurs, and bitumens, may be raised into the air, and keep distinct natures there, if so fixed a body, as common earth itself, can be brought to swim in the air. And yet of this the worthy writer newly quoted gives us, besides what annals relate, this testimony upon his own knowledge: *‡ Certè hic Kempniti undecimum abhinc annum mense Septembri effluerunt imbres, sic cum terra lutea commisti, ut eâ passim plateas scilicet stratas viderem conperfas.*

AND to shew you, that in some cases the particles even of vegetable bodies may not so soon perish in the air, as they vanish there, but may retain distinct natures at a greater distance, than one would think, from the bodies, that copiously emit them; I shall add, that having desired an ingenious gentleman, that went on a considerable employment to the *East-Indies*, to make some observations for me in his voyage; he sent me, among other things, this remark: that having sailed along the coast of *Ceylon* (famous for cinna-

\* An Essay of Subterranean Exhalations.

† *Agric. de nat. eorum, quæ effluunt è terra*, Lib. XII. pag. 236.

‡ *Agric. de nat. eorum, quæ è terra effluunt*, Lib. XII. pag. 263.



mon-trees and well-scented gums) though they coasted it almost a whole day, the wind, that then chanced to blow from the shore, brought them a manifestly odoriferous air from the island, though they kept off many miles (perhaps twenty or twenty-five from the shore. Nor should this be thought incredible, because the diffusion seems so disproportionate to that of other bodies dissolved by fluids; as, for instance, though salt be an active body, and resolvable into abundance of minute particles, yet one part of salt will scarce be tastable in an hundred parts of water. For sensibly to affect so gross an organ, as that of our taste, there is usually required in sapid particles a bigness far exceeding that which is necessary to the making bodies fit objects for the sense of smelling, and which is here mainly to be considered, there is a great difference between the power a body has to impregnate so thin and fine a fluid as air, whose parts are so rare and lax, and that, which it has to impregnate liquors, such as water or wine, whose parts are so constipated as to make it not only visible and tangible, but ponderous. On which occasion I remember, that having had a curiosity to try how far a sapid body could be diluted, without ceasing to be so, I found, by trial, that one drop of good chymical, and, as artists call it, essential oil of cinnamon, being duly mixed, by the help of sugar, with wine, retained the determinate taste of cinnamon, though it were diffused into near a quart of wine. So that, making a moderate estimate, I concluded, that upon the common supposition, according to which a drop is reckoned for a grain, one part of oil had given the specifick taste of the spice it was drawn from, to near fourteen thousand parts of wine. By comparing which experiment with what I noted about the proportion of salt requisite to make water taste of it, you will easily perceive that there may be a very great difference, in point of diffusiveness, between the little particles that make bodies sapid; which may serve to confirm both some part of the first chapter of the foregoing essay of the subtilty of effluvia, and what I was lately saying, to shew it possible that antimonial glass might impart store of steams to the emetick wine, without appearing, upon common scales, to have lost of its weight; since we see that one drop of so light a body as oil may communicate, not insensible effluvia, but tastable corpuscles, to near a quart of liquor. But this is not all for which I mention our experiment; for I must now add, that besides the almost innumerable sapid parts of a spicy drop communicated to the wine, it thence diffused a vast number of odorous particles into the air, which both I and others perceived to be imbued with the distinct scent of cinnamon, and which, perhaps the liquor would have been found able to have aromatized for I know not how long a time, if I had had leisure to prosecute the observation.

## C H A P. V.

THE third and last way I shall mention of shewing the determinate nature of effluvioms, is to be taken from the consideration of their effects upon other bodies than the organs of our senses (for of their operations upon these, we have already spoken in the foregoing chapter). For the effects that certain bodies produce on others by their effluvioms, being constant and determinate, and oftentimes very different from those which other agents, by their emissions, work upon the same, and other subjects, the distinct nature of the corpuscles emitted may be thence sufficiently gathered.

WE may, from the foregoing tract of the subtilty of effluvia, borrow some instances very pertinent to this place. For the temporary benumbedness or stupefaction, for example, produced in the fisherman's foot by the \* effluvia of the fish *Amoreatim*, men-

\* See the Essay of the Subtilty of Effluvioms, Chap. IV.

tioned by the ingenious *Piso*, manifests, that those stupefying emanations retained a peculiar and venomous nature during their whole passage through the shoe, stocking, and skin, interposed betwixt the fish and the nervous part of the foot benumbed by it. And though there are very few other bodies in the world that are minute enough to pass through the pores of glass, it is apparent, by the experiment there recited of the oblong iron hermetically sealed up in a glass-pipe, that the magnetical effluvia of the earth may retain their peculiar and wonderful nature, in a smallness that qualifies them to pass freely through the pores of glass itself.

BUT that I may neither repeat what you have already met with in the foregoing tract, nor anticipate what I have to say in the next, I will employ in this chapter some instances that may be spared from both.

THAT divers bodies of a venomous nature may exercise some such operations upon others by their effluvioms transmitted through the air, as they are wont to do in their gross substance, is a truth, whereof, though I have not met with many, yet I have met with some examples among physicians.

Lib. VI. THE learned *Sennertus* observes, as a known thing, that the apprentices of apothecaries  
part 7.  
cap. 1. have been-cast into profound sleeps, when in distilling opiat and hypnotick liquors they have received in at their nostrils the vapours exhaling from those bodies.

It is recorded by the † writers about poisons, that the root and juice of mandragora casts those that take it into a deep sopor, not unlike a lethargy. And though the apples of the same plant be thought to be much less malignant; yet *Levinus Lemnius* relates that it happened to him more than once, that having laid some mandrake-apples in his study, he was, by their steams, made so sleepy, that he could hardly recover himself; but the apples being taken away he regained alacrity, and threw off all drowsiness.

Lib. III. AMONG all poisons, there is scarce any whose phenomena are, in my opinion, more  
Acutor.  
Morbor. strange than those that proceed from a mad dog; and yet even this poison, which seems to require corpuscles of so odd and determinate a nature, is recorded by physicians to have been conveyed by exhalations. *Aretaeus* writes, as a learned modern quotes him, *Quòd à rabido cane, qui in faciem, dum spiritus adducitur, tantummodò inspiraverit, & nullo modo momorderit, in rabiem homo agatur.* And as there are relations, among physicians, of animals, that have become rabiosi by having eaten of the parts or excrements of rabid animals; so *Celsus Aurelianus*, who writes, that some have been made to run mad, not by being bitten, but wounded only with the claws of a mad dog, tells us also of a man that fell into a hydrophobia (which is wont to be a high degree of the rabies, and by some of the ancients writers was employed to signify that disease) without being bitten by a mad dog, but infected *solo odore ex rabido cane attracto*. By which odours, in this and other narratives of poisons, I understand not a bare scholastick species, but a swarm of effluvia, which most commonly are all, or at least some of them odorous. And though it may justly seem strange to many, that the venom of a mad dog should be communicated otherwise than by biting, which is supposed to be the only way he can infect by, it may appear less improbable, because *Matthaeus de Gradibus* names a person, who, he says, proved infected after many days, by only having put his hand into the mouth of a mad dog, who did not bite him. And the formerly mentioned *Matthiæolus* relates that he saw two that were made rabid without any wound, by the slubber of a mad dog, with which they had the misfortune to be besmeared.

Lib. VI. SENNERTUS himself affirms of a painter of his acquaintance, that when he had opened  
part 6.  
cap. 2. a box, in which he had long kept included realgar, a noxious mineral, sometimes used

† In Explicatione Herbarum Biblicarum, cap. 2.



by painters, and not unknown to chymists, and had unfortunately snuffed in the steams of it, he was seized with a giddiness in his head and fainting fits, his whole face also swelling, though by taking of antidotes he escaped the danger.

DIVERS other examples we have met with in the writings of physicians, which I forbear to add to these, because, I confess, I very much doubt the truth of them, though the deliverers of some of them be men of note. But the probability of most of the things already cited out of credible authors may be strengthened by what I shall now subjoin, as a further proof of the distinct nature of effluvia; of which it will be a very considerable proof, if medicines, which are of a milder and more familiar nature and operation than poisons, shall yet be able in some cases to retain, in their invisible particles swimming in the air, the same, though not so great power of purging, which is known to belong to them when their gross body is taken in at the mouth. Of this I have elsewhere, on another occasion, given some examples. To which I shall now add, that I know a doctor of physick that is usually purged by the odours or exhalations of a certain electuary, whose cathartick operation, when it is taken in substance, is wont to be but languid. And another doctor of my acquaintance causing good store of the root of black hellebore to be long pounded in a mortar, most of those that were in the room, and especially the party that pounded it, were thereby purged, and some of them strongly enough. And the learned *Sennertus*, somewhere, affirms that some will be purged by the very odour of colocynthis. And it is not to be passed by unregarded, that in the cases I have alledged, exhalations that are endowed with occult qualities (for those of cathartick medicines are reckoned among such) ascend into the air, without being forced from the bodies they belonged to by an external heat.

And if I would in this place alledge examples of the operations of such effluvia, as do not pass into the air, but yet operate only by the contact of the external parts of the body, I could give instances, not only of the purgative, but the emetic qualities of some medicines exerted without their being taken in at the mouth, or injected with instruments.

THERE are also other sorts of examples than those hitherto mentioned, that argue a determinate nature in the effluxions of some bodies emitted into the air. Approved writers tell us, that the shadow of a walnut-tree, with the leaves on it, is very hurtful to the head; and some instances they give us of great mischief it has sometimes done. And though the shadow, as such, is not likely to be guilty of such bad effects; yet the effluvia of the neighbouring plant may be noxious enough to the head. For I, that was not at all prepossessed with an opinion that it was so, and therefore, without scruple, resorted to the shade of walnut-trees in a hot country, was, by experience, forced to think it might give others the head-ach, since it did to me, who, thanks be to God, both was, and am still very little subject to that distemper. And this brings into my mind an observation that I have met with among some ingenious travellers into the *West-Indies*, who observe, in general, and, of late, a countryman of our own affirms it in particular, of the poisonous manchinello-tree, that birds will not only forbear to eat of the fruit of venomous plants, but, as to some of them, will not so much as light on the trees; which I therefore mention, because, probably, nature instructs them to avoid such trees by some noxious smell, or other emanation that offends the approaching birds. And I remember, that some of our navigators give it for a rule to those that happen to land in unknown islands, or coasts, that they may venture to eat of those parts of fruits which they can perceive the birds, like kind tasters, to have been pecking at before.

NICOLAUS FLORENTINUS (cited by *Sennertus*) tells us of a certain Lombard, that having in a house, that he named, at *Florence*, burned a great black-spider at the flame of a candle,

candle, so unwarily, that he drew in the steams of it at his nostrils, presently began to be much disordered, and fell into a fainting fit, and, for the whole night, had his heart much disaffected, his pulse being so weak, that one could scarce perceive he had any; though afterwards he was cured by treacle, diamole, and the powder of zedoary mixed together.

AND I remember, that being some years ago in *Ireland*, I gathered a certain plant (peculiar to some parts of that country) which the natives call *Maccu-buy*, because of strange traditions that go about it; the chief of which I found, by trial, not to be true; but yet being satisfied that its operations were odd, and violent enough, I was willing to gratify the chief physician of the country, who was desirous I should propose to him some ways of correcting it; and whilst I was speaking of one, that required the pounding of it, he told me, on that occasion, that intending to make an extract of it with vinegar, he caused his man to beat it well in a mortar, which the man soon repented he had begun to do; and the doctor himself, though at a pretty distance off, was so wrought upon by the corpuscles that issued out into the air, that his head, and particularly his face, swelled to an enormous and disfiguring bulk, and continued tumid for no inconsiderable time after.

I HAVE not leisure to subjoin many more instances, to shew the determinate nature of effluvioms, small enough to wander through the air; nor perhaps will it be necessary, if you please but to consider these two things. The first, that many odoriferous bodies, as amber, musk, civet, &c. as they will, by the adhesion of their whole substance, perfume skins, linen, &c. so they will, in time, perfume some bodies disposed to admit their action, though kept at a distance from them. And the other is, that in pestilential fevers, and divers other contagious sicknesses, as the plague, small-pox, or measles, the same determinate disease is communicable to sound persons, not only by the immediate contact of the infected party, but without it, by the contagious steams that exhale from his body into the air. And having said this, and desired you to reflect upon it, I shall conclude this chapter with an experiment, that, possibly, will not a little confirm a great part of it.

CONSIDERING then with myself how I might best devise a way of shewing to the very eye, that effluvia, elevated without the help of heat, and wandering in the air, may both retain their own nature, and, upon determinate bodies, produce effects, that a vulgar philosopher would ascribe to occult qualities; I remembered, that I had found by trials (made to other purposes) that volatile and sulphureous salts would so work upon some acid ones, sublimed with mercury, as to produce an odd diversity of colours, but chiefly an inky one; on which account I judged it likely that my aim would be answered by the following experiment.

I TOOK an ounce, or better, of such a volatile tincture of sulphur, as I have elsewhere \* taught you to make of quick-lime, sulphur, and sal-armoniack, and stopped it up in a phial capable of containing at least twice as much; then taking a paper whereon something had been written with invisible ink, I laid it down six inches off of the phial, which being unstopped, began, upon the access of the fire, to emit white fumes into it, and by these, what was written upon the paper, notwithstanding its distance from the liquor, quickly became very legible, though not quite so suddenly, as if a paper, written with the same clear liquor, were held at the like distance directly over the orifice of the phial. And having caused several pieces of clean paper to be written on, with a new pen

\* The liquor here mentioned is, for the main, the same with that described by the author in his book of Colours.



dipped in the clear solution of sublimate, made in water, it was pleasant to see, how divers of the letters of several of these papers, being placed within some convenient distance of the phial, would be made plainly legible, and some of them more, some less blackish, according to their distances from the smoking liquor, and other circumstances. But it was more surprizing to see, that when I held or laid some of these papers, though with the written side upwards, just upon, or over the orifice of the phial, though the contained liquor did not, by some inches, reach so high, yet the latent letters would become not only legible, but conspicuous, in about a quarter of a minute of an hour, measured by a good watch fit for the purpose, as more than one trial assured me. And as it may be observed, that in some circumstances the smoking liquor, and the solution of sublimate, will make an odd precipitate, almost of a silverish colour, so in one or two of our trials, we found a like colour produced by the steams of that liquor, in some of the colourless ink. Nor is it so necessary to employ a visibly smoking liquor for the denigrating of invisible ink at a distance. For I have, to that purpose, with good success, though not equal to that I have recited, employed a couple of liquors, wherein there was neither sulphur, nor sal armoniack, nor sublimate. What other trials I made with our volatile tincture of sulphur it is not necessary here to relate; only one experiment, which you will possibly think odd enough, I shall not omit; because it will not only confirm the precedent trials, but also much of the foregoing essay, by shewing the great subtilty, and penetrating power of effluviūms, that seem rather to issue out very faintly, than to be darted out with any briskness.

CAUSING then something to be written with dissolved sublimate upon a piece of paper, we folded the paper with the written side inwards, and then inclosed this in the midst of six sheets of paper laid one upon another, not placed one within another, and folded up in the form of an ordinary letter or packet, to be sealed, that the edges of the inclosing paper, being inserted one within the other, the fumes might not get into this written paper, but by penetrating through the leaves themselves; this done, that side of the packet on which there was no commissure, and on which, were it to be sent away, the superscription should be written, was laid upon the orifice of the phial, which (as was before intimated) was some inches higher than the surface of the liquor, and left there about ten minutes; after which, taking off the folded papers, and opening them, we found that the steams had pervaded all the leaves, in which the written paper had been inclosed. For, though the leaves did not appear stained or altered, yet the formerly latent characters appeared conspicuous. I have not time to discourse, whether, and how far this experiment may assist us to explain some odd effects of thunder, or of that strange phenomenon (glanced at in the foregoing chapter) which is said to have happened lately in the kingdom of *Naples*, after the great eruption of *Vesuvius*, which is said to have been followed by the appearing of the crosses formerly mentioned, some of which have been found on the innermost parts of linen that had been carefully folded up. But of these, and the like things, I say, I have now no time to discourse, whether any thing derivable from our experiment may be pertinently applied to their explication. For which reason I shall add no more, than that afterwards, for further trial, we took a printed book that chanced to be at hand, and which we judged the fittest for our purpose, because the leaves being broad, they might the better preserve a small paper to be placed in the midst of them, from being accessible to the exhalations side-wise, and having put the designed paper into this book, and held it to the orifice of the phial, though there were no less than twelve leaves between them, yet those letters that happened to be the most rightly placed, were made inky in the short

space of three minutes, at the utmost; though this liquor had been so long kept; and so often unstopped to try conclusions with it, that it had probably lost a good part of the most spirituous and piercing particles.

# NEW EXPERIMENTS

TO MAKE

## FIRE AND FLAME

STABLE AND PONDERABLE.

A PREFACE; shewing the Motive, Design, and Parts of the ensuing TRACT.

**T**HE inducements which put me upon the attempt expressed in the title of this essay, were chiefly these:

FIRST, I considered, that the interstellar part of the universe, consisting of air and æther, or fluids analogous to one of them, is diaphanous; and that the æther is, as it were, a vast ocean, wherein the luminous globes, that here and there, like fishes, swim by their own motion, or like bodies in whirlpools are carried about by the ambient, are but very thinly dispersed, and consequently, that the proportion that the fixed stars and planetary bodies bear to the diaphanous part of the world, is exceeding small, and scarce considerable, though we should admit the sun and fixed stars to be opacous bodies, upon the account of their terminating our sight; which dissident expression I employ, because I have elsewhere shewn by two or three experiments, purposely devised, that a body may appear opacous to our eyes, and yet allow free passage to the beams of light.

I FURTHER considered, that there being so vast a disproportion between the diaphanous part of the world, and the globes, about which it is every way diffused, and with which it is sometimes in great portions mingled, as in the water, which, together with the earth, makes up the globe we inhabit; and the nature of a diaphanous body's being such, that when the sun, or any other luminous body illustrates them, that which we call light does so penetrate, and mix itself *per minima*, with them, that there is no sensible part of the transparent body unenlightened; I thought it worth the inquiry, whether  
a thing



a thing so vastly diffused as light is, were something corporeal, or not? and, whether, in case it be, it may be subjected to some other of our senses, besides our sight, whereby we may examine, whether it hath any affinity with other corporeal beings that we are acquainted with here below?

I DID not all this while forget that the Peripateticks make light a mere quality, and that *Cartesius* ingeniously endeavours to explicate it, by a modification of motion in an æthereal matter; but I remembered too, that the Atomists of old, and of late the learned *Gassendus*, and many other philosophers, assert light to be corporeal; and, that some years since, though I declined to pass my judgment about the question, yet I had employed arguments that appeared plausible enough to shew that it was not absurd to suppose that the sun, which is the fixed star most known to us, might be a fiery body. And therefore doubting, whether the corporeity of light would be in haste determined by mere ratiocinations, I thought it very well worth the endeavouring to try, whether I could do any thing towards clearing the dispute of it by experiments; especially being persuaded, that, though such an attempt should be ineffectual, it would but leave the controversy in its former state, without prejudicing either of the contending hypotheses; and yet, if it should prove successful, the consequences of it would be very great and useful towards the explicating of divers phenomena in divers parts of natural philosophy, as in chymistry, botanicks, and (if there be any such) the allowable part of astrology. (Nor perhaps would it be impossible, by the help of slight theoretical alterations, to reconcile the experiments I designed to either of the above-mentioned hypotheses, and so, as to the explication of light, to one another.)

To compass, then, what I aimed at, I thought it was fit, in the first place, to try what I could do by the union of the sun-beams, they being on all hands confessed to be portions (as I may so speak) of true and celestial light; and then I thought fit to try what could be obtained from flame; not only, because that is acknowledged to be a luminary, but because I hoped the difficulties I foresaw in the other trials might be, in some measure, avoided in those made with flame; and if both sorts of them should succeed, the latter and former would serve to confirm each other. According to the method I proposed of handling these two subjects, I should begin with some account of what I attempted to perform in the sun-beams. But the truth is, that when I chanced to fall upon the inquiry that occasioned this paper, besides that the time of the year itself was not over-favourable, the weather proved so extraordinary dark and unseasonable, that it was wondered at; so that, though I was furnished with good burning-glasses, and did several times begin to make trials upon divers bodies, as lead, quicksilver, antimony, &c. yet the frequent interposition of clouds and mists did so disavour my attempts, that, however they were not all alike defeated, yet I could not prosecute the greatest part of them to my own satisfaction. And therefore, being unwilling to build on them as yet, I shall reserve an account of them for another opportunity; and now proceed to the mention of that sort of experiments, which, depending less on casualties, it was more in my power to bring to an issue.

I KNOW I might have saved both you and myself some time and pains, by omitting several of these trials, and by a more compendious way of delivering the rest. But I rather chose the course I have taken; partly, because the novelty and improbabilities of the truth I deliver, seem to require that it be made out by a good number of trials; partly, because I thought it might not be altogether useless to you, and your friends, to see upon what inducements the several steps were made in this inquiry; partly, because I was willing to contribute something towards the history, that now, perhaps, will be thought fit to be made of the increment or decrement that particular bodies may receive

by being exposed to the fire; and partly, in fine, because the incongruity of the doctrine here asserted to the opinions of the schools, and the general prepossessions of mankind, made me think it fit, by a considerable variety, as well as number of experiments, to obviate, as far as may be, the differing objections and evasions wherewith a truth so paradoxical may expect to be encountered.

## NEW EXPERIMENTS, &c.

**T**HOUGH there be among the following trials, a diversity, that invites me, as to rank them into four or five differing sorts, so to assign them as many distinct sections; yet, for the conveniency of making the references there will be occasion to make betwixt them, I shall wave the distinction, and set them down in one continued series.

AND because I am willing to comply with my haste, as well as to deal frankly, and without ceremony, with you, I shall venture to subjoin the naked transcripts of my experiments, as I had in an artless manner set them down, with many others, for my own remembrance, among my *adversaria*, without so much as retrenching some circumstances that relate leis to my present argument than to some other purposes.

I SHALL then begin with the mention of a couple of experiments, which, though they might conveniently enough be referred to another paper, yet I shall here set them down, because it seems very proper to endeavour to shew, in the first place, that flame itself may be, as it were, incorporated with close and solid bodies, so as to increase their bulk and weight.

### TRIALS of the First Sort.

#### EXPERIMENT I.

[A **P**IECE of copper-plate not near so thick as a half-crown, and weighing two drachms and twenty-five grains, was so placed with its broad part horizontal, in a crucible, whose bottom had a little hole in it for fumes to get out at, that it could not be removed from its position, nor be easily made to drop down or lose its level to the horizon, though the crucible were turned upside down; then about an ounce and half of common sulphur being put into a taller and broader crucible, that wherein the copper stuck was inverted into the orifice of it, that the sulphur being kindled, the flame, but not the melted brimstone, in substance, might reach the plate, and have some vent beyond it at the above-mentioned hole. This brimstone burned about two hours, in which time it seemed all to have been resolved into flame, no flowers of sulphur appearing to have sublimed into the inside of the upper crucible; and though the copper-plate were at a considerable



considerable distance from the ignited sulphur, yet the flame seemed to have really penetrated it, and to have made it visibly swell or grow thicker; which appeared to be done by a real accession of substance; since, after we had wiped off some little adhering sordes, and with them divers particles of copper that stuck close to them, the plate was found to weigh near two-and-thirty grains more than at first, and consequently to have increased its former weight by above a fifth part.]

## EXPERIMENT II.

[HAVING, by refining one ounce of sterling silver with saltpetre, according to our way, reduced it to seven drachms or somewhat less; we took a piece of the thus purified silver that weighed one drachm wanting two grains, and having ordered it as the copper-plate had been in the former experiment, after the flame of above one ounce and a quarter of sulphur (that quantity chancing to be suitable to the capacity of the crucible) had, for about an hour and a half, beat upon it, the silver-plate seemed to the eye somewhat swelled, and the lower surface of it, that was next the flame, was brought to a great smoothness, the weight being increased to one drachm five grains and three quarters; which increase of weight falling so short of that, which was gained by the copper, I leave it to you to consider, whether the difference may be attributed to the closeness and compactness of the silver, argued by its being heavier in specie than copper; or to the greater congruity of the pores of copper to be wrought on by the fiery menstruum; or to some other cause.]

If you should here ask me, by what rational inducements I could be led to entertain so extravagant an expectation, as, that such a light and subtle body as flame should be able to give an augmentation of weight to such ponderous bodies as minerals and metals; I shall now, to avoid making anticipations here, or needless repetitions hereafter, return you only this answer; that the expectation you wonder at may justly be entertained upon the same, or such like inducements, as you may easily discover in another paper, intitled *Corollarium Paradoxum*. For, supposing, upon the grounds there laid, that flame may act upon some bodies as a menstruum, it seems no way incredible, that, as almost all other menstrooms, so flame should have some of its own particles united with those of the bodies exposed to its action; and the generality of those particles being (as it is shewn in the paradox about the fewel of flames) either saline, or of some such piercing and terrestrial nature, it is no wonder, that being wedged into the pores, or being brought to adhere very fast to the little parts of the bodies exposed to their action, the accession of so many little bodies, that want not gravity, should, because of their multitude, be considerable upon a balance, whereon one or two, or but few of these corpuscles, would have no visible effect.

I could here, if it were expedient, mention some odd scruples about the preceding experiments, and some also of the subsequent; but lest you should, with some other of my friends, upbraid me with being too jealous and sceptical, I will not trouble you with them, but proceed to the next sort of trials, wherein, though the matter were not always manifestly beaten on by a shining flame; yet it was wrought on by that, which would be called flame, by those who take not that word strictly, but in a latitude, and which this igneous substance may more properly be stiled, than it can be called common fire, this being visibly harboured in burning coals, or other gross materials, from which our metals were fenced. And I have elsewhere shewn, by experiment, that visibility is not in all cases necessary to actual flame, particularly when the eye receives a predominant impression from another light.

TRIALS;

## TRIALS of the Second Sort.

## EXPERIMENT III.

[INTO a crucible whose sides had been purposely taken down to make it very shallow, was put one ounce of copper-plates; and this being put into our cupelling-furnace, and kept there two hours, and then being taken out, we weighed the copper (which had not been melted) having first blown off all the ashes, and we found it to weigh one ounce and thirty grains.]

## EXPERIMENT IV.

[SUPPOSING that copper, being reduced to filings, and thereby gaining more of superficies in proportion to its bulk, would be more exposed to the action of the fire than when it is in plates, as it was formerly, we took an ounce of that metal in filings, and putting them upon a very shallow crucible, and under a muffler, we kept them there about three hours (whilst other things that required so long a time were cupelling); and afterwards taking them off, we found them of a very dark colour, not melted, but caked together in one lump, and increased in weight (the ashes and dust being blown off) no less than about forty-nine grains. Part of which increment, above that obtained by the copper-plates in the former experiment, may not improbably be due to the longer time, that in this experiment the filed copper was kept in the fire.]

## EXPERIMENT V.

[BEING willing to see, whether calcined hartshorn, that I did not find easy to be wrought on by corrosive menstruums, would retain any thing of the flame, or fire, to which it should be exposed; we weighed out one ounce of small lumps of hartshorn, that had been burnt till they appeared white, and having put them into a crucible, and kept them in a cupelling-furnace for two hours, whilst some metals were driving off there by the violence of the fire; we found, that when they were taken out, they had lost six or seven grains of their former weight; perhaps either because, notwithstanding the external whiteness of the lumps, the internal parts of some of them might not be so exquisitely calcined, but retain some oleaginous or other volatile substance; or because, having omitted to ignite them well before they were weighed, they may have since their first calcination imbibed some moist particles of the air. Which conjecture seemed the likelier, because having kept them a while in the scales they were weighed in, they did within two or three hours make it somewhat preponderate. On which occasion I shall add, that, at the same time, with the hartshorn we put in one ounce of well-heated brick, and kept that likewise in the furnace for above two hours; at the end of which, weighing it whilst it continued hot, we did not find it to have either sensibly got or lost; but, some time after it seemed upon the balance to have imbibed some, though but very little moisture from the air.]



## EXPERIMENT VI.

[UPON a good cupel we put one ounce of English tin of the better sort, and having placed it in the furnace under the muffler, though it presently melted, yet it did not forsake its place, but remained upon the concave surface of the cupel, till at the end of about two hours, it appeared to have been well calcined; and then being taken out and weighed by itself, the ounce of metal was found to have gained no less than a drachm.]

## EXPERIMENT VII.

[AN ounce of lead was put upon the cupel, made of calcined hartshorn, and placed under the muffler, after that the cupel was first made hot, and then weighed. This lead did not enter into the cupel, but was turned into a pretty kind of litharge on the top of it, and broke the cupel, whereby some part of the cupel was lost in the furnace, and yet the rest, together with the litharge, weighed seven grains more than the ounce of lead and the heated cupel did, when they were put in.]

BUT, because, though this trial shewed that some weight was gained either by the metal or cupel, or both, yet it did not by this appear what either of them acquired; it seemed fit to subjoin a further trial.

## EXPERIMENT VIII.

[WE took a cupel about two ounces in weight, made of about ten parts of bone-ashes, and one of charcoal-ashes, made up together with ale. This was by itself put in a cupelling-furnace, under a muffler; and the laborant, well-versed in weighing, was ordered to take it out, when it was thoroughly and highly heated, and to weigh it whilst it was in that condition, I being then present; this being done, it was forthwith placed again under the muffler, where some metalline bodies were cupelling, and kept there for about two hours, at the end of which time it was taken out red-hot, and presently put into the same balance, as before, which was already fastened to a gibbet; where having caused the adhering ashes to be blown off, I found, that whereas, when it was first taken from under the muffler, we had but two ounces and two grains, now the same weight being put into the opposite scale, it had gained very near one-and-twenty grains. And here note, that it was not without some cause, that I was careful to have the cupel weighed red-hot. For I had a suspicion, that, notwithstanding the dryness of the bone, it might receive some little alteration of weight by imbibing some little particles wandering in the air; which suspicion the event justified. For leaving the cupel counterpoised to cool in the balance, in a short time it began sensibly to preponderate; and suffering it to continue there nine or ten hours, till we had occasion to use the balance, I found it at the end of that time to be about three grains heavier than before.]

THIS was not the only trial we made about the augmenting the weight of cupels; but this being the fairest, and exempt from those mischances, from which the other were not altogether free, I shall content myself to have set down this; in the mention of which I thought fit to take notice of the increase of the weight of the cupel after it had lain in the scales, and also that we weighed it at first, whilst it was thoroughly hot, because  
those

those circumstances, as not being suspected, may easily be left unthought on, even by skilful experimenters; and yet the weighing of the cupel, when it had been well nealed, and the not weighing it soon enough after it is taken from the fire, may keep those that shall reiterate this experiment, from making it cautiously and accurately enough. For if the former circumstance be omitted, that which the cupel may seem to have lost of its substance, was nothing but the adventitious moisture of the air; and if the latter circumstance be neglected, the weight it may seem to have gained from the fire, was indeed due to the watery particles of the air. I could wish also that trial were made, whether the success would be the same in cupels made in differing sorts of bone-ashes, and other materials, wont to be employed for that purpose. For that I had not opportunity to do.

#### EXPERIMENT IX.

IRON being a metal, that experience had informed me will more easily be wrought on by fluids that have particles of a saline nature in them, than is commonly believed; it was not unreasonable to expect that flame would have a greater operation on it (especially if it were before-hand reduced to small parts) than on any of the bodies hitherto described. Which supposition will be confirmed by the short ensuing note.

[FOUR drachms of filings of steel being kept two hours on a cupel under a muffer, acquired one drachm six grains and a quarter increase of weight.]

#### EXPERIMENT X.

[A PIECE of silver, refined in our own laboratory, being put upon a cupel under a muffer, and kept there for an hour and half, whilst other things were refining, was taken out and weighed again, and, whereas before it weighed three drachms, thirty-two grains and a quarter, it now weighed, in the same scales, three drachms, thirty-four grains and a half, or but little less.]

FINDING this memorial, among divers others about the weight of bodies exposed to the fire, I thought it not amiss to annex it in this place; though finding it to be but single, I would not have it to be relied on, till further trial have been made, to discover whether it was more than a casual and anomalous experiment; and if the silver had not been refined, I should have suspected that the copper that was blended with it, as it is usually blended with common silver, might have occasioned the increase of weight.

#### P O S T S C R I P T.

SINCE the foregoing experiment was first set down, meeting with an opportunity to reiterate the trial once more, we did it with half an ounce of filings of silver, well refined with lead in our own laboratory, and kept it about three hours upon the cupel; after the end of which time taking it out, we found it to be of a less pleasant colour than it was of before, and melted (though not so perfectly) into a lump, which weighed four drachms and six grains; and yet the success being so odd, and, if it prove constant, of such moment, I could wish the trial were further repeated in differing quantities of the metal.

EXPE-



EXPERIMENT XI.

[We took a drachm of filings of zink or spelter, and having put it upon a cupel under a muffler, we kept it there in a cupelling-fire about three hours (having occasion to continue the cupellation so long for other trials); then taking it off the cupel, we found it to be caked into a brittle and dark coloured lump, which looked as if the filings had been calcined. This being weighed in the same scales gained full six grains, and so a tenth part of its first weight.]

EXPERIMENT XII.

AMONG our various trials upon common metals, we thought fit to make one or two upon a metal brought us from the *East-Indies*, and there called Tutenâg, which name being unknown to our European chymists, I have elsewhere endeavoured to give some account of the metal itself; whence I shall borrow the ensuing note, as directly belonging to our present purpose.

[Two drachms of filings of tutenâg being put upon a cupel, and kept under the muffler for about two hours, the filings were not melted into a lump of metal, but looked as if ceruss and minium, being powdered, had been mingled together; some of the parts appearing distinctly white, and others red: the calx, being put into the balance, appeared to have gained twenty-eight grains and a quarter. Another time the experiment being re-iterated with the like circumstances, we found that two drachms of the filed tutenâg gained the like increase of weight, abating less than one grain.]

So that this Indian metal seems to have gained more in the fire, in proportion to its weight, than any we have hitherto made trial of.

EXPERIMENT XIII.

[BEING desirous to confirm, by a clear experiment, what I elsewhere deliver contrary to the vulgar opinion of those that believe, that in all cupellations almost all the lead that is employed about them, does, together with the baser metals that are to be purged off from the silver or gold, fly away in smoke, as indeed, in some sort of cupellations, a good proportion may be blown off that way; we took two ounces of good lead, and one drachm of filings of copper, and having caused a cupel to be ignited, and nimbly taken out of the furnace, and weighed, whilst it was very hot, it was presently put back, together with the two metals laid on it, into the cupelling-furnace, where having been kept for about two hours, it was taken out again, and it was found, according to what (as I elsewhere \* note) uses to happen, in such circumstances, to have nothing on the surface of it worth weighing distinctly in the scales, in which the cupel, with what was sunk into it, amounted to four ounces, three drachms, and eleven grains, which wanted but nine grains of the whole weight of the cupel and the two metals, when they were all three together committed to the fire.] So that, though we make a liberal allowance for the increment of weight that may, with any probability, be supposed to have been at-

\* Essay the Sixth, of the Usefulness of Natural Philosophy.

tained by the cupel, and what was put upon it, yet it will easily be granted, that very much the greater part of the metals was not driven off in fumes, but entered into the substance of the cupel.

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## T R I A L S of the Third Sort.

**A**FTER having shewn, that either flame, or the analogous effluxions of the fire, will be, what chymists would call corporified with metals and minerals exposed naked to its action; I thought it would be a desirable thing to discover, whether this flame or igneous fluid were subtle enough to exercise any such operation upon the light bodies sheltered from its immediate contact, by being included in close vessels; but it being very difficult to expose bodies in glasses to such vehement fires, without breaking or melting the glass, and thereby losing the experiment; I thought fit, first, to employ crucibles carefully luted together, that nothing might visibly get in or out; and of that attempt I find among my notes the following account.

### E X P E R I M E N T XIV.

[We took an ounce of steel freshly filed from a lump of that metal, that the filings might not be rusty, and having included them betwixt two crucibles, as formerly, kept them for two hours in a strong fire, and suffered them to continue there till the fire went out; the crucibles being unluted, the filings appeared hard-caked together, and had acquired a dark colour, somewhat between black and blue, and were increased five grains in weight.]

THE foregoing experiment being the first I mention of this kind, it will not be amiss to confirm it, by annexing the following memorial.

[AN ounce of filings of steel being put between the crucibles luted together, after they had been kept about an hour and half in the fire, were taken out, and being weighed, were found to have gained six grains.]

### E X P E R I M E N T XV.

[Two ounces of copper-plate were put into a new crucible, over which a lesser was whelmed, and the commissures were closed with lute, that nothing might fall in. After the same manner, two ounces of tin were included betwixt crucibles, and also two ounces of lead; these being put into the cupelling-furnace, were kept in a strong fire about an hour and a half, while something else was trying there. And then being taken out, the event was, that the copper-plates, though they stuck together, were not quite melted, and seemed, some of them, to have acquired scales like copper put into a naked fire, and the two ounces had gained eight grains in weight. The lead had broke through the bottom of the crucible, and thereby hindered the designed observation. The tin acquired six grains in weight, and was, in part, brought to a pure white calx, but much more of it was melted into a lump of a fine yellow colour, almost like gold, but deeper.]

The



The prosecution of this trial, as to the copper-plates, you will meet with in Experiment XXI. to which I therefore refer you.

N. B. BECAUSE lead, in cupellation, enters the cupel, we were willing to try, if we could so far hinder it from doing so, as to make some estimate what change of weight the operation of the fire would make in it; and therefore being able already to make a near guess how much a quantity of tin may gain by being calcined on a cupel, and remembering also, from some of my former trials, the indisposition which tin gives lead to cupellation, we mixed a drachm of tin with two ounces of lead, and exposing the mixture (in a cupel) to the fire under a muffler, we first brought it to fusion, and then it seemed at the top dry, and swelled, and discoloured; notwithstanding which, having continued the operation a good while, because of other things that were to be done with the same fire, we were not lucky enough to bring the experiment to an issue worth the relating here, in reference to the scope above-proposed, though in relation to another, the success was welcome enough.]

#### EXPERIMENT XVI.

[SUPPOSING, that if copper were beaten into thinner plates than those we lately used, and kept longer in the fire, this would have a more considerable operation upon them, we took one ounce of very thinly hammered pieces of copper, and putting them betwixt two crucibles (one whelmed over another) as in Experiment XV. with some lute at the corners of the juncture, to keep the fire from coming immediately at the metal, we kept them in the cupelling-furnace about three hours, and then disjoining the vessels, we found the metal covered with a dark and brittle substance, like that described in the above-quoted experiment. Which substance, when scaled off, disclosed a finely coloured metal, which, together with these burned scales, amounted to one-and twenty grains above the weight that was first put in.]

If, when these things were doing, I had been furnished with a very good lute, which is no such easy thing to procure, as chymists that have not frequently employed vulgar lutes, are wont to think; I would have made a trial of the ensuing experiment, for a good while, in the naked fire, notwithstanding that divers metalline minerals will scarce be brought to fusion in glasses, especially without such a fire, whose violence makes them break the vessels. For I thought, that by making a fit choice of the metals to be employed, I could prevent that inconvenience; but wanting the accommodations I desired, and yet presuming, that in a sand-furnace I might by degrees administer heat enough to melt so fusible a metal as fine tin, and keep it in fusion; I resolved to make some trials, first upon that, and then upon another metal. For though I was not sure of being then able to prosecute the experiment far enough; yet I hoped, I might, at least, see some effects of my first trial, which would enable me to guess what I was to expect from a complete one.

#### EXPERIMENT XVII.

[WE took then a piece of fine block-tin, and in a pair of good scales weighed out carefully half a pound of it; this we put into a choice glass retort, and kept it for two days, or thereabouts, in a sand-furnace, which gave heat enough to keep the metal in fusion without cracking the glass. Then taking out the mixture, we carefully weighed

it in the same scales, and found the superficies a little altered (as if it were disposed to calcination) and the weight to be increased about two grains, or somewhat better.]

### EXPERIMENT XVIII.

[THE other experiment I tried in glasses, was with mercury, hoping, that, if I could make a precipitate *per se*, in a hermetically sealed glass, I should, by comparing the weight of the precipitate, and the quicksilver that afforded, have a clear experiment to my purpose; and I should have no bad one, if I could but make it succeed with a glass, though not sealed, yet well stopped; instead of those infernal glasses (as they call them) which are commonly used, and wont to be left open (though some slightly stop them with a little paper, or cotton); but though, partly, that I might a little diversify the experiment, and make it the more likely to succeed in one or other of the glasses, I divided the mercury, and distributed it amongst several of them, and but a little to each, the success did not answer expectation, the hermetically sealed glasses being unluckily broken; and the precipitation in the others proceeding so slowly, that I was, by a remove, obliged to leave the trial imperfect; only I was encouraged (in case of a future opportunity) to renew it another time, by finding, that most of the glasses, though tall, and stopped with fit corks, afforded some very fair precipitate, but not enough to answer my design.]

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### TRIALS of the Fourth Sort.

MOST of the experiments hitherto recited having been made, as it were, upon the by with others, whose exigencies it was fit these should comply with; very few of the exposed bodies were kept in the cupelling-fire above two hours, or thereabouts. Upon which account, I thought fit to try, how much some bodies that had been already exposed to the fire, would gain in weight, by being again exposed to it; especially considering, that most calcinable bodies (for I affirm it not of all) which yield rather calces than ashes, by being without additament reduced in the fire to fine powder, seemed to be by that operation opened, or (as a chymist would speak) unlocked, and therefore, probably, capable of being further wrought upon, and increased in weight, by such a menstruum, as I supposed flame and igneous exhalations to be. And about this conjecture, I shall subjoin the ensuing trials.

### EXPERIMENT XIX.

[ONE ounce of calx of tin that had been made *per se*, for an experiment in our own laboratory, being put in a new cupel, and kept under the muffler for about two hours, was taken out hot, and put into the scales, where the powder appeared to have gained in weight one drachm, and thirty-five grains, by the operation of the fire, which made it also look much whiter than it did before, as appeared by comparing it with some of the calx that



that had not been exposed to the second fire; no part of the putty was, as we could perceive, melted by the vehemence of the fire, much less reduced into metal.]

## EXPERIMENT XX.

[OUT of a parcel of filings of steel that had been before exposed to the fire, and had its weight thereby increased some grains, not scruples; we took an ounce, and having exposed it at the same time with the calx of tin, and, for the same time, kept it in the fire, we took it out at the two hours end; and found the weight to be increased two drachms, and two-and-twenty grains. The filings were very hard baked together, and the lump being broken, looked almost like iron.]

## EXPERIMENT XXI.

THE following experiment, though it may seem in one regard but a continuation of the fifteenth, yet it has in this something peculiar from all the foregoing, that not only it affords an instance of the increase of weight obtained by a metal at the second time of its being exposed to the fire, but shews also, that such an increment may be had, though this second ignition be made in close vessels.

[SOME of the copper mentioned in Experiment XV. being accidentally lost, one ounce and four drachms of what remained was included betwixt two crucibles and exposed to a strong fire for two hours, and suffered to continue there till the fire went out; when it was taken out, it appeared to have gained ten grains in weight, and to have upon the superficial parts of the plates (as we observed) divers dark-coloured flakes, some of which stuck to the metal, but more, upon handling it, fell off.]

AND here I shall conclude one of the two parts of our designed treatise; for, though, I remember, that these were not all the trials that were made and set down upon the subject hitherto treated of; yet these are the chief, that having escaped the mischances which beset some others, I can meet with among my promiscuous memorials; whose number, when I drew them together, I could scarce increase, having by all these, and other trials of differing kinds, wasted my cupels and commodious glasses, where I could not well repair my loss. Whether I should have been able by reduction, specifick gravity, or any other of the ways, which I had in my thoughts, to make any discovery of the nature of the substance, that made the increment of weight in our ignited bodies; the want as well of leisure, as of accommodations requisite to go through with so difficult a task, keeps me from pretending to know. But these three things, I hope, I may have gained by what has been delivered; the first, that we shall henceforth see cause to proceed more warily in the experiments we make with metals in the fire, especially by cupellation. The next, that it will justify, and perhaps, procure an easier assent to some passages in my other writings, that have relation to the substance, whatever it be, that we are speaking of. And the third (which is the principal) that it will probably excite you, and your inquisitive friends, to exercise their sagacious curiosity in discovering what kind of substance that is, which, though hitherto overseen by philosophers themselves, and, being a fluid far more subtle than visible liquors, and able to pierce into the compact and solid bodies of metals, can yet add something to them that has no despicable weight upon the balance, and is able, for a considerable time, to continue fixed in the fire.

ADDI-

# ADDITIONAL EXPERIMENTS,

ABOUT ARRESTING AND WEIGHING OF

## IGNEOUS CORPUSCLES.

**E**XPERIMENTS to discover the increase in weight of bodies, though inclosed in glasses, being those that I considered as likeliest to answer what I designed in the hitherto prosecuted attempt, and finding the seventeenth experiment, as well as the next (tried upon mercury) to be very slow, and its performance not to be very great, I began to call to mind, what, many years ago, experience had shewn me possible to be performed, as to the managing glass-vessels, even without coating them, in a naked fire, provided a wary person were constantly employed to watch them. And supposing hereupon, that in no longer time than a laborant might, without being tired, hold out to attend a glass, a metal exposed in it to a naked fire might afford us a much more prosperous trial than that lately referred to, I afterwards resolved, when I should be able to procure some glasses conveniently shaped, to prosecute my design; in pursuance of which, though I had not any furnaces fitted for my purpose, I directed a laborant to make the following trials.

### EXPERIMENT I.

[We took eight ounces (*Troy* weight) of block-tin, which being cut into bits, was put into a good round phial with a long neck, and then warily held over quick coals, without touching them, till it was melted; after which, it was kept almost continually shaken, to promote the calcination, near an hour, the metal being all the while in fusion, and the glass kept at some distance from the thoroughly kindled coals. The most part of this time the orifice of the phial was covered with a cap of paper (which sometimes fell off by moving the glass) to keep the air, and steams of the coals, from getting into the neck. And at the end of this time, he, that held the glass, being tired, and having his hand almost scorched, the phial being removed from the fire, was broken, that we might take out the metaline lump, which had a little darkish calx here and there upon the upper surface, but much more beneath, where it had been contiguous to the bottom of the glass; then putting all this, carefully freed from little fragments of broken glass, into the same balance with the self-same counterpoise I had used before, I found, according to my expectation, an increase of weight, which amounted to eighteen grains, that the tin had acquired by this operation.]

E X P E.



EXPERIMENT II.

[THIS done, we separated the calx for fear of losing it, and having melted the metal in a crucible, that by pouring it out it might be reduced to thin plates, capable of being cut in pieces, and put into such another phial as the last; we weighed it again, together with the lately reserved calx, but found, that notwithstanding all our care, we had lost three grains of the eighteen we had gained. This done, we put the metal into another phial. But in regard the neck was shorter than that of the former, and could not, like it, be long held in one's hand; and because also I was willing to see what interest the shaking of melted tin has in the quickness of the calcination, the glass, which had a stopple of paper put to it to keep out smoke and air, was held at some distance from the coals, only whilst the tin was melting; and then was warily laid upon them, and kept there for two hours; at the end of which it was again taken off, and the metal weighed with the same counterpoise and balance, as formerly; and then it appeared to amount to eight ounces, twenty-four grains, and to have much more separable calx than at the first time. Nor did I much wonder, that the weight should be increased, in this last operation, but nine grains in two hours, and in the former, twice so many in half the time; since, during the two hours, the glass was kept in one posture, whereas, in the first operation, it was almost perpetually shaken all the while it was kept in fusion. And it is observed, that the agitation of melted minerals will much promote the effect of the fire upon them, and conduce to their calcination.

EXPERIMENT III.

THOUGH these trials might well satisfy a person not very scrupulous, yet to convince even those that are so, I undertook, in spite of the difficulties of the attempt, to make the experiment in glasses hermetically sealed, to prevent all suspicions of any accession of weight accruing to the metal, from any smoke or saline particles getting in at the mouth of the vessel. And in prosecution of this design, I thought upon a way of so hermetically sealing a retort, that it might be exposed to a naked fire, without being either cracked, or burst; an account of which trial was set down.

[EIGHT ounces of good tin, carefully weighed out, was hermetically sealed up in a new small retort, with a long neck, by which it was held in one's hand, and warily approached to a kindled charcoal fire, near which the metal was kept in fusion, being also ever now and then shaken for almost half an hour, in which time it seemed to have acquired on the surface such a dark colour, as argued a beginning of calcination, and it both emitted fumes, that played up and down, and also afforded two or three drops of liquor in the neck of the retort. The laborant being not able to hold the glass any longer, it was laid on quick coals, where the metal continued above a quarter of an hour longer in fusion; but before the time was come, that I intended to suffer it to cool, in order to the removing it, it suddenly broke in a great multitude of pieces, and with a noise like the report of a gun; but (thanks be to God) it did no harm neither to me, nor others, that were very near it. In the neck we found some drops of a yellowish liquor, which a virtuoso, that tasted it, affirmed to be of an odious, but peculiar sapor; and as for the smell, I found it to be very stinking, and not unlike that of the distilled oil of fish.]

BUT

*New Experiments to make*

BUT though our first attempt of this kind had thus miscarried, we were not thereby discouraged, but, in prosecution of the same design, made the ensuing trial.

## EXPERIMENT IV.

[THE tin, which had been before (in the first, or some such experiment) partly calcined in a glass, being melted again in a crucible, that it might be reduced to pieces small enough to be put into another glass, was put again into the scales, and the surplusage being laid aside, that there might remain just eight ounces; these were put into a bolt-head of white glass, with a neck of about twenty inches long, which being hermetically sealed (after the glass had been a while kept over the fire, lest that should break by the rarefaction of the air) the metal was kept in fusion for an hour and a quarter, as (being hindered by a company of strangers from being there myself) the laborant affirmed. Being unwilling to venture the glass any longer, it was taken from the fire, and when it was grown cold, the sealed end was broken off: but before I would have the bottom cut out, I observed that the upper surface of the metal was very darkly coloured, and not at all smooth, but much and very oddly asperated; and the lower part had between the bottom, and the lower part of the lump, a pretty deal of loose dark-coloured calx, though the neighbouring surface, and some places of the lump itself, looked by candle-light (it being then night) of a golden colour. The lump and calx together were weighed in the same scales carefully, and we found the weight to have increased twenty-three grains, and better, though all the calx we could easily separate, being weighed by itself, amounted not to four scruples, or eighty grains.

FOR confirmation of this experiment, I shall subjoin another, wherein but a quarter of so much metal was employed, with such success, as the annexed memorial declares.

## EXPERIMENT V.

[Two ounces of filings of tin were carefully weighed, and put into a little retort, whose neck was afterwards drawn slenderly out into a very small apex: then the glass was placed on kindled coals, which drove out fumes at the small orifice of the neck for a pretty while. Afterwards the glass, being sealed up at the apex, was kept in the fire above two hours; and then being taken off, was broken at the same apex; whereupon I heard the outward air rush in, because, when the retort was sealed, the air within it was highly rarefied. Then the body of the glass being broken, the tin was taken out, consisting of a lump, about which there appeared some grey calx, and some very small globules, which seemed to have been filings melted into that form. The whole weighed two ounces, twelve grains, the latter part of which weight appeared to have been gained by the operation of the fire on the metal. In the neck of the retort, where it was joined to the body, there appeared a yellowish and clammy substance thinly spread, which smelt almost like the foetid oil of tartar.

## EXPERIMENT VI.

To vary the foregoing experiments by making trials on a mineral, that is held to be of a very metalline nature, but is not a true metal, nor will be brought to fusion by so mode-



moderate a heat, as will suffice to melt tin, and yet has parts less fixed than tin, as being far more easily sublimable, we thought fit to make the following experiment.

[We took an ounce of filings of zink, carefully weighed; and having as carefully put them into a round bolt-glass, we caused the neck to be drawn out very slender, and then ordered the laborant to keep it upon quick coals for the appointed time. Afterwards returning home, I called for the glass, which, he said, he had kept four hours upon the coals; answering me also, that there did, for a great part of the time, smoke appear to ascend from the zink, and get out at the unstopped apex. And in effect I observed, that the upper part of the glass was lined with flores or sublimate of a darkish grey. The glass being dexterously cut asunder, we took out not only the filings of zink, some of which were melted into little globules, but the flores too; and yet weighing all these in the same scales we had used before, we found five grains and somewhat better wanting of an ounce. Which we the less wondered at, because of the continuance of the lately mentioned exhalations emitted by the filed mineral.]

#### EXPERIMENT VII.

For more ample confirmation of the truth discovered by what I have been reciting about tin, I thought fit to try the like experiment upon another metal, which, though of somewhat more difficult fusion than tin, I had reason to think might, if employed in a moderate quantity, and warily managed, be kept melted in glass without breaking it. And accordingly, having carefully weighed out four ounces of good lead, cut beforehand into pieces little enough for the orifice of the glass, I caused them to be put into a small retort with a long neck, wherein was afterwards left but an orifice not much bigger than a pin's head: then leaving directions with the laborant what to do, because I was myself called abroad, at my return he brought me, together with the glass, this account; that he had kept it over and upon the coals two hours, or better, and then supposing the danger of breaking the glass was over, he had sealed it up at the little orifice newly mentioned, and kept it on the coals two hours longer. Before the glass (which I found to be well sealed) was broken, I perceived the pieces of lead to have been melted into a lump, whose surface was dark and rugged, and part of the metal to have been turned into a dark coloured powder or calx: all this being taken out of the retort, was weighed in the same balance, whereon the lead appeared to have gained by the operation somewhat above thirteen grains.

#### EXPERIMENT VIII.

To shew that metals are not the only bodies that are capable of receiving an increase of weight from the fire, I thought fit to make upon coral a trial, whereof my memorial gives me this account.

[LITTLE bits of good red coral, being hermetically sealed up in a thin bubble of glass, after two drachms of them had been weighed out in a pair of nice scales, were warily kept at several times over and upon kindled coals, and at length being taken out for good and all, were found of a very dark colour, and to have gained in weight three grains and about a half.]

## EXPERIMENT IX.

ONE experiment there is, which, though it might have come in more properly at another place, is not to be omitted in this, because it may invite us to consider, whether, in the foregoing experiments, excepting those made on lead and tin in sealed vessels, there may not be more of the fire adherent to, or incorporated with the body exposed to it, than one would conclude barely from the recited increments of their weight. For having taken very strong fresh quick-lime, provided on purpose for choice experiments, and exposed it, before the air had time to flake it, upon the cupel, to a strong fire, where it was kept for two hours; I found that it had increased in weight even somewhat beyond my expectation. For being seasonably put into the balance, the lumps, that weighed, when exposed, but two drachms, amounted to two drachms and twenty-nine grains; which makes this experiment a pregnant one to our purpose. For by this it appears, that notwithstanding a body may for many hours, or even for some days, be exposed to a very violent fire, yet it may be still capable of admitting and retaining fresh corpuscles; so that, though well made lime be usually observed to be much lighter than the stones whereof it is made, yet this lightness does not necessarily prove, that, because a burnt lime-stone has lost much of its matter by the fire, it has therefore acquired no matter from the fire; but only infers, that it has lost far more than it has got. And this may give ground to suspect, that in most of the foregoing trials, the accession of the fiery particles was greater (though in some more, in others less so) than the balance discovered; since, for aught we know, divers of the less fixed particles of the exposed body might be driven away by the vehemence of the heat; and consequently the igneous corpuscles, that fastened themselves to the remaining matter, might be numerous enough not only to bring the accession of weight, that was found by the scales, but to make amends for all the fugitive particles, that had been expelled by the violence of the fire. And since so fixed a body as quick-lime is capable of being wrought upon by the igneous effluvia, so as that they come to be, as it were, incorporated with it, it may, perchance, be worth considering, whether in other calcined, or incinerated bodies, the remaining calces, or ashes, may not retain more than the bare impression (unless that be stretched to mean some participation of a substance) of the fire. Whether these particles, that adhere to, or are mingled with the stony ones of the lime, may have any thing to do in the heat and tumult, that it produced upon the slaking of lime, this is not a fit place to examine. And though by this experiment, and those made in sealed retorts, which shew that what is afforded by fire may in a corporeal way invade, adhere, and add weight to even fixed and ponderous bodies, there is a large field opened for the speculative to apply this discovery to divers phenomena of nature and chymistry; yet I shall leave this subject unmeddled with in this place.



A

## D I S C O V E R Y

OF THE

## P E R V I O U S N E S S

O F

## G L A S S

T O

## PONDERABLE PARTS OF FLAME.

With some Reflections on it by way of COROLLARY.

**T**HAT I might obviate some needless scruples, that may be entertained by suspicious wits upon this circumstance of our additional experiments, "That the glasses employed about them were not exposed to the action of mere flame, but were held upon charcoals" (which to some may seem to contain but a grosser kind of fire); and that also I might, by diversifying the way of trial, render such experiments both more fit to afford corollaries, and more serviceable to my other purposes, I attempted to make it succeed with a body so thin and disengaged from gross matter, as mere flame is allowed to be, knowing, that by going cautiously with it to work, one might handle a retort without breaking it, in spite of a violent agitation of kindled matter.

## E X P E R I M E N T I.

SUPPOSING then that good common sulphur, by reason of its great inflammability, and the vehemency and penetrancy of its flame, would be a very fit fuel for my purpose, I provided a small double vessel so contrived, that the one should contain as many coals, as was necessary to keep the sulphur melted, and that the other, which was much smaller,

*A Discovery of the Perviousness of Glafs.*

smaller, and shaped like a pan, should contain the brimstone requisite for our trial; and lastly, that these two should be with a convenient lute so joined to one another, that all being closed at the top, save the orifice of the little pan (the fire and smoke of the coals having their vent another way) no fire should come at the retort to be employed, but the flame of the burning brimstone. Then two ounces of filings of tin being heedfully weighed out, and put into a glass retort provided for such trials, and made fit to be easily sealed up at the neck, when the time should be convenient, the sulphur (which ought to be of the purer sort) was kindled, and the glass by degrees exposed to it; where it continued, as the laborant informed me (the smell of brimstone, peculiarly offensive to me, forbidding me to be present) near two hours before the metal melted; after which, he kept the retort near an hour and half more, with the metal melted in it. Then bringing it me to look upon, I perceived a pretty deal of darkish calx at the bottom, and partly too upon the surface of the far greater part of the metal, which now lay in one lump. The part of the retort that had been sealed, being broken off, we first took out the calx, and then the lump, and putting them into the scales they had been formerly weighed in, found them to have made a very manifest acquit of weight, which, if both the laborant and I be not mistaken (for the paper which should inform us, is now missing) amounted to four grains and a half, gained by the recited operation. Afterwards, we being grown more expert in making such trials, the experiment was repeated with the same quantity of filings of the same metal: at the end of the operation (which in all lasted somewhat above three hours) having broken off the sealed neck of the retort, we found that a good proportion of dark-coloured calx had been produced. This being weighed with the uncalcined part of the metal, the two ounces we first put in, appeared to have acquired no less than eleven grains and a half (and somewhat better).

SUCH superstructures, both for number and weight, may possibly in time be built on this and the like experiments, that I shall venture to obviate, even such a scruple, as is like to be judged too sceptical. But I remember, that, considering upon occasion of some of the experiments, formerly recited, that though it were very improbable, yet it did not appear impossible, that the increment of weight, acquired by bodies exposed in glass-vessels to the fire, might proceed, not from the corpuscles of fire, but from the particles of the glass itself, loosened by the power of so intense a heat, and forcibly driven into the inclosed body; I was content to take a couple of glasses, whereof one was shaped into a little retort, and having weighed them, and then having kept them for a considerable time upon kindled coals, and then weighed them again, I could gather little of certainty from the experiment (the retort at one time seeming to have acquired above half a grain in the fire) save that there was no likelihood at all, that so considerable an encrease of weight, as we divers times obtained in close vessels, should proceed from the glass itself, and not from the fire.

## EXPERIMENT II.

BECAUSE it seems evident enough, that whatever chymists tell us of the hypostatical sulphur, common brimstone is a body heterogeneous enough, having in it some parts of an oily or inflammable nature, and others acid, and very near of kin to the spirits of vitriol; I thought fit to vary our experiment, by making it with a liquor that is generally reputed to be as homogeneous as chymists themselves are wont to render any, I mean with a spirit of wine, or some such liquor as will totally flame away without  
affording



affording foot, or leaving any drop of phlegm behind it. In prosecution of this design, we carefully weighed out an ounce of filings of block-tin, and put them into a glass retort, fit for the purpose, whose neck was afterwards drawn out to a great slenderness; and we also provided a conveniently shaped metalline lamp, such as that the flame of this ardent spirit might commodiously burn in it, and yet not melt or crack it; which lamp, though furnished with a cotton wick, afforded no foot, because, as long as it was supplied with liquor enough, it remained unburnt. These things being in readiness, the retort was warily approached to the flame, and the metal was thereby in a short time melted. After which, the glass being kept exposed to the same flame for near two hours in all, the sealed apex of the retort was broken off, and there appeared to have been produced a not inconsiderable quantity of calx, that lay loose about the remaining part of the tin, which, upon its growing cold, was hardened into a lump. This, and the calx, being taken out of the retort with care, that no little fragment of glass should at all impose upon us, was weighed in the same scales as formerly, and found to have gained four grains and a half, besides the dust, that stuck in the inside of the retort, of which we reckoned enough to make about half a grain more; so that of so fine and pure a flame, as of this totally ardent spirit, enough to amount to five grains, was arrested, and in good measure fixed by its operation on the tin it had wrought upon.

### EXPERIMENT III.

For confirmation of the former trial, wherein we had employed the *spiritus ardens* of sugar, we made the like experiment with highly rectified spirit of wine, only substituting an ounce of lead instead of one of tin. The event, in short, was this; that after the metal had been for two hours or better kept in the flame, the sealed neck of the retort being broken off, the external air rushed in with a noise (which shewed the vessel to have been very tight) and we found pretty store of the lead, for it was above seven scruples, turned into a greyish calx, which together with the rest of the metal being weighed again, there was very near, if not full six grains of increase of weight acquired by the operation.

1. N. B. THE lump of lead, that remained after the newly recited operation, being separated from the calx, was weighed and cut in pieces, that it might be put into a fresh retort, wherein it was again exposed to the flame of spirit of wine, that I might satisfy myself, whether probably the whole body of the lead might not, by repeated operations, or (perhaps by one continued long enough) be reduced to calx. And though after the retort (whose neck had been drawn out) had been kept in the flame for about two hours, it was, by the negligence of a foot-boy, unluckily broken, and some of the calx lost; yet we made a shift to save about five grains of it (whose colour was yellowish; which was enough to make it likely, that if we had had conveniency to pursue the operation to the utmost, the whole metal might have been calcined by the action of the flaming spirit.

2. N. B. AND lest you should be induced by some chymical conceit to imagine, that the particles that once belonged to flame, did make more than a coalition with those of the lead, and by a perfect union were really transmuted into the metal whose weight they encreased; I shall add, that (according to a method elsewhere delivered) I examined the seven scruples of calx, mentioned to have been made in the third experiment, by weighing them in air and water, and thereby found, as I expected, that though the absolute gravity of the metal had been encreased by the particles of flame, that stuck

fast.

fast to it, yet this aggregate of lead and extinguished flame had lost much of its specific gravity. For whereas lead is wont to be, to water of the same bulk, as about eleven and an half to one; the subtil calx of lead was to water of the same bulk, little, if at all, more than as nine to one.

THESE are not the only experiments I made of the operation of mere flame upon bodies inclosed in glasses; but these, I suppose, are sufficient to allow me to comply with my present haste, and yet make good the title prefixed to this paper. For whence can this increase of absolute weight (for I speak not of specific gravity) observed by us in the metals exposed to the mere flame, be deduced, but from some ponderable parts of that flame? And how could those parts invade those of the metal inclosed in a glass, otherwise than by passing through the pores of that glass? But because I judge it unphilosophical, either to be more careful, that what one writes should appear strange, than be true; or to be forward to advance the repute of strangeness to the prejudice of the interest of truth, though it be perhaps but a remote one, or a collateral one; I shall deal so impartially, as to subjoin on this occasion two or three short intimations, that may prove both seasonable for caution, in reference to the porousness of glass, and give a hint or two in relation to other things.

I do not then, by the foregoing experiments, pretend to make out the porosity of glass any farther, than is expressed in the title of this paper; namely, in reference to some of the ponderable parts of flame. For otherwise I am not at all of their mind, that think glass is easily penetrable, either, as many do, by chymical liquors; or, as some, by quicksilver; or, as others, at least by our air; those opinions not agreeing with the experiments I made purposely to examine them, as you may find in another paper.

AGAIN, if we compare the increase we observe to be made in the weight of the bodies, that we expose to the naked fire, and those of the same or the like kinds, that we included in glasses, or so much as in crucibles; it may be worth considering, whether this difference in acquired weight may not give cause to suspect, that the corpuscles, whereof fire and flame consists, are not all of the same size, and equally agitated, but that the interposed vessel keeps out the grosser particles like a kind of strainer, though it gives passage to the minutest and most active.

I OFFER it also to consideration, whether this perviousness of glass, even to the minute particles that pervade it, and their adhesion to the metal they work on, does necessarily imply pores constantly great enough to transmit such corpuscles: or, whether it may not be said that glass is generally of a closer texture, than when in our experiments the pores are opened by the vehement heat of the flame, that beats upon it, and in that state may let pass corpuscles too big to permeate glass in its ordinary state; and, that this penetration is much assisted by the vehement agitation of the igneous parts, which, by the rapidness of their motion, both force themselves a passage through the narrow pores of the glass, and pierce deep enough into those of the included body, to stick fast there (as hail-shot thrown with one's hand against a board will pass off from it, but being shot out of a gun will pierce it, and lodge themselves in it): and I know a menstruum, that does not work upon a certain metal, whilst the liquor is cold, or but faintly heated; and yet by intending the heat, would be made to turn it into a powder or calx (for it does not properly dissolve it).

PERHAPS it may not be amiss to add on this occasion, that though glass be generally acknowledged to have far smaller pores, than any other matter wont to be implied to make vessels, that are to be exposed to the fire; yet, till I be farther satisfied, I shall forbear both to determine, whether the rectitude, that some philosophers suppose in the



pores of glass, as it is a transparent body, or rather in their ranks or rows, may facilitate the perviousness we above observed in glass, and to conclude from the foregoing experiments, that ponderable parts of flame will be able, as well to pass through the pores of metalline vessels, as those of glass. For though, with a silver vessel, made merely of plate, without solder, I made two or three trials (of which you may command an account) in order to the resolving of these doubts; yet by an accident, which, though it were not a surprizing one, was unlucky enough to defeat my endeavours, I was kept, for want of fit accommodations, from bringing my intended trials to an issue.

AND now having endeavoured by the foregoing advertisements, to prevent the having unsafe consequences drawn from our experiments; it remains, that I briefly point at three or four corollaries, that may more warily be deduced from them. To which, if I get time, I may subjoin a hint or two about further enquiries.

## COROLLARY I.

*Confirming this paradox, that flame may act as a menstruum, and make coalitions with the bodies it works on.*

THE experiments we have made and recited, of the permeating of flame (as to some of its parts) through glass vessels, and of its working on included metals, may much confirm the paradox I have elsewhere proposed, that flame may be a menstruum, and work on some bodies at the rate of being so; I mean, not only by making a notable comminution and dissipation of the parts, but by a coalition of its own particles with those of the fretted body, and thereby permanently adding substance and weight to them. Nor is it repugnant to flame's being a menstruum, that in our experiment the lead and tin, exposed to it, were but reduced to powder, and not dissolved in the form of a liquor, and kept in that state. For, besides that the interposed glass hindered the igneous particles from getting through in plenty enough; I consider that it is not necessary, that all menstrooms should be such solvents, as the objection supposes. For whether it be (as I have sometimes suspected) that menstrooms, that we think simple, may be compounded of very differing parts, whereof one may precipitate what is dissolved by the other; or for some other cause, I have not now time to discuss. Certain it is, that some menstrooms corrode metals and other bodies, without keeping dissolved all, or perhaps, any considerable part; as may be seen, if you put tin in a certain quantity of aqua fortis, which will in a very short time reduce it almost totally to a very white substance, which, when dry, is a kind of calx. And so by a due proportion of oil of vitriol, abstracted from quicksilver by a strong fire, we have divers times reduced the main body of the mercury into a white powder, whereof but an inconsiderable part would be dissoluble in water. And such a white calx I have had by the action of another fretting liquor on a body not metalline.

AND having thus cleared our paradox of the opposed difficulty, my haste would immediately carry me on to the next corollary, were it not, that there is one phenomenon belonging to this place, that deserves to be taken notice of. For whether it be, as seems probable, from the vehement agitation of the permeating particles of flame, that violently tear asunder the metalline corpuscles, or from the nature of the igneous menstruum (which being, as it were, percolated through glass itself, must be strangely minute) it is worth observing, how small a proportion, in point of weight, of the additional

tional adhering body may serve to corrode a metal, in comparison of the quantity of vulgar menstruums, that is requisite for that purpose. For whereas we are obliged to employ, to the making the solution of crude lead, several times its weight of spirit of vinegar, and (though not so many times) even of aqua fortis, it was observed in our experiment, that, though the lead was encreased but six grains in weight, yet above six score of it were fretted into powder, so that the corrosive body appeared to be about the twentieth part of the corroded.

## COROLLARY II.

*Proposing a paradox about calcination, and calces.*

ANOTHER consequence, deducible from our discovery of the perviousness of glass to flame, may be this, that there is cause to question the truth of what is generally taken for granted about calcination, and particularly of the notion, that not only others, but chymists themselves, have entertained about the calces of metals and minerals. For whereas it is commonly supposed, that in calcination the greater part of the body is driven away, and only the earth, to which chymists add the fixed salt, remains behind; and whereas even mechanical philosophers (for two or three of them have taken notice of calcination) are of opinion, that much is driven away by the violence of the fire, and the remaining parts, by being deprived of their more radical and fixed moisture, are turned into dry and brittle particles: whereas these notions, I say, are entertained about calcination, it seems, that they are not well framed, and do not universally hold; since, at least, they are not applicable to the metals our experiments were made on. For, it does not appear by our trials, that any proportion, worth regarding, of moist and fugitive parts, was expelled in the calcination; but it does appear very plainly, that by this operation the metals gained more weight than they lost; so that the main body of the metal remained entire, and was far from being, either as a peripatetick would think, elementary earth, or a compound of earth and fixed salt, as chymists commonly suppose the calx of lead to be. From which very erroneous hypothesis they are wont to infer the sweet vitriol of lead, which they call *saccharum Saturni*, to be but the sweet salt of it extracted only by the spirit of vinegar, which does indeed plentifully enough concur to compose it. Whence I conclude, that the calx of a metal even made as they speak, *per se*, that is, by fire without additament, may be, at least in some cases not the *caput mortuum*, or *terra damnata*, but a magistery of it. For, in the sense of the most intelligible of the chymical writers, that is properly a magistery, wherein the principles are not separated, but the bulk of the body being preserved, it acquires a new and convenient form by the addition of the menstruum, or solvent, employed about the preparation. And, not here to borrow any argument from my notes about particular qualities, you may guess how true it is that the greatest part of the body, or all the radical moisture, is expelled in calcination, which therefore turns the metal into an arid, unfusible powder; by this, that I have several times, from calx of lead, reduced corporal lead. And I remember, that having taken what I guessed to be but about a third, or fourth part of the calx of lead, produced by the third experiment, I found by a trial purposely devised, that without any flux-powder, or any additament, but merely by the application of the flame of highly rectified spirit of wine, there could, in a short time, be obtained a considerable proportion of malleable lead; whereof the part I had the curiosity to examine, was true malleable lead; so little was the arid powder, whence  
this



this was reduced, deprived by the foregoing calcination of the supposed radical moisture requisite to a metal. The consideration of what may be drawn from this reduction, in reference to the doctrine of qualities, belongs not to this place.

## COROLLARY III.

ONE use, among the rest, we may make, by way of corollary, of the foregoing discovery, which is in reference to a controversy warmly agitated among the corpuscular philosophers themselves. For some of them, that follow the Epicurean or atomical hypothesis, think that when bodies are exposed in close vessels to the fire, though the igneous corpuscles do not stay with the bodies they invade, yet they really get through the pores of the interposed vessels, and permeate the included bodies in their passage upwards; whereas others, especially favourers of the Cartesian doctrine, will not allow the atomists igneous corpuscles, which they take to be but vehemently agitated particles of terrestrial matter, to penetrate such minutes pores as those of glass; but do suppose the operation of the fire to be performed by the vehement agitation made of the small parts of the glass, and by them propagated to the included bodies, whose particles, by this violent commotion, are notably altered, and receive new textures, or other modifications.

BUT our experiments inform us, that though neither of the two opinions seems fit to be despised, yet neither seems to have hit the very mark: though the Epicurean hypothesis comprize somewhat more of the truth, than the other. For though it be not improbable that the brisk agitation, communicated by the small parts of the glass to those of the body contained in it, may contribute much to the effect of the fire; and though, by the small increment of weight we found in our exposed metal, it is very likely, that far the greater part of the flame was excluded by the close texture of the glass; yet, on the other side, it is plain that igneous particles were trajected through the glass, which agrees with the Epicureans; and they, on the other side, mistook in thinking that they did but pass through, and divide, and agitate the included bodies; to which, nevertheless, our experiments shew, that enough of them, to be manifestly ponderable, did permanently adhere.

WHETHER these igneous corpuscles do stick, after the like manner, to the parts of meat, dressed by the help of the fire, and especially roast-meat, which is more immediately exposed to the action of the fire, may be a question which I shall now leave undiscussed, because I think it difficult to be determined, though, otherwise, it seems worthy to be considered, in regard it may concern men's health to know, whether the coction of meat be made by the fire, only as it is a very hot body, or whether it permanently communicates any thing of its substance to the meat exposed to it: in which last case, it may be suspected, that not only the degree and manner of application of a fire, but the nature of its fuel, may be fit to be considered.

## COROLLARY IV.

THE experiments above recited give us this further information, that bodies very spiritous, fugitive, and minute, may, by being associated with congruous particles, though of quite another nature, so change their former qualities, as to be arrested, by a solid

and ponderous body, to that degree, as not to be driven away from it by a fire intense enough to melt and calcine metals.

Exp. III. For the foregoing trials (taking in what I lately delivered of the lessened specifick gravity of calcined lead) seems plainly enough to discover, that even the agitated parts of flame, minute enough to pass through the pores of glass itself, were, as it were, entangled among the metalline particles of tin and lead, and thereby brought to be fixed enough to endure the heat, that kept those metals in fusion, and little by little reduced them into calces: which is a phenomenon, that one would not easily look for, especially considering how simple a texture that of lead or tin may be supposed to be, in comparison of the more elaborate structures of very many other bodies. And this phenomenon, which shews us what light and fugitive particles of matter may permanently concur to the composition of bodies ponderous and fixed enough, may perchance afford useful hints to the speculative; especially if this strict combination of spirituous and fugitive substance with such, as being gross or unwieldy, are less fit than organized matter, to entangle or detain them, be applied (as it may be with advantage) to those aggregates of spirituous corpuscles, and organical parts, that make up the bodies of plants and animals. And this hint may suggest a main inference to be drawn from the operations of the sunbeams on appropriated subjects, supposing it to prove like that of flame on tin and lead.

AND now having dispatched our corollaries, we might here enquire, whether all the particles of fire and flame, that are subtil, and agitated enough to penetrate glass, and fasten themselves to included bodies, be reduced by ignition to the same nature, or else retain somewhat of their proper qualities? which enquiry I have some cause not to think so undeterminable, as at first blush it may appear. For one of the ways that may be proposed for this examen, is already intimated at the close of the third experiment; which shews, that we may compare the specifick gravity of the calces of the same metal, made in glasses by the operation of flames, whose fuels are of very differing natures. And I said, one of the ways, because it is not the only way I could name, and have partly tried. But though I might say more concerning expedients of this kind, and could perhaps propound other enquiries, that may reasonably enough be grounded upon the hitherto recited phenomena (and those of some other like trials) yet I must not unreasonably forget, that the pursuit of such disquisitions would lead me much farther, than I have now the leisure to follow it.



A

L E T T E R

CONCERNING

A M B E R G R I S,

AND ITS BEING

A VEGETABLE PRODUCTION.

First published in the *Philosophical Transactions*, N<sup>o</sup> 97, p. 6113,  
for September 13, 1673.

S I R,

SOME occasions calling me this afternoon up to *London*, I met there with a very intelligent gentleman, who was ready to go out of it; but before he did so, he willingly spared me some time to discourse with him about some of the affairs of our East-Indian Company, of which he was very lately deputy-governor; and, his year being expired, is still one of the chief of the court of committees, which a foreigner would call directors, that manage all the affairs of that considerable society. And, among other things, talking with him about some contents of a journal lately taken in a Dutch East-Indian prize, I learned from him, that he, who understands that language very well, is now perusing that manuscript, and, among many other things recorded there, that concern the oeconomical and political affairs of the said Dutch Company, he met with one physical observation which he thought so rare, that remembering the curiosity I had expressed for such things, he put it into English, and transcribed it for me, and immediately drawing it out of his pocket, he presented me the short paper, whereof I now shew you the copy: upon perusal of which, you will very easily believe, that not only his civility obliged me, but the information

*A Letter concerning Ambergris.*

it brought me, surprized me too. For the several trials and observations of my own, about ambergris, have long kept me from acquiescing either in the vulgar opinions, or those of some learned men concerning it; yet I confess, my experiments did much less discover what it is, than this paper has done, in case we may safely and entirely give credit to its information, and that it reach to all kinds of ambergris. And, probably, you will be invited to look on this account, though not as complete, yet, as very sincere, and, on that score, credible, if you consider, that this was not written by a philosopher, to broach a paradox, or serve an hypothesis, but by a merchant, or factor, for his superiors, to give them an account of a matter of fact; and that this passage is extant in an authentic journal, wherein the affairs of the company were, by publick order, from time to time, registered, at their chief colony, *Batavia*. And it appears by the paper itself, that the relation was not looked upon as a doubtful thing, but as a thing, from which a practical way may be deduced to make this discovery easily luciferous to the Dutch Company. And I could heartily wish, that in those countries that are addicted to long navigations, more notice, than is usual, were taken and given of the natural rarities that occur to merchants and seamen. On which occasion, I remember, when I had, in compliance with my curiosity, put myself into our East-Indian Company, and had, by their civility to me, been chosen of their committee, as long as my health allowed me to continue so, I had the opportunity in some register books of merchants, English and Dutch, to observe some things, which would easily justify this wish of mine, if my haste, and their interest, would permit me to acquaint others with them. But to return to our account of ambergris, I think you will easily believe, that if I had received it not by a paper, but immediately from the writer, I should, by proposing divers questions, have been enabled to give you a much more satisfactory account, than this short one contains. But the obliging person that gave it me, being just going out of town, I could not civilly stay him to receive my queries about it; which though (God permitting) I may propose, ere long, if I can light on him again, yet I fear he has given me in these few lines, all that he found about this matter. However, this relation, as short as it is, being about the nature of a drug so precious, and so little known, will not, I hope, be unwelcome to the curious; to whom none is so like to convey it so soon, and so well, as M. O.; whose forwardness to oblige others by his various communications, challenges returns of the like nature from others, and particularly from his affectionate humble servant.

*Follows the Extract itself out of a Dutch journal, belonging to the Dutch East-Indian Company.*

“**A**MBERGRIS is not the scum, or excrement of the whale, &c. but issues out  
 “ of the root of a tree, which tree, how far soever it stands on the land, always  
 “ shoots forth its roots towards the sea, seeking the warmth of it, thereby to deliver  
 “ the fattest gum that comes out of it: which tree, otherwise, by its copious fatness,  
 “ might be burned and destroyed. Wherever that fat gum is shot into the sea, it is so  
 “ tough, that it is not easily broken from the root, unless its own weight, and the  
 “ working of the warm sea doth it, and so it floats on the sea.  
 “ THERE was found by a soldier,  $\frac{7}{8}$  of a pound, and by the chief, two pieces weigh-  
 “ ing five pounds. If you plant the trees where the stream sets to the shore, then the  
 “ stream will cast it up to great advantage. *March 1, 1672, in Batavia: journal ad-*  
 “ *vice from—*”



# T R A C T S,

CONSISTING OF

OBSERVATIONS about the SALTNESS of the SEA.

An Account of a STATICAL HYGROSCOPE, and its USES.

TOGETHER WITH

An APPENDIX about the FORCE of the Air's MOISTURE.

A FRAGMENT about the NATURAL and PRETERNATURAL STATE of  
BODIES.

TO ALL WHICH IS PREMISED

A SCEPTICAL DIALOGUE about the POSITIVE or PRIVATIVE NATURE  
of COLD.

WITH

Some EXPERIMENTS of Mr. *BOYLE*'s referred to in that Discourse.

OF THE

POSITIVE OR PRIVATIVE NATURE

OF

C O L D.

A SCEPTICAL DIALOGUE between *CARNEADES*, *THEMISTIUS*,  
*ELEUTHERIUS*, *PHILOPONUS*.

## SECTION I.

*Eleuth.* MAY one be allowed to ask *Carneades*, what book it is he is reading with  
so much attention?

*Carn.* THE question, *Eleutherius*, is very allowable, and as easily answered, by saying,  
that what I was reading, is our friend Mr. *Boyle*'s newly published History of cold.

*Them.* Your readiness, *Carneades*, to answer, encourages me also to ask you a ques-  
tion ; which shall not be, as probably you expect it should, how you like this new piece?  
for

for I know you would be too kind to the author, not to tell me, that he has detected some old errors, and made discovery of some new truths: but my question shall be about what is my wonder, as well as that of divers others, who think it strange, that a writer that has delivered so many effects and other phænomena of cold, should omit to tell us so much, as whether he asserts it to be a positive quality, or a bare privation of heat; as, since *Cardan* (in his treatise *De Subtilitate*) some other learned men, and especially *Cartesius*, have maintained.

*Carn.* You will not wonder if a person that you look upon, and I confess not injuriously, as a friend to Mr. *Boyle*, tell you, that this author, by the many histories he has presented us, and by his not seeming to dare to determine the controversy you have mentioned, shews, that he was more solicitous to lessen his ignorance, than to pretend to knowledge: and upon the observation I have made of his humour in general, I presume one principal reason of his silence may be, that he has not yet completed the trials he had designed about cold; and thinks that in abstruse subjects, such as this is, it is not so convenient to deliver a positive opinion of the nature of it at the beginning, as to reserve it for the latter end, after the history of the phænomena; when the nature of the thing enquired into may, as it were, spontaneously result from the considerations suggested by the precedent matters of fact surveyed together.

*Eleuth.* If such a wariness were indeed the motive of your friend's silence, I shall easily excuse it; and perhaps think too, that the like would not mis-become naturalists on many other occasions. And yet I do not dislike *Themistius's* question; for it is one thing to venture upon declaring the adequate nature of cold, and another to determine, whether it be a positive, or a privative quality? the latter attempt importing a much less venture than the former.

*Carn.* I WILL not pretend to know the very reasons that induced the author silently to pass by this controversy; but having been once present, when he had occasion to discourse of it, I then conjectured, that among his experiments of cold, that are not yet published, there may be some uncommon ones, that may have suggested to him scruples, which obliged him to forbear declaring himself, till he had cleared them, which those that are unacquainted with such trials, may probably have never thought of.

*Them.* If what you call a controversy, were indeed worthy of that name, I should not unwillingly allow of your friend's silence; but the opinion broached by *Cardan*, and adopted by Mr. *Des Cartes* and others, seems to me so devoid not only of reason, but of all appearance of it, that methinks one that has delivered such considerable effects of cold, as Mr. *Boyle* has done, may well ascribe to their cause, at least a positive nature; and without at all being guilty of boldness, reject an opinion, that is not only barely an error, but an extravagance, and perhaps a plain absurdity.

*Carn.* POSSIBLY the gentleman we are speaking of, may be wary and sceptical enough to reckon among difficult things, not only the declaring the adequate nature of cold, and the manner of its operations; but the demonstrating whether it be a positive quality or not. And though I will not take upon me to know his thoughts about that subject, which, perhaps, are grounded upon some of his peculiar experiments and notions; yet, for discourse sake, I am content to debate with *Themistius*, Whether or no the opinion, he so severely censures, be not only erroneous, as, for aught appears, Mr. *Boyle* himself may be found to have thought it; but also, as *Themistius* would have it, absurd.

*Them.* I READILY accept of your offer; for it cannot be an unpleasant entertainment to observe the arts, whereby one, that I know will not speak impertinently, will endeavour to make reason elude the clearest testimonies of sense. And though I might press you



you with the concurrent authority of *Aristotle*, and all the philosophers that have lived between his time and those of that extravagant fellow *Cardan*; yet I shall rather employ, to convince you, the authority and reasons of a grand leader among your new philosophers, who being a great broacher of paradoxes, and having upon that score written books expressly against *Aristotle*, was not like to have sided with him, unless the evidence of truth had, as it were, necessitated him to do so.

*Carn.* I PRESUME, you mean the learned and subtle *Gassendus*, whom I am glad you have pitched upon for your cause's champion, not only because, in defending the common opinion, he waves the common practice of troubling his readers with a multitude of authorities, which to me, in such a case as this, would signify very little, and betakes himself to arguments; but because, being so modern and judicious a writer, we may well suppose him to have summed up and improved what can be said in behalf of the cause he maintains. Upon which account, I shall be excused from answering impertinent objections against the opinion I defend, and from the trouble of ranging about, among other authors, for more weighty arguments than those, which the disproving of his will shew to be unsatisfactory.

*Them.* I AM glad you named the author, I meant *Carneades*, for I apprehended you had not met with what he says upon this subject; because I could scarce imagine that an intelligent person, after having read his arguments, will doubt of a truth he hath so clearly evinced by them. But since I perceive you have seen what he has written, I shall, without farther preamble, propose his reasons to you, though not in the very same order wherein he has couched them.

*Eleuth.* BUT before you begin them, give me leave to ask *Carneades* a short question, whose answer will, I suppose, conduce, if not be necessary, to the clearing of the state of the controversy betwixt you. For it is one thing to deny belief to the received opinion, that cold is a positive quality, and another thing to assert, that it is but a privation of heat; since, if *Carneades* does undertake the latter of these two, he must bring positive arguments to prove cold to be but a negative thing. Whereas, if he content himself to play a doubting part, it may suffice him, being in effect but a defendant, to shew, that the proofs brought to conclude cold to be a positive quality are not cogent.

*Corn.* I ACKNOWLEDGE your question, *Eleutherius*, to be pertinent, and not unseasonable. And, I presume, you will not be surprized, that a person accused of scepticism answers it by declaring that he undertakes not to demonstrate, that cold must be a privative or negative quality, and thinks it sufficient for his turn, to shew that the arguments brought to evince it to be a positive one, are not concluding. And, since you have already diverted *Themistius* from beginning so soon as he intended, it will not be amiss, that I continue that suspension a little longer, to prevent, what I know we both hate, verbal controversies; which yet may very easily spring from undetermined acceptations of words, as ambiguous as I have observed heat (of which I now make cold but a privation) to be.

WE may therefore consider, that the word heat being made use of to signify, as well the operations of that quality upon other bodies (as when the heat of the fire makes water boil, or that of the sun melts wax, and hardens clay) as its operations upon the sense of man (as when a moderate degree of heat is said to cause pleasure, and an excessive one to produce pain); this term, I say, as Mr. *Boyle* also has somewhere noted, may be employed sometimes in a more absolute and indefinite sense, and sometimes in a more confined and respective sense; in the latter of which, it is estimated by its relation to the organs of feeling of those men that judge of it. Upon which account, men are wont to esteem no body hot, but such an one, the agitation of whose small parts is brisk enough

enough to encrease or surpass that of the particles of the organ that touches it; for if that motion be more languid in the object, than in the sentient, the body is reputed cold; as may appear by this, that if the same person put one of his hands, when it is hot, and the other when it is cold, into lukewarm water, that liquor will feel cold to the warm hand, and warm to the cold.

*Eleuth.* So that, according to this doctrine, methinks, one may, for brevity sake, conveniently enough apply to your two fold notion of heat, those expressions which some schoolmen employ about certain qualities, of any of which they say, that it may be either materially or formally considered. And by analogy to their doctrine, since heat is a tactile quality, and, as such, imports primarily a relation to the organ of touching, that relation, with what depends upon it, may pass for that, which is the *formale*, in the quality, called heat; and its effects and operations upon other bodies, may supply us with a notion of heat, materially taken.

*Carn.* I do not always quarrel, *Eleutherius*, with terms borrowed from the schools, if they be as much more short and expressive than others, as they are more unusual, or even barbarous. But there is another distinction of heat, partly grounded upon that already proposed, which, because it may be of use in our future discourse, will not be unfit to be here intimated. For we may consider, that though, for the most part, a hot body is taken in the vulgar sense, for that, wherein the degree of heat is sensible to our organs of feeling; yet, in a looser sense, and which, for distinction sake, we may call philosophical, because concluded by reason, though not perceived by sense, a body may be conceived not to be destitute of heat, even when the degree of that quality is not great enough to be felt by the touch; provided it can produce, in some degree, those other operations, which, when more intense, are acknowledged to proceed from manifest heat. For elucidation of which we may alledge, that in very frosty, and yet clear weather, the sun may be judged to warm the air, when it melts snow, and thaws ice; though, perhaps, many men, especially of tender constitutions, feel in their fingers and toes much stiffness, and more pain, upon the account of cold. To this I may add the common observation, if you grant the truth of it, that snow melts much sooner upon land newly turned up by the plow, than, *cæteris paribus*, in the neighbouring ground; which argues a warmth in that newly exposed earth; though, according to the touch, it would questionless appear cold. But we may be furnished with a clearer, and more pregnant instance, by but recalling to mind what was just now mentioned of the warmth of tepid water, which was not to be felt by a hot hand, but produced there a contrary sensation of cold. Which instance I therefore scruple not to repeat, because it affords an experiment in favour of that premised distinction, which, I think, may also have this ground in reason, that a considerable heat is often requisite to be sensible to our hands, &c. which are continually irrigated with the circulating blood that comes very warm out of the heart, and enlivened by animal spirits plentifully supplied from the brain.

If *Eleutherius* thinks fit to accommodate this distinction in the vulgar, and in the philosophical sense to his heat, formally and materially taken, I leave him to his liberty. And I shall also leave it to you both, Gentlemen, to accommodate to cold, *mutatis mutandis*, as they speak, what has been said about the distinctions of heat; because, I fear, *Themisius* thinks himself to have been too long detained already from proposing his arguments, which he may now begin to do as soon as he pleases.



## SECTION II.

*Them.* I WILL then, with your permission, begin with that argument of *Gassendus*, which I am able to give you in his own words; because, upon the occasion of Mr. *Boyle's* book, I made a transcript of what he says, to evince the positive nature of cold; and having the transcript yet about me, it is easy for me to tell you, that it is this \*:  
*Is sunt frigoris effectus, quales habere privatio, quæ actionis est incapax, non potest.*

This argument, though he begins not with it, I choose to make the first, because I think it of such weight, that, though it were the only one he could alledge, it would serve his turn and mine, since it is drawn from the effects of cold, which, though he mentions them but in few and general words, experience shews to be both so manifold and so considerable, that if *Carneades* employ an hundred times as much time to answer the argument they afford, as I have done to recite it, he will, I think, do no more than would be necessary, and perhaps not enough to be sufficient. For cold affects the organs of feeling, and sometimes causes great pain in them, condenses air and water, and breaks bottles that are too well stopped, congregates both homogeneous and heterogeneous things, increases hunger, checks fermentation in liquors, produces heat by antiperistalsis, in deep cellars, mines, &c. and yet freezes men and beasts to death, dismantles whole woods and forests of their leaves, and does (I know not how many) other feats; among which it is not the least admirable, though one of the most common, that it turns the fluid and yielding waters of rivers and lakes, and sometimes of part of the sea itself, not too far from the shore, into firm and solid ice, which is often in northern climates strong enough, not only to be travelled upon by merchants with their carriages, but to be fought upon by whole armies with their trains of artillery. From which, and other instances, it is manifest, that effects so numerous and great cannot proceed from a mere privation, or any negative thing, but require a considerable, and therefore sure a positive quality to produce them.

*Carn.* THIS objection, *Themistius*, is, I confess, a considerable one, and of more weight than any of the rest, if not than all of them put together; but, as I think it very worthy to be answered, so I think it very possible to be well answered; and to give you my reasons for my so thinking, I shall distinctly consider in the argument the two particulars which it seems to consist of.

AND first we are told, that if cold be but a privation, it cannot be the object of sense. To clear this difficulty, which, I know, you will think it very hard, if at all possible to do, I must beg your leave to observe something about sensation in general; not as designing an entire and solemn discourse of that subject, but because the particular remark I am about to make is necessary to the solution of our present difficulty. I observe then, that that, which, at least in such cases as we are speaking of, produces in the mind those perceptions which we call sensations of outward objects, is the local motion caused by means of their action upon the outward organs in some internal part of the brain, to which the nerves belonging to those organs correspond; and the diversity of sensations may be referred to the differing modifications of those internal motions of the brain, either according to their greater or lesser celerity, or other circumstances, as our friend Mr. *Boyle* has somewhere exemplified in the variety of sounds; whereof some are grave, some sharp, some harmonious and pleasant, some jarring and offensive; and yet all this

\* Vid. *Gassend. Physicam. Sect. I. Lib. VI. cap. 6.*

strange variety proceeds from the variations of those strokes or impulses which the air, put into motion by sonorous bodies, gives to the ear.

To this it will be consonant, that as the air, or rather the mind by the intervention of the air, is differently affected by a very grave sound, and a very acute one; though the former proceed from the want of that celerity of motion in the undulating air, which is to be found in the latter; which slowness, or imminution of motion, does, as such, participate of, or approach to the nature of rest; so in the sensory of feeling there may, upon the contact of a cold body, be produced a very differing perception from that, which is caused by the contact of a hot body; and this, though the thing perceived, and by us called coldness, consists but in a lesser agitation of the parts of the cold body, than of those of the hot body, in respect of our hands or other organs of feeling.

AND this leads me, for the farther clearing of this matter, to represent to you, that since it is manifest, that bodies in motion are wont to communicate of their motion to those more slow bodies they happen to act upon, and to lose of their own motion by this communicating of it; since this, I say, is so, if, for instance, a man take a piece of ice in his hand, the agitation of the particles of the sensory will, in good part, be communicated to the corpuscles of the ice, which, upon that account, will quickly begin to thaw; and the contiguous parts of the hand losing of the motion they thus part with to the ice, there needs nothing else to lessen the agitation they had before. And there needs no more than this slackening or decrement of agitation, to occasion in the mind such a new and differing perception, as men have tacitly agreed to refer to coldness.

*Eleuth.* It seems by this discourse, *Carneades*, that you think that sensation is properly and ultimately made in, or by the mind, or discerning faculty; which, from the differing motions of the internal parts of the brain, is excited and determined to differing perceptions; to some of which men have given the names of heat, cold, or other qualities. So that, according to you, if a considerable change or variation be made in the most ordinary, or in the former motion or modification of motion of the parts of a sensory, and consequently of the parts that answer them in the brain, new sensations will be produced, whatever the cause of this alteration be, whether privative or positive.

*Carn.* You do not misapprehend my thoughts, *Eleutherius*, and what you say gives me a rise to illustrate this matter yet a little farther, by observing, that the sensories may be so accustomed to be affected after a certain manner by those external objects, whose operation on them is very familiar, or perhaps almost constant, that the privation, or the bare imminution of the wonted operation-leaves the parts of the sensory, for want of it, in a different disposition from what they formerly were in; which change in the sensory, if it be not too small, will be attended by a perception of it in the mind. To declare and confirm this by an example, we may consider, that though darkness be confessedly a privation of light, and the degrees of it gradual imminutions of light; yet the eye, that is, the perceptive faculty, by the intervention of the eye, may well enough be said to perceive both light and darkness, that is, both a positive thing and the privation of it. And it is obvious, that the motion of a shadow, which is a gradual privation of light, is plainly, and without difficulty, discoverable by the eye; of which the reason may be easily deduced from what I have been lately saying. And to shew you, that there is on these occasions such a change made in the organs of seeing, as is visible even to by-standers, I shall need but to appeal to the experiment of making in the daytime a boy or girl look towards an enlightened window, and then towards an obscure part of the room; for when the latter comes to be done, you will plainly perceive, that for want of such a degree of light as was wont to come in at the pupil, and straighten a  
little



little that perforation of the uvea; that round circular hole, or, as you know they call it, apple of the eye, will grow very manifestly larger than it was before, and then it will appear again, if the eye be exposed to a less shaded light.

This observation may be seconded by what happens to a man, when coming out of the sun-shine, where the sun-beams much contract his pupil to shut out an excessive light that would be offensive to the organ, he comes presently into a dark room, where he must continue some time before he can see others, as well as he is seen by them, whose pupils have had time to be so enlarged, as in that darker place to let in light enough to make objects visible to their eyes, which are not so to his, whose pupils are yet contracted by the light they were but just before exposed to. To this I might add divers other phenomena, explicable upon the same grounds; but I shall rather choose to relate to you an uncommon accident, which happening to eyes somewhat unusually disposed, does more remarkably discover what alteration darkness, or a privation of light, may have upon those organs. I know a very learned man, who is no less studious of mathematicks, and other real parts of knowledge, than skilled in those, which are taught of the schools; this virtuoso, who seemed to me to have something peculiar in his eyes, confessed, and complained to me, that if he come, though but out of a moderate light of the open air, into a room that is any thing dark, he does not only feel such an alteration, as other men are wont to do on the like occasion; but is so powerfully affected by it, that he thinks he sees flashes of fire before his eyes, and feels a troublesome discomposure in those parts, that sometimes lasts an hour or two together, if he so long continue there.

*Eleuth.* I know not, *Carneades*, whether after this you will think it any great confirmation of your opinion, that *Aristotle* has somewhere this saying, that, *Oculus cognoscit lucem & tenebras*.

*Carn.* I THANK you, *Eleutherius*, for so pertinent an allegation, though not for my own sake, yet for theirs that will more easily receive a truth upon the testimony of *Aristotle*, than that of nature. And now, I hope, that *Themistius* will consent, that, dismissing the argument hitherto examined, we proceed to the next.

### SECTION III.

*Them.* SINCE you will have it so, I shall comply at present, and the rather, because, not only I foresee there will be occasion to speak of it again, but because you experimental philosophers that are wont so much to cry up the informations you think you receive from sense, sometimes, in spite of contrary dictates of reason, will, I hope, be prevailed with by the argument I am about to propose, which is so manifestly grounded upon sense, that without denying that we do feel what we feel, we cannot deny cold to be a positive quality. For thus *Gassendus* most convincingly argues; *Cum per hyemem immittimus manum in labentis fluminis aquam, quod frigus in ea sentitur, non potest dici mera privatio, aliudque prorsus esse apparet sentiri aquam frigidam, & sentiri non calidam. Et fac eandem aquam gelari, sentietur haud dubie frigidior: an dices hoc esse nihil aliud quam minus calidam sentiri? Atqui calida jam antea non erat: quomodo ergo potuit minus calida effici?*

*Carn.* I WILL not say, *Themistius*, his argument is not specious, but you, perhaps, or at least *Eleutherius*, will not affirm it to be more than specious, if you please to consider, with me, two or three things that I have to suggest about it.

*Of the Positive or Privative Nature of Cold.*

AND first, to shew *Themisius*, that, whatever he was just now intimating, experimental philosophers do not prefer the immediate impressions made on the senses to the dictates of reason, though they think the testimony of the senses, however sometimes fallacious, much more informing than the dictates of *Aristotle*, which are oftentimes, and that groundlessly, repugnant to them; I will represent to you, that the organs of sense, considered precisely as such, do only receive impressions from outward objects, but not perceive what is the cause and manner of these impressions, the perception, properly so called, of causes belonging to a superior faculty, whose property it is to judge, whence the alterations made in the sensories do proceed, as may easily be proved, if I had time and need to do so, by many instances, wherein the senses do, to speak in the usual phrase, mis-inform, and, as far as in them lies, delude us, and therefore must be rectified by reason. As when the eye represents a straight stick, that has part of it under water, as if it were crooked; and two fingers, laid cross over one another, represent us a single bullet, or a button, rolled between them, as if there were a couple; so that it is very possible (for I forbear saying it is true, having not yet proved it) that though the sensory be very manifestly, and vehemently, affected upon the contact of cold water, or other cold bodies; yet the cause of that impression, or affection, is, and may be judged and determined by reason to be, other than that which the sense may to an inconsiderate person suggest. As when a child, or one that never heard of the thing before, first sees a stick, whereof one part is in the air, and the other under water, he will presently, but erroneously, conclude that phenomenon to be caused by the sticks being crooked or broken.

NEXT we may consider, that sensations may, in divers cases be made, as well from alterations that may happen in the internal parts of the body, as from those that are manifestly produced in the external organ by external objects and agents; as may appear by hunger, thirst, the titillation of some parts of the body, barely upon venereal thoughts, and (which belongs directly to our present argument) the great coldness that we have known hysterical women complain of in their heads and backs, and the great and troublesome degree of cold, which we every day observe, upon the first invasion of the fits of agues, especially quartans; which troublesome symptoms, that sometimes last for several hours, are therefore commonly called the cold fits.

AND now it would be seasonable, for me to call upon you to remember (and add to what I have now said) that, which, at the beginning of our conference, I took notice to you of, about sensation in general; if I did not presume that those things are yet fresh enough in your memory, to allow me to proceed directly to answer the objection, which I shall do, though not like a school-man, yet like a naturalist, by giving an account of the proposed phenomenon, without having recourse to that hypothesis, which it is urged to evince.

I OBSERVE, then, that though in the respective sense above-mentioned, water, wherein the objection supposes the hand to be plunged, be cold, in regard its parts are less agitated, than the spirits and blood harboured in the hand; yet, in a philosophical sense, it is not quite destitute of heat, since it is yet water, not ice, and would not be a liquor, but by reason of that various agitation of its minute parts, wherein fluidity, a quality essential to liquors, consists. Upon the score of this respective coldness of the water, the hand is refrigerated; for the spirits and juices of that organ meeting in the water, with particles much less agitated than they are, communicate to them some part of their own agitation, and thereby lose it themselves, upon which decrement of wonted agitation such a change is made in the sensory, and, though not so manifestly in some other parts



parts of the body, as is perceived by the animadversive faculty under the notion of coldness; sensation (whatever obscure definitions are wont to be given of it) being indeed an internal perception of the changes that happen in the sensories.

And if now, as the objection supposes, the water wherein the hand is plunged comes to be more refrigerated than before, the spirits, blood, and other parts of the hand, finding the aqueous corpuscles more slowly moved than formerly, must, according to the laws of motion (according to which, a body, that meets another much more slowly moved than itself, communicates to it more of its motion, than if it were less slowly moved) transfer to them a greater measure of their own motion, and consequently themselves come to be deprived of it; and upon this increase of the slowness of motion in the parts of the hand there follows a new and proportionable perception of the mind, and so a more vehement sensation of cold. But though it be not to be admired, that the bare slowness of motion in the object should be discernible by sense, albeit it seems to participate of rest, which, with you, passes for a privation, since the ear perceives, when a voice grows faint, and when a sharp sound degenerates into a flat one; and we can perceive by the hand (abstracting from heat and cold) the celerity or slowness of bodies, that in their passage strike upon it, as for instance, of winds, or streams; yet this is not the only thing I think fit to be taken notice of on this occasion. For I consider farther, that besides the most consistent and stable parts of the hand, there are, from the heart and the brain, fresh blood and spirits continually transmitted to the hand; and the former of these, the blood, is, according to the laws of its circulation, and after it has received a great change in the much refrigerated hand, carried back through other parts to the heart; whence it is, in the same circulation, distributed to the whole body. To which may be added, that when the great refrigeration of the hand happens, external agents may contribute to the effects of it, as I shall by and by have occasion to shew.

If then you please to remember, that upon the turning one's eye to the dark part of a room less enlightened than the window, though darkness be but a privation, and though the obscurity of that part be not absolute, but consist only in a less degree of light; yet the action of the spirits, and other parts of the body, is so changed, upon occasion of the light's acting more faintly than was usual upon the organ, that the pupil is immediately and manifestly dilated, and in some cases, as in that which I mentioned to you of a learned man, much considerabler effects ensue; you will not wonder, that, where not only the spirits, but the blood (whence those spirits are generated) that circulates through the whole body, and upon whose disposition all the other parts so much depend, is very much disaffected, there should be felt a great alteration in the hand, which is the most immediately exposed to the action of the cold water. And for the reasons newly given, it ought to be as little strange, that in other parts of the body, the disordered, and not circulating blood, should have its wonted action on them considerably altered; since the more stable parts, and especially those external ones; that are most exposed to the cold, have their pores straightened, and consequently their texture somewhat altered; on the same occasion on which the wonted agitation of the spirits, with the particles that compose the blood, is notably lessened. And that such causes may produce great effects in a human body, you will be more prone to admit, if you consider the disorders that happen in the cold fit of an ague, and oftentimes, upon the shutting up of those excrementitious steams that are wont to be discharged by insensible transpiration; to whose being stopped in the body, by the constriction of the pores, which chiefly happens through cold, some learned physicians, especially the famous *Sennertus*, impute the cause of most fevers, as indeed experience itself does but too frequently shew it to be guilty of many.

*Phil.*

*Phil.* I CONFESS, *Carneades*, you have said some things, that I thought not on before; but yet *Gassendus's* argument seems to be such, that I fear it will be hard to hinder many from saying, that if cold be but a privation of heat, it is a privation of a strange nature; for it may be introduced into bodies that were not hot before, nay, in some cases, into such as are naturally cold, and also by consequence must have been put into a preternatural state to be at any time hot.

*Carn.* THIS objection, *Philoponus*, being in effect so much the same with that of *Gassendus*, that it differs from it but in the dress you give it, it will scarce require a peculiar and distinct answer; and therefore, as soon as I have reminded you of the distinction that we have formerly made of the vulgar and philosophical sense of the word cold, I shall need to alter but a little what I said before, by telling you, that since fluidity consists in the various agitation of the insensible corpuscles of a liquor, and that heat consists in a tumultuary, but a more vehement agitation of the insensible parts of a body, and so, that hot water scarce differs otherwise than gradually from that, which is cold to sense; if cold be taken in the larger and philosophical sense, it may well be said, that as long as water retains the form of water, and so continues to be a fluid body, though it may be very cold to the touch, yet it is not absolutely or perfectly cold, and therefore is capable of a farther degree of coldness, which it receives when brought to congelation; for till then it was not destitute of those agile corpuscles that were requisite to keep it fluid; and till then, *Gassendus* himself must acknowledge that it was not absolutely or perfectly cold; because he, as you may remember, did in his former (but lately-mentioned) argument ascribe the glaciation of water to the invasion of those that he calls corpuscles of cold.

*Eleuth.* GIVE me leave to add, *Carneades*, that it is not every glaciation itself that brings liquors to be perfectly cold in the philosophical sense of that expression, and quite expels or subdues all the agile particles that were in the water before it was turned into ice. For I think, that to effect this change, it is sufficient that so many of these restless particles be destroyed or disabled, that there remains not enough of them to keep the water in a state of fluidity, so that the surplusage may yet continue in the frozen liquor, and whilst they are there, perform several things, as the making it evaporable in the air, and even odorous, and by their recess or destruction the ice may grow yet more cold. And as this notion suits very well with the differing degrees of hardness, that we find in differing portions of ice, sometimes upon the account of the matter (as frozen water is harder than frozen oil) and sometimes upon that of the different degrees of cold in the same water, or other matter (as our friend somewhere observes) so it may be highly confirmed by an experiment I saw him make, but that is not yet published.

THE sum of the experiment was this; that he first put an hermetically sealed thermoscope into a glass, broader at the top than at the bottom, and greased the inside with tallow, that ice might not strongly stick to it. In this glass was put water, more than enough to cover the ball of the instrument; and that water being warily frozen, notice was taken, whereabouts the tincted spirit of wine rested in the stem; after which, the instrument and the ice being removed into the open air, upon an exceeding frosty morning, the ice was taken off from the ball, and presently after, the tincted liquor, as the maker of the trial expected, subsided a pretty way (the length of the instrument considered) below the former mark; which argued that he rightly guessed that such a degree of cold as is sufficient to turn water into ice, may not produce a body perfectly cold; this ice itself keeping the inclosed ball, in a sense, warm, by fencing off the air, which, at that time (even in our temperate clime) by the effect, appeared to be colder than the very ice. And, methinks, it may strengthen *Carneades's* discourse, to represent, that  
there



there is no sufficient cause, why many things that are reckoned among privations or negations, by the Peripateticks themselves, as well as cold is by *Carneades*, may not admit of degrees; as may be exemplified by deafness, ignorance, and divers other things. And to bring a case, not very unlike that under consideration, we may take notice of a total eclipse of the moon, which you know always happens when she is at the full. For darkness in the air being acknowledged to be a privation or negation of light, when the earth, interposed between the moon and the sun, has eclipsed her, for instance, nine digits (as astronomers speak) men generally complain of darkness in the air, though there remain a considerable part of the discus, or the hemisphere of the moon, obverted to us, yet enlightened by the sun; but when the interposed earth proceeds to cover the remaining three digits, and so makes the eclipse total, the darkness also is said and esteemed to be much increased; nor would men otherwise be persuaded, though *Themistius* should tell them that the air cannot have grown darker, though it were dark before; and indeed though the air was more and more darkened in proportion to the increase of the eclipse, yet it was never completely darkened till it became total. But I fear I dwell too long upon one argument.

#### SECTION IV.

*Eleuth.* LET me therefore, *Carneades*, sum up what I take to be your doctrine, and tell these gentlemen, that I think you do not look upon the sensation of cold as a thing effected by an entire privation, properly so called, and considered as such; but that, according to you, that slowness of motion in the particles of cold water, which the hand finds when it is thrust into that liquor, does occasion the spirits, and the corpuscles of the blood, to part with to those of the water a considerable share of their own superfluous agitation, whereby they lose it themselves; upon which is consequent a perception of this change made in the hand, which, if it be very great, is also frequently accompanied with some sensible change in other parts of the body, occasioned chiefly by the frequent returns of the circulating and highly refrigerated blood to the heart, whence it is dispersed to the whole body. According to which doctrine, the sensation of cold is but a perception of the lessened agitation of the parts of the hand, either stable or fluid, especially of the blood; which alterations are in great part produced, not by the coldness of the water, as cold is a privation, but from the new modification of the action of the blood and spirits upon the nervous and membranous parts, the constriction of whose pores concurs to that modification. And if I do not misunderstand your opinion, *Carneades*, methinks it may be confirmed by this, which I have known observed by experienced surgeons, that by too strict ligatures unskilfully made, an arm, for instance, may be gangrenated; in which case, all the proper and immediate effect of the ligature is but the constriction of the part, though that constriction being unusual and excessive, it proves the occasion of the mortifying of the hand and arm, by hindering the free and usual access of the blood and spirits to that limb; upon which, by the depraved action of the parts of the body one upon another, and the concurrence of external agents, there ensues a mortification or gangrene of the part, which, if due remedies be not timely employed, is communicated to other parts, and kills the man.

*Carn.* WHATEVER become of your instance, *Eleutherius*, I thank you for your readiness to propose it in favour of my hypothesis, which you will easily judge not to be much concerned in the close of the excellent *Gassendus* his argument, for the positive nature of cold. For though these words of his —

*Them.*

*Of the Positive or Privative Nature of Cold.*

*Them.* You may save yourself the trouble of naming of them now, since, whatever they may seem to you, I profess I look upon them, as containing a distinct argument, which I shall therefore propose in its due place hereafter; but in the mean time, and before we leave the argument you would have us dismiss, give me leave to remind you, *Carneades*, of some part of your former discourse, and to take thence a rise to tell you, that you who told us that we ought not to consider the operations that qualities have upon our own sensories only, but also what they do to other bodies, will, I hope, allow me to demand, how a privation, or, if you will, how an imminution of motion can produce the hundredth part of those effects, which we daily see produced by cold in the bodies that are about us.

*Carn.* I thought, *Themistius*, I had intimated to you already what might have prevented your question; but since I see it is otherwise, you shall not find me backward to explain myself a little more fully. I do not pretend, that either an absolute privation of motion in a body, or a slowness of motion in the parts of it, is, as such, the proper efficient cause of the effects, vulgarly but unduly ascribed to cold alone; for, in my opinion, cold is rather the occasion, that the true efficient cause of such effects, which, I think, are properly to be ascribed to those physical agents, whose actions or operations happen to be otherwise modified, than else they would have been upon the occasion of that imminution or slackness of agitation which they meet with in cold bodies, by occasion of which they are both deprived themselves of the agitation they communicate to such slow bodies, and thereby act no longer, as, were it not for that loss, they would, and by a natural consequence of this change, which is made in themselves, they do also, though less notably, modify the action of other bodies upon them: from which unusual alterations happening in a world so framed as this of ours is, and governed by such laws, respecting motion and rest, as are observed among bodies, there must, in all probability, result many new, and some of them considerable phenomena. For though quiescent bodies seem not to have any action, which among corporeal substances seems to be performed only by local motion; yet bodies quiescent themselves may concur to great effects, both by determining the motions of other bodies, this or that way, or by receiving their motion totally, or in part, and so depriving the formerly moving bodies of it. Thus the arches of a bridge, though immoveable themselves, by guiding the water of the river that beats against them, may occasion a rapid and boisterous stream, capable to drive the greatest mills, and perform more considerable effects, though the river, before it met with them, ran calmly enough, as is evident at *London* bridge, especially when the water is near a low ebb. And now I have mentioned water, I will add, that though water itself be not a quiescent body, but, being a liquor, has its parts in perpetual motion among themselves; yet since that agitation is exceeding slow, in comparison of the swiftness of a cannon-bullet, in respect whereof the calm surface of the water participates of the nature of a quiescent body, bullets themselves shot from out of guns elevated but little above the level of the water (upon which score they make but a very sharp angle with it); these bullets, I say, do not unfrequently rebound from the surface of the water, and consequently, even these so wonderfully swift bodies receive a new determination from it.

*Eleuth.* One may add, *Carneades*, to your instances, that, in a tennis-court, the wall, against which balls are strongly impelled by a racket, contributes much to the mischief that those balls do often to by-standers in the gallery, as the wall, though itself unmoved, gives a new determination to the moving ball, and by its resistance makes it rebound or reflect at an angle equal to that of the ball's incidence. And this concurrence of the wall to such effects is the more evident, because of this other circumstance, which also befriends your opinion, that, if the impelled ball, instead of hitting against the wall, hits against



against the net, this, by yielding, deprives the ball of its impetus, and hinders the reflection that would else ensue.

*Carn.* You have, I confess, somewhat prevented me, *Eleutherius*, but yet not altogether; for though I was going to propose the example of a ball, yet it was in somewhat a differing way; for I was about to propose to *Themistius* the example of a ball, which, if it be forcibly and perpendicularly thrown against the hard ground, has its determination so altered, that whereas it moved before towards the centre of the earth, it immediately, with almost the like swiftness of motion, tends directly upwards. And if on the other side you throw the ball, not against a hard, but against a muddy piece of ground, it will not rebound, losing its own motion, by communicating it to the parts of the yielding mud; as may be in some measure illustrated by the great commotion made in a small pond of water, when a ball (or a round stone) being but gently let fall upon the surface of it, has its motion thereby deadened, and transferred to the parts of the liquor, which, perhaps, will be visibly agitated at the remotest brink of the pond.

*Eleuth.* THESE examples may conduce much to explicate your doctrine, *Carnades*; but since *Themistius* himself was so equitable a while ago, as to allow you much time to defend such a paradox as yours against *Gassendus's* argument, I shall, with your leave, of which I doubt not, to the examples already mentioned, add this one more. Suppose upon a stream that runs through some town, which is not very rare, there were built a number of differing mills, some for the grinding of corn, others for the fulling of cloth, others for the moving of bellows to melt ores and metals, others for forging of sword-blades, others for making of paper, and others for other uses; and suppose, that an enemy coming to besiege this town should successfully imitate *Cyrus's* stratagem, when by suddenly diverting the course of *Euphrates* he took *Babylon*; would it not be consequent to this diversion of the water into some lower place, and this ceasing of the stream to run in its former channel, that the action of all these mills, by which so many differing operations were performed, must of necessity cease too? though the besiegers do not produce this change by any positive and direct violence that they offer to the mills, but only by hindering them from receiving the wonted impulses which were requisite to keep them in motion.

*Carn.* I DISLIKE not your instance, *Eleutherius*, which yet will not altogether render useless what I was going to say about a wind-mill, which will illustrate one part of my doctrine, for which your water-mill does not seem to have been intended. And, that this example may the better do so, I will suppose a wind-mill to be built in some low place near the bank of your stream, which stream we will suppose to be liable, as some others are, upon the falling of great and sudden rains upon the neighbouring hills, to overflow its banks, in case the increase of the water be not then hindered by the wind-mill's lifting up constantly some parts of it, and conveying it away by pipes or otherwise; and then let us suppose, what really sometimes happens, that the wind should so cease, that there should not blow any wind strong enough to move the sails for a great while together; will it not hence manifestly follow, that by reason of this absence of the wind, which absence has the nature of a privation or negation of a stream-like motion in the air, not only there will be a ceasing of those effects and operations, whatever they were, that were wont to be performed within the mill itself, but also there will be a durable intermission of that main work of the mill, whereby it carried off such a quantity of water; which work ceasing with the wind, whilst the flowing in of the water does not cease too, but continues, as formerly, the still-increasing water must bear down or overflow its wonted banks, or other boundaries, and by its unruly effusions drown the neighbouring parts, and produce the disorders, that is, the new phenomena naturally consequent

quent to an inundation made by such a quantity of water. And if the water conveyed away by means of the mill, through pipes or channels, were employed to water grounds, or other particular uses, the growth or fertility, at least of the vegetables, that water was requisite to nourish, or the other uses, to which it was necessary, must consequently be much, if not totally, hindered.

*Phil.* I KNOW not, whether we may not refer to the subject of your discourse, what may be observed in paralytick affections, where a little viscus or narcotick humour, obstructing, or otherwise disaffecting one part of a nerve, though its proper and immediate action be only to hinder, or weaken the spirits, that were wont, in competent plenty, to pass freely along the nerve to the muscles whereto it leads; yet the action of the other parts of the body, and the relaxation of the fibres, do oftentimes produce a tremulous motion in the limbs, and particularly the hands; and sometimes also the mouth, neck and other parts, are drawn awry in an odd and frightful manner.

*Carn.* THOUGH I approve of *Philoponus's* fancy, yet I think a more quick and notable instance to the same purpose may be taken, from what happens to birds, and rats, and cats, and such kind of warm animals, in Mr. *Boyle's* engine. For as the air by the agitation of its parts, or that of some ethereal substance that pervades it, entertains the fluidity of water, and other aqueous liquors; and when that agitation is hindered, or too much lessened, water ceases to be fluid, and upon that divers violent effects ensue, wont to be ascribed to glaciation: so the bodies of warmer animals, having been born in the air, and perpetually exposed to the action of it (though that be seldom heeded) when being placed in the receiver of the air-pump, and by the operation of that instrument, which withdraws the former air, and keeps out the new, the air, that was wont continually to act upon them, is kept from doing so any longer, though this absence, or not touching of the air, be but a privative or negative thing, yet, by reason of the structure of the animal, his spirits and humours, assisted by the concurrence of more general causes, are brought to act so differently from what they were wont to do, that the blood and juices swell, the stomach vomits, the animal grows faint and staggers, the limbs, and at length the whole body are convulsed, the circulation is stopped, and at last the animal killed; and all this done in a very few minutes of an hour, without the visible intervention of any positive agent.

*Eleuth.* WHAT you say, *Carneades*, concerning the quick and violent death of warm animals in Mr. *Boyle's* engine, puts me in mind of an experiment I saw made in that instrument upon cold animals, which, methinks, may well illustrate the comparison we lately employed of a wind-mill. For as those great artificial engines lose their motion, and the operations depending on it, if that stream of air, we call the wind, be held from keeping them going; so insects, and some other cold animals, have their differing motions so dependent upon the contact of the air, that, as soon as ever they are deprived of it (by the engine we are speaking of) divers sorts of them will lie moveless, as if they were dead; and I have known several of them, that were put in together, continue in that state for many hours, as long as it pleased our friend to withhold the air; but when once he thought fit to let a stream of air enter the receiver, these seemingly dead animals, as worms, bees, flies, &c. like so many little wind-mills of nature's (or rather, her great author's) making, were set a moving in various manners (as creeping, flying, &c.) suitable to their differing species.

*Carn.* So that, to sum up, in a few words, the result of these instances, and the rest of the past discourse on the same subject, it appears by what has been said, that the effects undeservedly ascribed to cold need not, in our hypothesis, be referred to a privation, but to those positive agents, or active causes, which, by their own nature, are determined



determined to act otherwise, or suffer otherwise from one another, in cases, where there is a great hindrance, or ceasing of wonted agitation, than where there is not.

SECTION V.

*Them.* It may, perhaps, now be time to put *Carneades* in mind, that, in what he has been discoursing all this while, he has proposed answers but to a couple of *Gassendus's* arguments, and left the rest untouched.

*Carn.* I should readily grant, *Themistius*, that I have dwelt too long upon so few arguments, if I did not hope, that by fully answering them, and giving the company a particular account of my notions concerning cold, I might very much shorten and facilitate the remaining part of my task, which engages me to return answers to the other arguments you speak of, the grounds of solving which, I think, I have already laid in the past discourse. And therefore you may go on to propose the next argument of *Gassendus*, as soon as you please.

*Them.* And I shall do it, *Carneades*, in that learned man's own words, which I well remember to be these: *Fac manum immitti in aquam nunc calidam, nunc frigidam; quomobrem manus intra istam, non intra illam refrigeratur? An quia calor manis intra frigidam retrahitur, manisque proinde relinquitur calida minus? At, quidnam calor refugit, quod intra frigidam reperiatur? nonne frigus? at si frigus est tantum privatio, quidnam calor ab illa meluit? privatio sane nihil est, atque adeo nihil agere, unde ejus motus incutiatur, potest.* *Gassend. Lib. VI. cap. 6.*

*Carn.* This objection, *Themistius*, may indeed puzzle many school-philosophers, but will easily admit an answer in my hypothesis. For that does not oblige, or so much as tempt me to ascribe (as a Peripatetick would do) to a mere quality (for such is heat) both a knowledge of its danger, and a care, and skill, to preserve itself from its enemy, the cold, by a retreat inwards. For, agreeably to what I lately delivered, it is obvious for me to explicate the phenomenon thus: when a man puts his hand into warm water, the agitation of the corpuscles of that liquor surpassing that of the spirits, blood, and other parts of his hand, cannot but excite in him a sense of heat; but when he puts the same hand into cold water, the case ought to be much altered, not by any imaginary retreat of the spirits, but the communication of motion, by other parts, to the surrounding water, by which means, there must be in the hand a great lessening of the former agitation of its parts, the perception or sense of which decrement of motion is that which we call the feeling of cold.

*Eleuth.* I THINK, indeed, *Carneades*, that though this argument may be considerable against those, that the learned framer of it might have in his eye, it is but invalid against you. But can you as well decline the force of that other objection, which *Gassendus* more insists on, and which seems as directly to oppose you, as any other adversaries of his hypothesis?

*Them.* I PRESUME, *Eleutherius*, you mean that cogent argument, which *Gassendus* proposes, and prosecutes more fully than the rest, deducing it from the way of artificially freezing water by a mixture of snow and salt, placed about the outside of the glass, that contains the liquor. For, from this practice, he rationally concludes, that since this frigorick mixture is, through the glass, able to freeze the water into ice, it may as justly be affirmed to act by corpuscles of cold, as fire can be to act by calorick corpuscles, when kindled coals, placed on the outside of the glass, make the contained water boil. And this cogent argument will, I hope, prove the more satisfactory to *Carneades*,

since it is not drawn from what he would call a disputable peripatetick notion, but from the same quiver, whence he affects to take his shafts, experience itself.

*Carn.* I FREELY acknowledge, gentlemen, this argument to be very plausible; but that it is clear and cogent, I must not grant, till I be better satisfied that it is so.

AND I shall scarce think it as evident, that ice, and salt, act by a positive quality, as that burning coals do so, though cold seems as well to be produced by the former, as heat by the latter. For innumerable experiments shew, that heat, in the fire especially, is a positive quality, consisting in a tumultuary and vehement agitation of the minute parts of the body, that is said to be hot, and producing also in the bodies, that it is communicated to, a local motion, which is manifestly a positive thing. This is so evident, in the heating of bodies by mere attrition, the smoking and melting of divers bodies in the sun-beams (especially at fit times of the day, and year) the sudden boiling and dissipation of water, oil, &c. dropped on a red-hot iron, and many other obvious instances, that it were a needless work to go about to prove it, especially, since both *Themistius's* Peripateticks, and *Gassendus* himself, who so often disagree about other things, agree in confessing, that heat is a positive quality.

*Them.* BUT remember, *Carneades*, that the grounds on which they do so, are the same on which *Gassendus* justly builds the proposition, that cold also is a positive quality.

*Carn.* I DID not forget that, *Themistius*; for I was about to subjoin to what I last said, that it is evident, not only by the confession of my adversaries, but by that (which to me is much more considerable) of nature herself, proclaiming it in the instances I just now mentioned, that heat is a positive quality; whereas, that cold likewise is so, does not appear to me by the experiment of artificial congelations. For in this, all that is clear in matter of fact, is, that snow, or beaten ice, and salt, are put about a vessel full of water, or other aqueous liquor, and that, within a while after, this water begins to be turned into ice; but that this glaciation is performed by swarms of atoms of cold, that permeating the glass, invade and harden the liquor, is not perceived by sense but concluded by a ratiocination, the cogency of which I am allowed to examine, without affronting the certainty of sense, that not being concerned in the case. If then an intelligible way can be proposed of fairly explicating the phenomenon, besides that insisted on by *Gassendus*, the objection drawn from this experiment against my hypothesis will be invalid. And such an explication monsieur *Des Cartes* ingeniously gives in his me-

1. lib. me-  
teor. cap.  
3.  
teors: *Quia materia subtilis* (says he) *partibus hujus aquæ circumfusa crassior aut minus subtilis, & consequenter plus virium habens, quam illa quæ circa nivis partes hærebat, locum illius occupat, dum partes nivis liquefcentia partibus salis circumvolvuntur. Facilius enim per salis aquæ quam per dulcis poros movetur, & perpetuo ex corpore uno in aliud transire nititur, ut ad ea loca perveniat, in quibus motui suo minus resistitur: quo ipso materia subtilior ex nive in aquam penetrat, ut egredienti succedat, & quum non satis valida sit ad continuandam agitationem hujus aquæ, illam contrahere sinit.*

*Phil.* I LEAVE *Themistius* to consider, whether this explication be without exception; but I confess it is not without analogy, and that even amongst the four first qualities themselves. For when we chymists have a mind to dry (for instance) the calces, or precipitates, or other powders, from which we have filtrated the liquors we employ to wash or dulcify them, it is usual either to put the filters wherein these powders remain almost in the form of mud, or to spread the stuff itself upon brown paper, or pieces of brick, or chalk, which much hasten the exsiccation of the things laid upon them, not by any drying particles which they emit into the soft substances, but by imbibing the superfluous parts of the liquor, and thereby freeing from them the substances to be dried. And I remember I have seen our friend *Mr. Boyle*, by immersing a piece of soft crumb



crumb of bread into an actually cold liquor, that would hastily imbibe its aqueous corpuscles, and dry it in a minute, or two, of an hour, so as to make it feel hard.

*Eleuth.* THESE instances bring into my mind another chymical experiment, that I have seen made by the same gentleman, which was, that by putting into weak spirit of wine a sufficient quantity of salt of tartar, he quickly dephlegmed the spirit without distillation, or so much as heat. And this will the better illustrate the Cartesian explication, because it is manifest, by the change that will be made of the most part of salt of tartar into a liquor, that will not mix with the now dephlegmed spirit of wine, that the reason of the operation is, that the aqueous particles of the phlegmatic spirit, finding, it seems, more convenience, or facility, to continue their motion among the fixed corpuscles of the salt, than the vinous ones of the spirit, pass into the alkali, and dissolve it; and thereby desert the liquor, through which they were diffused before. And I know another saline body, that so unites with water, as not to be, by the eye, distinguishable from it, and yet is of such a texture, that water is so much less disposed to mingle with it, than with spirit of wine itself, that it will forsake the body it kept in agitation, to pass into this spirit; and so leave that which it kept in the form of a liquor before, to appear in the form of a consistent body; which instance comes some what nearer, than the former, to the experiment of glaciation.

*Carr.* THOUGH what you have recited, gentlemen, be not unwelcome to me, yet, I think, I can propose you an experiment fitter to elucidate the Cartesian explication. For, I remember, that our common friend, having a mind to shew that a small proportion of agile matter, invisibly diffused through a body, that would be otherwise consistent, may bring it to, and keep it in the state of fluidity; devised and shewed me the following experiment. He took camphire broken into small bits, and casting a convenient quantity of it upon aqua fortis, suffered it to float there, till, without heat, the camphire was dissolved into a liquor, and it looked and felt like an oil, which, though shaken with the aqua fortis, would emerge to the top again. If this oil were kept well stopped, that the spirits of the menstruum might not evaporate, it would (as he affirmed trial had taught him) continue long fluid, he having sometimes kept it a year, or two, or more. And that it is the agile spirits of the aqua fortis, that keep the camphire fluid, he has made probable by divers things, that I must not now stay to recite. And that the quantity of these agile particles is but small, I am induced to think by this, among other things, that when I have made a small parcel of but moderate aqua fortis turn a pretty proportion of camphire into oil, and separated that oil from it, I could, by casting fresh camphire on the same menstruum, reduce that also into the form of oil. Now, that these fluidifick spirits (if I may so call them) are not sensibly warm (no more than the Cartesian *materia calefactis*) in water, is manifest to the touch: and whereas I at first suspected, that the reason why the pouring of this oil into water doth presently reduce it into camphire again, might be the coldness of the water; I after thought, upon a farther information, that the reason rather was, that the nitrous spirits being disposed to pass out of the oil into the water, this liquor readily imbibed and diluted them, and consequently, disabled so many of them, that those that remained, could not do their former work any longer: since he had tried purposely, that the reduction of the oil into camphire would presently be made, though that liquor were not poured into cold water, but hot: so that the agitation that it received from the particles of the menstruum, though not to our touch sensibly warm, was much more efficacious, than that which it received from the heat of the water.

*Eleuth.* I know not, whether besides the instances that have been now proposed, one may not alledge such an argument also in favour of the Cartesian opinion about cold,

as would not be insignificant, though it should be made appear, that cold may sometimes be produced by, or upon the emission of corpuscles, that in some sense may be called frigorifick. For there may be corpuscles of such a nature, as to size, shape, and other attributes, as to be fit to enter the pores, and pierce even into the inward parts of water, and some other bodies, so as to expel the calorifick corpuscles they chance to meet with, or to clog, or hinder their activity, or on some other account, considerably to lessen that agitation of the minute parts, by which the fluidity of liquors, and the warmth of other bodies, are maintained. But even in such cases, though the agent, and the actions that produce coldness, be positive things; yet the nature of coldness itself may consist in a privation. As when a man is killed by a bullet, his death is effected by a positive, and even impetuous action, and yet death itself is but a privation of life. If also, in a dark room, a man cast cold water upon a burning coal, though the water act by its positive quality of moisture, and, by virtue of that, extinguish the fire, and, by that means, destroy the light, yet the darkness, that is consequent upon this action, is not a positive thing, but a privation.

## SECTION VI.

*Phil.* THE pause you here made, gentlemen, makes me think it seasonable to put the company in mind, that it begins to grow late, and therefore to call upon *Themistius* to produce what he has yet to alledge out of *Gassendus*.

*Them.* THE philosopher you have named, has indeed another weapon to destroy the error about cold, which he confutes. And this argument, like a two-edged sword, that cuts on both sides, does not only confirm what he maintains, but destroy the chief objection that can be made by his adversaries. The argument I speak of, he proposes in these terms: *Tametsi multa videantur ex sola caloris absentia frigescere, nihilominus nisi frigus extrinsecus introducatur, non tam profectò frigescere quàm decalescere sunt censenda. Esto enim lapis, lignum, aut aliquid aliud, quod nec calidum, nec frigidum sit, id ubi fuerit admotum igni calefiet sanè; at cum deinceps calor excedet, neque frigidum ullum circumstabit, non erit cur dicas ipsum frigesceri potius quàm minus calidum fieri, redireve in suum statum.*

*Carn.* WHETHER this contain not a dispute *de modo loquendi*, I shall leave the company to judge, by what I shall return in answer to it. I say then, that it seems to me, that there is in the discourse an obscurity, if not an ambiguity, though, I am confident, not affected by the candid *Gassendus*. But to answer as directly as I can; if we speak only of a coldness, as to sense, I see not, why water, or wood, or any such body, that is heated by the fire, may not, upon its removal thence, be said to grow cold, and not barely to *decalescere*, in our philosopher's sense of that word. For the heat and coldness of water, in reference to sense, consisting, as I lately shewed in this, that the particles of it are more or less agitated, than the hand that is immersed in it, they need nothing else to make the liquor grow cold, than such an immutation of the brisk motion of its corpuscles, that they cease to be as much agitated, as those of our organs of feeling: and if this already impaired agitation be still more and more lessened, the liquor will still grow colder and colder, without the help of any positive cause, until at length the agile parts, that kept it fluid, being quite expelled, or disabled, the form of the liquor comes to be exchanged for that of ice.

*Phil.* BUT what say you to that part of *Gassendus*'s argument, where he proposes an adaphorous body, which, when affected with an adventitious heat, would not grow cold



cold by the bare removal, or cessation of that heat, unless it were refrigerated by an agent, that were positively and actively cold?

*Eleuth.* I SAY, *Philoponus*, this supposition should not be made, and that I know of no such adiaphorous body. For since, as I have been obliged to inculcate, those bodies must be cold, as to sense, whose parts are less agitated than those of our hands, and consequently metals, stone, wood, and other solid bodies, and also water, wine, and all other unmingled liquors we know, being heated by the fire, will grow cold again of themselves, because the adventitious motion ceasing by degrees, either upon the recess of the igneous corpuscles, or the imparting of the extraneous agitation to the air, or other contiguous bodies, the stone, or water, &c. will again have so much fainter an agitation, than that of a man's sensory, as to be by him judged cold: and because almost all the species of permanent bodies here below, that are known, have, in what is called their natural state, a less degree of agitation in their insensible parts, than men's organs of feeling are wont to have, those bodies may be said to be naturally cold, and therefore ought not to be supposed to be indifferent to cold or heat.

*Phil.* BUT whether or no nature do really afford us an adiaphorous body; yet surely the mind is able to conceive one, and therefore *Gassendus* may be allowed to suppose such bodies, and *Carneades* may be obliged to answer what he argues upon that supposition.

*Carn.* IT is one thing to propose an adiaphorous body, as barely an intelligible, or a possible thing; and another, to give instances of it, as *Gassendus* has done in particular bodies, in which that indifference is not to be found. And it is this last kind of supposition, that I disallowed in *Gassendus's* argument. But if a body should be proposed, as adiaphorous in reference to heat or cold, I might say, without prejudice to my cause, that if such a body should be carried into a hot place, it might there grow warm; and if it should be removed back again, and kept, till it lost that new adventitious heat, it might rather *decalescere*, than grow cold as to sense. But the reason is, because it is not every degree of imminution of heat, that is able to denominate a body cold, but such a degree as reduces the parts of it to a fainter motion, than is at that time in those of our organs of feeling; and till this be done, or at least very near done, the proposed body is still (if I may so speak) in the state of heat, as to sense: which last words I add, because, that in reference to other bodies, it may then be notably refrigerated. As lead, that has but heat enough to keep it in fusion, may, by the pouring on of such water, as to a man's hand would feel hot, be brought to grow hard, which loss of fluidity is also the natural effect of cold, though perhaps, both the metal, and the liquor, be yet as to sense considerably hot.

*Eleuth.* So that, according to you, none of the kinds of bodies, that are actually known in nature, are adiaphorous as to sense. On which occasion let me note by the by, that the frequent variations of sense must render it but an uncertain standard of heat and cold: and upon supposition, that there were an adiaphorous body in reference to our sense; yet it would not be so in reference to all other bodies, or, in the phrase of our *Verulam*, speaking of heat, in *ordine ad universum*. And for what remains, the controversy grounded on *Gassendus's* argument seems to be rather verbal, than real, and may be determined, or composed, by settling the distinct acceptions of the words cold and heat.

## SECTION VII.

*Phil.* WHEREFORE I wish that we might not waste the little time that is left us, upon niceties of no greater concernment; and I think this short time would be better employed, if *Carneades* would be pleased to tell us a little more particularly, what he supposes to be the thing, that withheld *Mr. Boyle* from delivering an opinion about the nature of cold.

*Eleuth.* YET, methinks, it is but fair, that *Carneades*, who has all this while been confined to the answering another's arguments, should now take his turn to propose his own.

*Carn.* I FIND, in each of your motions, gentlemen, something so equitable, and so expedient, that I shall in part comply with both. And that I may hasten to do what *Philoponus* desires, I shall do no more than briefly point at two things, that may be alledged in favour of the hypothesis I defend. For if you reflect upon what we have already discoursed, we may take notice of things there, that will scarce be well accounted for by being ascribed to positive cold, but may be far better explained agreeably to our hypothesis. And must add, in the next place, that I, who sustained the person of a respondent, may pretend to have sufficiently discharged my office, if I have shewn the invalidity of all the opponents arguments; and it is his part, who asserts a positive thing in nature, to make it good, whereas he that denies it, needs not alledge any other reason why he does so, than the authority of that justly received axiom in philosophizing, *Entia non sunt multiplicanda absque necessitate*. And, I hope, there will need no other engine to demolish an ill-formed and proofless opinion about cold, than an axiom so solid and efficacious, that in the opinion of almost all the modern naturalists it has been able to abolish such potent and immense bodies as the *primum mobile* itself, and a superior orb or two, the least of which contained that firmament, in comparison whereof the whole earth is but a point. And not only so, but the same axiom has banished the angels and intelligences from the celestial orbs, that *Aristotle* and his followers had assigned them to turn about; or rather hath released those noble and happy spirits from the drudgery, to which the philosophers of so many ages had needlessly doomed them.

*Eleuth.* I THE less distrust the validity of the axiom you alledge, because I observe it to be the ground, on which is built a great part of the reformation of philosophy, that is introduced by the moderns. For one of the main things that first moved considering men to seek for more satisfactory opinions, than those of the peripatetic schools, was, that these obtruded a great many tenets in philosophy, that were not only unproved, but unnecessary to the explication of the phenomena of nature; as it were not difficult to shew.

BUT I see *Philoponus* preparing to renew the motion he lately made, in which the shortness of time makes me now think it seasonable to join with him, I being no less desirous than he to know, what may be the motives of your friend's declining to declare himself fully about the nature and cause of cold.

*Carn.* I HAVE already intimated to you, at the beginning of our conference, that he is himself the fittest person to be addressed to for satisfying this enquiry. But not to be altogether silent on this occasion, I shall tell you, that, as far as I can guess, he waits till farther trials and speculations have resolved him in some points, wherein he is not yet satisfied: for, being of a temper backward enough to acquiesce without sufficient evidence, when the enquiry is difficult, and the subject important; he seems to me to  
be



be kept in suspense, both by some speculative doubts, and the phaenomena of divers experiments, some of which are not delivered in his book. It would be now improper to mention the scruples and hesitations they have occasioned in him; though of those, I have heard him speak of, I shall name some instances that occur the most readily. As I remember I heard him make enquiry, as to those that would have cold produced by corpuscles of cold; whether, and on what account, those little fragments of matter are cold? whether those frigorifick particles, that must in multitudes crowd into water to turn it into ice, have gravity or levity, or are indifferent to both? And how any of the three answers, that may be made to this enquiry, will agree to some phaenomena, that may be produced? what structure the corpuscles of cold can be of, that should make them frigorifick to that innumerable variety of bodies they are said to pervade? And, whether the frigorifick faculty of these corpuscles be loosable, or not? As also, whether or no they be primitive bodies; and if it be said, they are not, whether there was not cold in the world before they were produced, and whence that cold could proceed? And if it were said, they are primitive bodies, he demanded, how it came to pass, that by putting a certain factitious body actually warm, into water that was also warm (both which appeared by a good sealed weather-glass) there should presently be produced an actual coldness (discernible by the same thermoscope)? These, and I know not what other scruples and difficulties, suggested to him by his thoughts, or his experiments, were the things, that, I suppose, prevailed with a man of his temper to forbear for a while the declaring of his sentiments about cold, lest the event of some farther trial should shew him cause to retract them.

*Phil.* What you have freshly intimated, *Carneades*, of Mr. *Boyle's* having other hesitations, than those you have named and suggested by experiments, not published in his history, does, I confess, the more excite my curiosity to have, at least, a taste of those perplexing phaenomena.

*Carn.* You may easily guess, *Philoponus*, by what I have told you already, that you are not to expect a full satisfaction from me on this occasion. But yet, that your curiosity may not be frustrated, I shall venture to acquaint you with two phaenomena, which were, I suppose, none of the least motives of his backwardness to declare himself. But though some body perhaps thinks, that the grounds of solving these phaenomena, and most of the newly recited scruples, may be picked out of some things, that may already have passed among us in this conference; yet, because we have not now time to enter upon a discussion of this matter, I am willing you should suspend the debate, till we have occasion to meet another time; and therefore I shall now only acquaint you with a couple of experiments, that he set down for a virtuoso, who was to solve the two main problems suggested by them. The first whereof was, whence water should, upon congelation, acquire so vast a force, as he found it had, to lift up great weights, and burst containing bodies; though it seemed by several circumstances, that the motion of the water is very much diminished, when it is changed into ice. And the second problem is thus conceived; if, as a brisk agitation of a body's insensible parts produces heat, so the privation of that motion is, as *Cardan*, and the Cartesians would have it, the cause of cold; whence is it, that, if certain bodies be put together, there will be a manifest and furious agitation of the small parts, and yet, upon this conflict, the mixture will not grow hot, but sensibly and even considerably cold? The narratives themselves, of the experiments, are too long to be now read over to you. And therefore, I shall leave the paper, that contains them, among you, to be perused at your leisure, between this and our next meeting, till when I must bid you farewell; only desiring

you in the mean while to remember, that, as I have but acted a part imposed upon me in our past conference, so, notwithstanding any thing that I have said in my assumed capacity, I reserve to myself the right of appearing as little pre-engaged, as any of you at our next meeting.

# T W O P R O B L E M S A B O U T

## C O L D,

Grounded on NEW EXPERIMENTS,

And proposed in a LETTER to a FRIEND.

To my very Learned Friend Mr. J. B.

S I R,

**I** PRESUME, that you will not be surprized to be told, that I send you the inclosed papers, not only that I might gratify your curiosity, but that you may by them be enabled to help me to satisfy my own; and therefore I shall accompany the historical transcripts I made of the following experiments, as I found them registered for my own remembrance, with some of the doubts suggested to me by some of the phenomena that occurred. But yet I shall not trouble you with all the difficulties that at first troubled me, but reduce the exercise, I desire to give your sagacity to the solution of two problems. And I will begin with propounding that first, which is grounded upon the last of the two following papers, because, though the historical part of that be much the longest, yet the grounds of my quere concerning it will be much more briefly proposed, the experiment itself naturally suggesting this problem; “How, upon the mixture of two or three bodies, such as those mentioned in the paper, there should manifestly ensue a great and tumultuary agitation of small parts, and yet even during this conflict, not any sensible heat, but a considerable degree of cold, be produced,” and that even in the internal parts of the mixture?

Prob. II.

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THE inducements to make this problem need not be far fetched, it being obvious enough, that, according to the corpuscularian philosophy, which you and I agree in, a brisk, and various agitation of the minute parts of a body is that, which makes it hot, both in reference to our sensories, and to its operations on other bodies. But I doubt the rise of the problem is much more easy to be understood, than the cause of the phenomenon, about which I will not ask you, whether one may not assert, that local motion is, in its own nature, a generical thing, which may be so diversified by circumstances, that one kind of modification of it, as it is made in corpuscles of several sizes, and shapes, may be the cause of heat, and another, that of cold? or else, whether we may suppose, that cold is a positive thing, and operates by real corpuscles of cold, which happening to abound, and yet to be locked up in the bodies, whose mixtures I employed, they are, by the great conflict that dissolves the texture of the clashing salts, separately put into motion, and that in such numbers, that though really there would be a heat produced by the brisk and confused agitation of some of the parts, yet that heat is not only concealed, and checked, but mastered by the over-powering operation of the frigorifick corpuscles. But to ask you about this, or any other particular way of solving our phenomenon, were to forget, that my aim is to learn not your opinion of this, or that particular conjecture, or fancy, about our problem, but in general, how it may be best resolved, and what you think to be the true cause of so odd an effect.

HAVING thus dispatched the little I had to say about the paper, that suggested the second problem, I will now suppose, that you have read the phenomena, that contain the rise of the first, to which I shall proceed, without farther preamble, since the question, or problem, that these naturally call for, is, "Whence this vast force of freezing water proceeds?" Prob. I.

For the breaking of resisting bodies being to be made by a violent local motion, and cold, according to the judgment even of the moderns, either consisting in, or at least, being accompanied with a privation, or a great imminution of motion, it seems very difficult to conceive, how cold should make water to exert so wonderful a force. I know the learned *Gassendus*, and divers other philosophers teach us, that glaciation is performed by the entering of swarms of corpuscles of cold, as they call them, into the liquor. But I much doubt, whether, from this hypothesis, a good solution of our phenomenon will be derived, since these atoms of cold seem not barely, as such, to make that expansion of the water, which is required in the experiment by me recited. For I see, that though water will be more and more refrigerated, according as the air grows colder and colder, yet, till it be brought to an actual glaciation, all the swarms of the frigorifick atoms in it are so far from expanding it, that they more and more condense it. And even that degree of cold, which destroys fluidity, though it expands water, does not do it merely by the multitudes of the frigorifick corpuscles, that invade the pores of the lately fluid body, since pure spirit of wine, and almost all chymical oils, though exposed to the same degree of cold, that turns water into ice, or, as I have tried, unto a far greater than is necessary to do so, will be but the more condensed by those swarms of particles. But, which is more considerable, I have carefully observed, that, besides common or expressed oils, chymical oil of aniseeds itself, being frozen, or conereted by an intense degree of cold, will not be expanded, but notably condensed, and accordingly grow specifically heavier than before. And this was one thing that kept me from expecting the removal of our difficulty from the ingenious explication given of freezing by the Cartesians, when they teach, that the eel like particles, whereof they suppose water to consist, are very remissly agitated, and their want of plianthness makes their contexture less close; which seems not to agree with the lately mentioned

trials. And though these eel-like particles should lose all their flexibleness, though in that case, it may probably be said, that they would take up less room than before, if nothing oppose their expansion. yet it does not thence appear, how they should acquire so vast a power to expand themselves in spite of opposition, as we have shewn water, by freezing, does acquire.

I did not hope to resolve our problem by the help of a vulgar supposition, that well stopped vessels are broken in frosty weather *ob fugam vacui*, since I found that supposition to be erroneous by divers experiments, some of which are mentioned in the history of cold.

It seemed less improbable, that some assistance to the solving of our difficulty might be given by two other things. Whereof the first is, that, for aught I have yet observed, no liquor but water, or that which participates of water, by having aqueous particles separable from it, will be made to swell by cold; nor will water itself do so upon every degree of cold, but only upon so great an one as actually turns it into ice. And the second is, that upon the glaciation of water, and aqueous liquors, we may observe in the ice many bubbles, greater or smaller, intercepted between the solid parts, and supposed to be full of air (I say, supposed, because, upon trial, I found them to have yielded but a small proportion of common air); which supposition, if true, would perhaps invite one to suspect, that the air contained in these bubbles might have an interest in our phenomenon; since I have found, by trial purposely made, that air congregated into visible, though not great portions, may exercise a considerable elasticity, which appeared not whilst it was invisibly dispersed through the water.

And if I did not suppose, both that you had taken notice, that there are wont to be numerous particles of springy air dispersed through the pores of water; and that you had considered, whether the want of plianthood, occasioned by cold in the aqueous corpuscles, whilst they are yet agitated and brandished by some permeating matter; and whether, upon the change of the pores, that we may conceive to be made in freezing water, either by the recess of one sort of subtil corpuscles, or the admission of another, or the closer confipation of the grosser parts, there may not be produced in corpuscles, that compose water) to say nothing of the intermixed air, or the concretions, or the coalitions, occasioned by the cold) a springiness capable to make many little bodies, endowed with it, exert a great force against the sides of the vessel, that oppose their joint endeavour to expand themselves: if, I say, I did not believe, that these, and the like suspicions, had occurred to you, as well as to me, together with the difficulties, wherewith each of them seems to be incumbered, I would acquaint you with what thoughts and trials occurred to me about these, and the like conceits. But I not daring to think this could prove other than a needless work, I must remember, that my business, in this paper, is to propose difficulties, not the ways of solving them; it being, from your kindness and sagacity, that these are as well expected, as desired, by,

S I R,

Your, &c.

A N



A N

## A T T E M P T

TO MANIFEST and MEASURE the

## G R E A T E X P A N S I V E F O R C E .

O R

## F R E E Z I N G W A T E R .

CONSIDERING, when I writ the history of cold, that, though divers phenomena might induce an attentive observer to think, that freezing water had an expansive force, yet I had not met with any that endeavoured, or even proposed to measure it, whether, because they reflected not on it at all, or judged not the force considerable; I, who looked with other eyes upon it, thought fit to repair that omission, but was then so ill furnished with requisites for doing it fully, that, I remember, I complained of it in my history of cold. And though, even afterwards, when the time of the year was favourable, I could not procure such accommodations as my design exacted; yet, thinking an imperfect way of measuring to be better than none, I preferred, to the making no attempt at all, the endeavouring to do what the least defective instruments I could procure would permit me, towards the making an estimate by known measures, of the expansive power of freezing water. For though I did not expect I should be able accurately to define it, yet I hoped I should make such an estimate, as to know, that force not to be, as one would think it, faint and contemptible, but very great and considerable.

I REMEMBER on this occasion, that to manifest the force of freezing water, I caused the barrel of a short gun to have a screw fitted to the nose of it, by which we might exactly stop it, as we did the touch-hole another way; then filling the barrel with common water, and closing it accurately by the help of the screw, we laid it in a conveniently-shaped vessel, wherein we encompassed it with a frigorifick mixture (of snow, or ice, and salt) and in a short time we found, as we expected, the barrel to be burst, part of the ice appearing along the gaping slit that had been made in the body of the iron by the freezing water, which, by this effect, seemed to emulate the justly-admired force of kindled gun-powder. But the design of this short paper tending not so much to prove, as (in some sort) to measure the expansive force of water, I shall subjoin the transcripts of two or three experiments, made chiefly for that purpose.

E X P E .

## EXPERIMENT I.

[THERE was taken a strong cylinder of brass, whose cavity was two inches in diameter; into this was put a bladder of a convenient size, with a quantity of water in it, that the neck of the bladder (which I had taken care to have oiled) being strongly tied, the water might not get out into the cavity of the cylinder, nor be capable of expanding itself some other way than upwards. Then into this cylinder was fitted a plug of wood, turned on purpose, which was somewhat less in diameter than the cylindrical cavity, that it might rise and fall easily in it. Upon the upper part of this plug was laid a conveniently shaped flat body, upon which were placed divers weights to depress the plug, and hinder its being lifted up by the expansion wont to be made in water that is made to freeze; then a frigorifick mixture being afterwards applied to the cylinder, it appeared, within half an hour, or somewhat more, by a circle, that had been purposely traced on the side of the plug, where it was almost contiguous to the orifice of the cylinder, that the water in the bladder began to expand itself, and about two hours after, having occasion to shew the experiment to some inquisitive persons, the circle appeared to have been heaved up, in my estimate, about  $\frac{2}{3}$ , if not half of an inch, notwithstanding all the weights that endeavoured to hinder the ascension, though these weights amounted to 115 pounds, which were all the determinate weights we could then procure, besides a brick, and some other things, that were estimated at five pounds more; nor did I doubt that a far greater load would not have hindered its expansion.]

## EXPERIMENT II.

[WE took a brass cylinder, whose dimensions were three inches eight tenths in diameter, and in depth four inches. Into this we put a fine bladder of a convenient size, almost filled with water, and strongly tied about the neck; upon this bladder we put the wooden plug to stop up the orifice, as much as was convenient, and upon the plug we put a piece of a flat board for the weights to stand upon. These things being prepared, we conveyed the cylinder with all that belonged to it, save the board, into a large wooden bowl, where we applied to the cylinder a good quantity of the frigorifick mixture, made with beaten ice and bay salt; and having first marked with a circular line the edge or contact, where the orifice or lip of the cylinder touched the plug, we laid on the weights upon the board; and when by their weight they had depressed the plug till the cover of it leaned upon the cylinder, we disposed ourselves to attend the issue of the trial. The event whereof was this, that when the action of the frigorifick mixture had produced some ice in the water included in the bladder, that liquor appeared to have dilated itself strongly enough to begin to raise the plug with the super-incumbent weights, and by degrees they were, by the growing ice, raised, till the mark, diligently made on the plug, where the edge of the cylinder touched it, was about a tenth part of an inch above the station it had before the plug had been depressed. Then we took out the bladder, and found the cylinder of water within the bladder not to be wholly turned into ice, but to contain some quantity of unfrozen water in the parts about the centre, which liquor, if we had not so soon desisted from the experiment (as for certain reasons we did) might probably have raised the weights somewhat higher. But as it was, the ice in length was but three inches and about  $\frac{1}{3}$ , and yet so small a quantity of ice sufficed to raise,



faise, besides the board they leaned on, as many weights of lead as amounted to an hundred pounds avoirdupois.]

EXPERIMENT III.

[THE day after the above-mentioned experiment was made, to try yet farther the expansive force of freezing water, the same was reiterated after the manner above delivered, but with this difference, that having procured more weight, when the plug was lifted up  $\frac{3}{4}$ , or somewhat better (which plug began sensibly to rise within half, or three quarters of an hour, after the frigorifick mixture was applied) it was loaded with a weight of two hundred pounds, and a fifteen pound-piece of lead, and other bodies, as boards, &c. to lay the weights upon, which being also weighed by themselves, came to fifteen pounds more, so that the whole amounted to two hundred and thirty pounds; and if the hundred pounds were both of them, as their bulk and shape invited us to guess, of that sort of weights which are called the great hundred, containing an hundred and twelve pounds a-piece, twenty-four pounds must be added to the sum, which would thereby be made up two hundred and fifty-four pounds.]

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A

NEW EXPERIMENT

ABOUT THE

PRODUCTION OF COLD

By the CONFLICT of BODIES, appearing to make an  
EBULLITION.

AND now, that we are searching after the nature of cold, I am put in mind, that I have sometimes wondered at a certain experiment that is so anomalous, and seems so little of kin to the usual phenomena of cold, that though I do not particularly teach the way of making it, because I could not do it without discovering something in chymistry, that cogent considerations forbid me at present to publish; yet I cannot forbear to relate, on this occasion, the matter of fact, both because it may afford considerable hints to sagacious inquirers, and because it seems so little congruous to most theories of the causes of cold, that it may make the framers of theories more wary, and help also

to excuse my backwardness to propose hypotheses about cold in a resolute and confident way.

THE experiment is this: we took three saline bodies, each of them purified by the fire; and whereas there are divers bodies, that being mingled together acquired a heat, which neither of them had apart; and whereas it is said by some, that there are a few, which being blended together, make a mixture somewhat colder than either of themselves, these salts of ours being put together in due proportion, do, upon their mixture, produce that, which the eye judges to be a great effervescence; but though the hissing noise be loud, and though the numerous bubbles suddenly generated will make the matter apt to overflow the glass, if the one be not capacious, and the other be not put in by little and little; yet even whilst this seeming ebullition lasts, the glass, which one would expect to find very hot (as usually happens upon the mixture of the salt of tartar, and spirit of nitre, and upon the confusion of the like saline bodies disposed to produce together such efflorescencies) instead of growing hot, does, if it be held in one's hand, feel much cooler than before, and that in a wonderful degree; insomuch, that even in winter the outside of the glass would quickly be covered with great drops of dew, which after a while would unite, and trickle down by their own weight. And this we could make to last for a great while, by casting in by degrees more and more of one of the ingredients on the other. And besides that, this copious dew on the outside of the glass, reached as high as the mixture within, which argued whence it proceeded; besides that, purposely looking on the bottom of the glass, whose outside was concave, we found no such drops of dew there, because the vapours of the external air could not, in any quantity, have access to it; which shewed the dew, conspicuous elsewhere, not to come from the transudation of the finer parts of the mixture through the pores of the glass; besides these things, I say, I remember, that having sometimes purposely wiped off the dew here and there with my handkerchief, the dry parts of the glass would in no long time regain fresh drops of dew. And this odd experiment we did for the main repeat, not only in the presence of an industrious chymist (whose trials unexpectedly gave us the rise of the experiment) but also alone, and at differing seasons of the year.

I SHALL add, that having afterwards, about the middle of *November*, thought fit to vary a little, and repeat the experiment, because I could then make use of a sealed weather-glass, which I had not at hand when I made the former trials; I took two deep glasses, into the one of which I put a good quantity of fair water, and in the other I made such a mixture, as I was lately mentioning; and having by a string (to prevent the altering of the temper of the included air by the warmth of my fingers) let down the weather-glass into the water, that the liquor shut up in the instrument might be cooled by the ambient water; after it had staid there a reasonable time, I took it out, by the string that was fastened to the upper part of it, and letting it down into the mixture, that was then hissing, and filling the vessel that contained it with multitudes of successively emerging and hastily vanishing bubbles; I perceived nevertheless, that the coldness of the seemingly effervescent mixture made the imprisoned tinctured liquor to subside so low, that from four inches and three quarters (or thereabout) at which height it stood in the carefully divided stem, when the weather glass was taken out of the water, it fell in a short time lower than to one inch and an half. And because I foresaw that this might seem scarce credible, especially if I should relate how swiftly the imprisoned liquor subsided at the beginning; I shall annex, that, for farther satisfaction of others, I removed the thermometer out of the mixture into the common water again, where it soon reached to somewhat above four inches and a half; and not content with that, I put it  
a second



a second time into some of the frigateactive mixture before it had done foaming, in which it fell, as before, somewhat below an inch and a half, and, presently after, almost as low as to an inch. And having once more put it back into the glass that contained the water, the included liquor re-ascended to above four inches and a half, and this in an excellent sealed weather-glass, whose stem was not in all above ten inches long, with a ball proportionably big. And for farther confirmation, I took notice, that, whilst the mixture, by its hissing noise, and its strangely numerous bubbles, seemed to be in a state of ebullition, the outsides of the glass that contained it, were, as far as the mixture reached, so plentifully bedewed with the condensed vapours of the ambient air, that their weight carried them down in little streams, which left round about the bottom of the vessel a pretty quantity of liquor, that appeared by its taste not to have been made by the transudation of any of the sharp and saline liquors that were agitated within the glass. There remained only one scruple, which was suggested to me by the remembering of a circumstance, which, however, at the making of the fore-mentioned trials, I had not minded, and which possibly most observers would have neglected; but calling to mind that the water I had made use of to immerse the weather-glass in, was brought out of a room, wherein a fire was wont (though not constantly) to be kept, whereas the ingredients of the mixture were kept, and put together in a chamber, which, though contiguous to the former, had no chimney in it; I thought fit, for greater circumspection sake, to let the water stand all night in this last-mentioned chamber, that the ambient air might have the same operation upon it, as upon those bodies that were to be ingredients of the mixture; and then repeating the formerly-recited experiment, though I thought it needless to spend time to watch, as before I had done, the greatest difference in cold betwixt the water and the bubbling mixture; yet by making removes of the weather-glass to and fro, from one liquor to another, it sufficiently appeared, that the greater coldness, remarkable in the mixture, did not before proceed in any considerable degree (if in any degree at all) from the water's not having been kept in the same room with it.

So that by these different trials it seems manifest, that the coldness of the mixture was not a deception of the sensory, since it would be discovered by the operation it had, not only upon the vapours of the air on the outside of the glass, but upon the thermometer itself, placed in the midst of the mixture, which this last-named circumstance argues to have been cold throughout, and even in its innermost parts.

AND to shew how much this strange coldness depended upon the peculiar texture of the mixture, or the structure of its component corpuscles, and the peculiar kind of motion that was excited in the tumultuating particles; I shall here subjoin a relation, which probably will not appear despicable; namely, that, in the first place, I took some of the acid liquor, the rest of which I had made use of to make the mixture, whereof I have been speaking; and put a convenient quantity of fair water, which had been kept a night or two in the same room (wherein was no chimney) with it, that there might be no cause of suspicion, that the one had been exposed to a more or less cold air than the other; and yet these two liquors did scarce sensibly differ in coldness; though to discover whether they did or no, I removed from one to another of them a good sealed weather-glass, with a very slender stem.

AND, in the next place, I took a convenient quantity of the pure salt I had so often employed, and cast it into a glass full of water, which I had kept many hours in the same room with it, and wherein I had a little before placed a sealed weather glass, that the included liquor might be brought to the temper of the ambient liquor; but upon this injection, the tinted liquor of the thermoscope subsided so little, as not to make me look upon this salt as being itself extraordinarily cold, since other obvious salts (that I have

at other times cast into water to cool it a little) and even sea-salt would (according to my estimate) have refrigerated it as much, if not more. Nor did I observe the glass where-in I was wont to keep store of our salt (though I had oft'n occasion to handle it) disclose to the touch any remarkable degree of coldness; so that the coldness of our hissing mixture could not be attributed to that of either of the ingredients apart, but was a quality emerging upon their being blended. Now, when I thus made these preparatory trials, having afterwards placed in the same window (of the chamber last-mentioned) a couple of glasses, with common water in one, and in the other some of that mixture, of whose frigefactive power I had very recently made trial; I left them to stand there together all night, and left also standing by them such a sealed weather-glass as I have been mentioning; and the next morning, when all the visible commotion or agitation of the minute parts of the contrary salts of the mixture was quieted, I put the weather-glass, first, into one of those two liquors, and then into the other, and after removed it back into the former again, without perceiving any difference worth minding, betwixt the coldness of the mixture, and that of common water; and with much the like success I repeated the trial, after the water and the other liquor had stood in the same room (unfurnished with a chimney) for near two days and nights.

AND for farther confirmation, I shall add, that having instead of the salt, which I hitherto made use of, taken some of the spirit that was wont to come over together with that salt, and did so abound with it, that a good deal of it lay undissolved at the bottom of the liquor; having, I say, employed this saline spirit, instead of the salt itself, and having for trial's sake mixed with it another spirit, drawn in my own laboratory for the purpose, which to me seemed as like, as could be made, to that, which I had all this while made use of; I found, that the mixture of these two liquors, though it produced far fewer bubbles than I was wont to have, instead of growing cold, grew lukewarm, and quickly impelled the liquor in the weather-glass, from a little above three inches, to as much above eight; and yet, besides that this last spirit was, as far as I could perceive, and that after the same manner, drawn from the same materials with that I had used all this while; the smell and taste (which are both of them peculiar and odd enough) concurred to manifest the two spirits to be of the same kind.

AND, for farther proof, I shall add, that to satisfy myself the more fully, I took a parcel of the same liquor, I had lately employed with success in making the frigorick mixture; and yet even this liquor, which with the dry salt would questionless have produced a frigefactive mixture, as well as the rest had done, which I had a little before taken out of the same phial; this liquor, I say, put to a new portion of the saline spirit above-mentioned, though they did not produce minute bubbles numerous enough to make a foam; yet the mixture, instead of growing very cold, grew manifestly lukewarm, not only in the judgment of the touch, but by its operation on a good sealed weather-glass, carefully, and for a competent while employed to examine the temper of it. Whereas, on the contrary, having purposely kept some of the frigorick spirit by the fire-side, till its temper was so altered, that it nimbly enough rarefied and impelled up the spirit of wine contained in a sealed weather-glass, immersed in it, and having into this liquor cast some of the frigorick salt, even whilst the spirit of wine was rising, and would probably have risen a pretty while longer; this injected salt, when it began to be dissolved, did not only give a check to the rising liquor, and quickly put a stop to its ascent; but, as I expected, soon made it subside again, till it fell about three inches or more (which was very much in a short weather-glass) beneath the station where the spirit of wine had rested, before the liquor was set by the fire-side; nay, afterwards, I tried, that a frigorick salt, being well warmed by the fire side, did, with an appropriated liquor,



1. liquor, that was also warmed, produce a coldness manifestly perceivable by the weather-glass. So that in these cases a body but moderately cold, nay, actually warm, hastily reduced one, actually warm, or at least tepid, to a far greater degree of actual coldness than itself had.

THESE are some of the experiments I tried with the liquors and salts, of which, upon allowable considerations, I must now forbear to set down the way of preparing; but, that even at present I may not be altogether wanting to the curious, I devised a way of making a succedaneum to this experiment, which I shall here willingly annex, as that, which though it be much inferior to what I may one day be at liberty to acquaint the reader with; yet it will shew the main thing intended, by manifesting, that cold may, by the mingling of bodies, be produced or increased to a degree exceeding that of either of the bodies that composed the mixture; and this, though at the same time a seeming effervescence be made by the bodies that thus refrigerate each other.

I took then very good salt of tartar, and putting to it a convenient quantity of spirit of vinegar, I did, whilst the mixture was hissing (but seemed to the touch to have refrigerated the glass that contained it (immerse into it the ball of a good sealed thermoscope furnished with spirit of wine. And, though the weather-glass were not much above a foot long, yet the coldness of this mixture made the tinted liquor descend hastily enough two inches and almost a half. And to shew farther, that this mixture was actually colder than cold water, removing the weather-glass out of the mixture into that liquor, the tinted spirit began to re-ascend, and that so nimbly, that in about three minutes (that the ball of the thermoscope staid under water) the spirit of wine had re-ascended about an inch and a half, if not more. And to try, whether this coldness of the mixture did proceed from or depend upon some texture of the parts that was not very permanent, and yet did not quite degenerate, immediately after the ingredients had ceased to work upon one another; I remember, that near an hour after the ebullition of the spirit and salt of tartar was over, the thermoscope being removed out of the common water, where it had stood immersed, into the mixture, descended about half an inch or more. For want of salt of tartar I could not begin the experiment anew, and so am not sure it will always succeed uniformly\*. But yet to give myself what further satisfaction I could, by trying the same experiment in such a way as might discover whether or no the phenomenon did not depend upon, or require some peculiar texture in the fixed salt that had been employed; I took some alcali (made by dissolving pot-ashes in fair water, and reducing them by coagulation to a white salt) and pouring spirit of vinegar to it, I found, that this mixture did not, whilst it hissed, grow at all colder, but rather somewhat warmer. And, for farther satisfaction, immersing into it the ball of the newly-mentioned weather-glass, I found that it ascended in a short time about an inch, and, being removed into the water, descended about half an inch; and by making removes of it from one of these liquors into the other, two or three times more, I found, that the spirit of wine did rise and fall according to what has been newly observed, but its motions upwards and downwards were both less than before, and more slow.

\* The Author's wariness was not here amiss, he having afterwards found that this experiment did not always succeed.

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O B S E R V A T I O N S  
AND  
E X P E R I M E N T S  
ABOUT THE  
S A L T N E S S O F T H E S E A.

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The F I R S T S E C T I O N.

C H A P. I.

**T**HE cause of the saltneſs of the ſea appears, by *Aristotle's* writings, to have buſied the curioſity of naturaliſts before his time; ſince which, his authority, perhaps, much more than his reaſons, did, for divers ages, make the ſchools, and the generality of naturaliſts, of his opinion, till towards the end of the laſt century, and the beginning of ours, ſome learned men took the boldneſs to queſtion the common opinion; ſince when the controverſy has been kept on foot, and, for aught I know, will be ſo, as long as it is argued on both ſides but by dialectical arguments, which may be probable on both ſides, but are not convincing on either. Wherefore, I ſhall here briefly deliver ſome particulars about the ſaltneſs of the ſea, obtained by my own trials, where I was able; and where I was not, by the beſt relations I could procure, eſpecially from navigators.

FIRST then, whereas the Peripateticks do, after their maſter *Aristotle*, derive the ſaltneſs of the ſea from the aduſion of the water by the ſun-beams, it has not been found, that I know of, that where no ſalt, or ſaline body, has been diſſolved in, or extracted by water expoſed to the ſun or other heat, there has been any ſuch ſaltneſs produced in it, as to juſtify the *Aristotelian* opinion. This may be gathered, as to the operation of the ſun, from the many lakes and ponds of freſh water to be met with, even in hot countries, where they lie expoſed to the action of the ſun. And as for other heats, having out of curioſity diſtilled off common water in large glaſs bodies and heads, till all  
the



the liquor was abstracted, without finding, at the bottom, the two or three thousandth part, by my guess, of salt, among a little white earthy substance that usually remained. And though I had found a less inconsiderable quantity of salt, which, I doubt not, may be met with in some waters, I should not have been apt to conclude it to have been generated out of the water, by the action of the fire, because I have, by several trials purposely made, and elsewhere mentioned, found, that in many places (and I doubt not, but if I had farther tried, I should have found the same in more) common water, before ever it be exposed to the heat of the sun or other fire, has in it an easily discoverable saltiness of the nature of common salt, or sea-salt, which two I am not here solicitous to distinguish, because of the affinity of their natures, and, that in most places, the salt eaten at table, is but sea-salt freed from its earthy and other heterogeneities, the absence of which makes it more white than sea-salt is wont to be with us. These last words I add, because credible navigators have informed me, that in some countries, sea-salt, without any preparation, coagulates very white; of which salt I have had (from divers parts) and used some parcels.

BUT some of the champions of *Aristotle's* opinion are so bold, as to alledge experience for it, vouching the testimony of *Scaliger* to prove that the sea tastes saltier at the top than at the bottom, where the water is affirmed to be fresh. But as for the authority of *Scaliger*, though I take him to be an acute writer, yet I confess, that, for reasons elsewhere given, I do not allow it that veneration, which I find given it by very learned men; nor am I over prone, even as to matters of fact, to acquiesce in what he tells us, when he neither signifies that he delivers things upon his own experience, or declares from what credible information from others he received them.

It is true, that having often observed that sea-salt dissolved in water is, upon the recess of the superfluous liquor, wont to begin its concretion, not as most other salts do, at either the lateral or lower parts of the vessel, but at the top of the water, I will not think it impossible, that sometimes in very hot climates, or weather, the sea may taste more salt at the top, than at some distance beneath it. But considering how great a proportion of the salt common water is wont to be impregnated with, before it suffers saline concretions to begin, and how far short of that proportion the salt contained in the sea-water is wont to be, inasmuch, that about *Holland*, a Dutch geographer or two have not found it to amount to the proportion of one to forty; and I in *England* found it to be no more than I shall hereafter specify; it seems not unlikely, that *Scaliger's* \* observation was well made, and it must be very unlikely that it should generally hold, if the saltiness of the superficial parts of the sea be compared with that of the lower parts of it.

AND yet I do not build my opinion wholly upon this argument of some modern philosophers, that salt being a heavier body than water, must necessarily communicate most saltiness to the lowest parts.

For though this argument be a probable one, yet water being a fluid body, the restless agitation of whose corpuscles makes them, and the corpuscles they carry with them, perpetually shift places, whereby the same parts come to be sometimes at the top, and sometimes at the bottom; this consideration, together with what was lately noted of the peculiar disposition of dissolved sea-salt, to begin its coagulation upon the surface of the water, may make the argument, we are considering, suspected not to be so cogent, as at first sight one may think it. Which suspicion I might somewhat countenance by subjoining, that in divers metals, and other tinted solutions, I have not usually observed the upper part of the liquor to be manifestly deeper coloured than the lower; though,

\* See the third Section towards the latter end.

between metalline bodies and their menſtruums, the diſproportion of ſpecifick gravity does uſually much exceed that, which I have met with, between ſea ſalt and common water.

## C H A P. II.

It is urged out of *Linschotten* by a learned modern writer, that wanting freſh water near *Goa* (the Metropolis of the Portugals in the *East-Indies*) they make their ſlaves fetch it, by diving, from the bottom of the ſea; which ſeems a clear evincement of the Peripatetick opinion. But, in this obſervation, I cannot acquieſce, for two reaſons; the one, becauſe, that though what is alledged, as matter of fact, were ſtrictly true, yet ſo general a concluſion could not be ſafely drawn from that particular inſtance, ſince in other parts of the ſea, the contrary has been found by experience, as I ſhall ſhew ere long. And other reaſons than thoſe given by the Peripateticks may be rendered of what happens at *Goa*, which reaſons may extend to the like caſes, if elſewhere they ſhall happen to be met with. For it may very well be, that ſprings of freſh water may ariſe in ſome parts of the ſurface of the earth that are covered with the ſea, as they do in innumerable vallies and other places of the terreſtrial ſurface that is not ſo covered; not to mention thoſe ſprings that appear in divers places upon a low ebb, covered with the ſea during the flood. The curious Hungarian † governor that gives us an account of the wonderful waters that ennoble his country, relates, that in the river *Vagus*, that runs by the fortrefs *Galgotium*, the veins of hot water ſpring up in the bottom of the river itſelf. *Neque in ripa tantum*, ſays he, *eruntur calidae, ſed etiam intra amnem, ſi ſundum ejus pedibus ſuffodiās, calent autem immodicè, &c.* Nay, I have been aſſured by more than our learned eye-witneſs, that there is a place upon the Neapolitan coaſt, where they (and, I think, a writer or two of thoſe parts) obſerved the water to ſpring up hot beneath the ſurface of the ſea, inſomuch, that one of my relators thruſting in his hand and arm ſomewhat deeper than was convenient, found there an offenſive degree of heat.

BESIDES (which is my ſecond conjecture) as to the particular caſe of *Goa*, I had the curioſity to enquire of a great traveller, and a man of letters, that lived in that city and the neighbouring places, and gave me a pertinent account of them, eſpecially of that place, whence the freſh water is fetched by the divers, which his curioſity led him to viſit, and take ſpecial notice of; but I found by him, that the divers do not now think it needful to fetch their freſh water ſo low as from the bottom of the ſea, and that, by the little depth, whence his and other men's curioſity cauſed it to be taken up, he judged it did not ſo much come from any freſh water ſprings riſing at the bottom of the ſea, as from a ſmall river (whoſe name I do not remember) that not far from thence runs into the ſea, with ſuch a juncture of circumſtances, that at the mentioned places, the freſh water does yet keep itſelf tolerably diſtinct, and is not yet ſo far made brackiſh, as not to continue potable, though not very good. Which conjecture of his I could make probable, by what I have had from eminent and obſerving men among our own navigators, touching the ſliding of waters one over another, in ſome parts of the ſea, eſpecially near the mouths of rivers. But the diſcuſſion of this matter, and the particulars of the account given me of the ſituation of the place where water is dived for near *Goa*, would require more words than they would in this place deſerve, unleſs the point under debate were more important to our preſent purpoſe.

† De admirandis Hungariæ Aquis.



I MIGHT here pretend to a clear demonstration, by experience of the contrary of what *Scaliger* delivers, by vouching the testimony of the learned *Patricius*, who affirms, that being upon the sea, which takes its denomination from the island of *Crete* (now *Candia*) he did, in the company of a Venetian magistrate, *Mocinigo*, let down a vessel (furnished with a weight to sink it) to the bottom of the sea, where, by the help of a contrivance, it was unstopped, and filled with water there, which being drawn up, was found to be not fresh, but salt. This experiment, I say, I could oppose as a demonstration against *Scaliger*; but though it be a very probable argument, and more considerable than any I have seen brought by the Peripateticks for their opinion, yet, I confess, it would be more satisfactory to me, if it would not permit me to suspect, that in the drawing up of the vessel through the salt water, though there had been fresh water taken in at the bottom, the taste may have been altered by the subingression of salt water, which being, bulk for bulk, heavier than fresh, would by its ponderousness endeavour to sink into the ascending vessel, and thereby more easily expel part of the fresh water, and mingle with the rest. Wherefore, I shall confirm the saltiness of the sea at the bottom by some observations that are not liable to the same objections as that of *Patricius*.

THE first is that of the person, whom I elsewhere mention, to be able, by help of an engine, to stay a considerable time at the bottom of the sea; for of him I learned, among other things, that I desired to be informed of touching that place, that he found the water to have as salt a taste there as at the top.

THE next observation I obtained by means of a great traveller into the *East* and *West-Indies*, who having had the curiosity to visit the famous pearl-fishing at *Manar*, near the great *Cape of Comori*, answered me, that he had the same curiosity that I expressed to learn of the divers, whether they found the water salt at the bottom of the sea, whence they fetch their pearl-fishes? and that he was assured by them that it was so; and the same person being asked by me about the saltiness of the sea in a certain place under the Torrid Zone, which the relation of a traveller inclined me to think to abound extraordinarily with salt, affirmed to me, that not only the divers assured him, that the sea was there exceeding salt at the bottom, but brought up several hard lumps of salt from thence, whereof the fishermen and others were wont to make use of to season their meat, as he himself also did; which yet I may ascribe not only to the plenty of salt already dissolved in the water, but to the greater indisposition, that some sorts of salts, whereof this may be one, have to be dissolved in that liquor.

TO these I shall add this third observation: meeting with an inquisitive engineer that had frequented the sea, and had several opportunities to make observations of other kinds in deep waters, I desired him that he would take along with him a certain copper vessel of mine, furnished with two valves opening upwards, and let it down for me the next time he went to sea; on which occasion he told me, that, if I pleased, I might save myself the trouble of the intended trial, for, with a tin vessel, very little differing from that I described unto him, he had had the curiosity, near the straight of *Gibraltar's* mouth (where he had occasion to stay a good while) to fetch up sea-water from the depth of about forty fathoms, and found it to be as salt in taste as the water near the surface.

THESE observations may suffice to shew that the sea is salt at the bottom in those places where they were made; but yet I thought it was not fit for me to acquiesce in them, but rather endeavour to satisfy myself, by the best trial I could procure, to be made with my copper vessel (as more strong and fit than a tin one) what saltiness is to be found in the water at the bottom of our seas, not only, because it may more concern us to know, that, but chiefly, because, though I deny not, that in the foregoing observations the

the taste may sufficiently prove, that the sea is salt at the bottom as well as the top, yet I thought the taste, by reason of the predispositions and other unheeded affections it is liable unto, no certain way to judge, whether the top and the bottom be as salt one as the other. Wherefore, I thought it would be more satisfactory to examine the sea-water by weight, than by taste; and in order thereunto, having delivered the above-mentioned instrument to the engineer I lately spake of, when he was going to sea, he sent me, together with it, a couple of bottles of sea-water, taken up, the one at the top, and the other at the bottom, at fifteen fathoms deep. The colour and smell of these two waters were somewhat differing; but when I examined them hydrostatically, by weighing a roll of brimstone first in one, and then in the other, I scarce found any sensible difference at all in their specifick gravities. So that if the degree of the saltness of sea-water may be safely determined by its greater or lesser weight, then so far forth as this single experiment informed me, the saltness is equal at the top and bottom of the sea; I said, if the degree, &c. because of what I shall hereafter take notice of about salts of less specifick gravity than sea-salt.

### C H A P. III.

It follows now that I make out what I formerly intimated, that though it were granted, that near *Goa*, and perhaps in some other places, the divers may have found the water fresh at the bottom of the sea, it would not therefore necessarily follow, that the sea-water, generally speaking, is fresh at the bottom; for the observations lately mentioned sufficiently manifest the contrary; and as to those very few places (if really there have been any) where the sea-water has been found fresh at the very bottom, I think one may ascribe the taste of the water to the bubbling up of springs of fresh water at or near enough to those very places. I know this may appear a paradox, since it may seem altogether unlikely, that so small a stream of water, as can be afforded by a spring, should be able to force its way up in spite of the resistance of so vast a weight as that of the super-incumbent sea-water, especially since this liquor, by reason of its saltness, is heavier in specie than fresh water.

BUT this objection needs not oblige me to forsake my conjecture; for whatever most men believe, and even learned men have taught, to the contrary, it matters not how great the quantity of liquor be, which is laterally higher than the lower orifice of the pipe, or channel, that gives passage to the liquor that is to be impelled up into it; provided the upper surface of the liquor in the channel or pipe have a sufficient perpendicular height in reference to that of the stagnant water; for no more of all this fluid will hinder its ascent than the weight of such a pillar of the said fluid, as is directly super-incumbent on it. \* *Stevinus*, and I, have, by differing ways, particularly proved, that, according to the laws of the true hydrostatics, the prevalence of the two liquors that press against each other, is not to be determined according to the quantity of them, but to be adjudged to that which exceeds the other in perpendicular height; so that, considering the channel wherein a spring runs into the sea, as a long and inverted siphon, if that part of the either neighbouring, or more distant shore, whence the spring, or river, takes its course, be a neighbouring hill, or rock, or any other place considerably higher than that part of the bottom of the sea, or of the shore covered with the surface of the sea, at

\* Vid. *Stevinum*, Prop. 10. Lib. IV. Statics. And see the Author's Hydrostatical Paradoxes.

which



which the channel which conveys fresh water terminates, that liquor will issue out in spite of the resistance of the ocean.

To illustrate at once, and prove this paradox, I thought upon the following experiment. I took a vessel of a convenient depth, and a siphon of a proportionable length, both of them of glass, that their transparency might permit us to see all that passed within them. Into the larger vessel we put a quantity of sea-water, and into the longer leg of the siphon, which had been for that purpose inverted, we poured a convenient quantity of fresh water, which we kept from running out at the shorter leg, by stopping the orifice of the longer with the thumb or finger; then this siphon being so placed in the greater vessel, that the orifice of the shorter leg was a great deal beneath the surface of the salt water, and the superficies of the fresh water in the longer leg was a pretty deal higher than that of the surrounding salt water, we unstopped the orifice of the upper leg, whereby the water in the siphon, tending to reduce itself to an æquilibrium or equality of height, in both legs, the water in the upper leg being much higher and heavier than that in the other, did, by subsiding, drive away the water in the shorter leg, and make it spring out at the orifice of the shorter leg, in spite of the breadth and specifick gravity of the salt water. And this impelling upwards of the fresh water lasted as long as the surface of that water, in the longer leg, retained its due height above that of the surrounding sea-water; which circumstance I expressly mention, because there being a difference amounting to between a fortieth and fiftieth part, betwixt the specifick gravity of our sea-water and common fresh water, by reason of the salt, which makes the former the heavier, the fresh water in the longer leg of the siphon ought to be between a fortieth and fiftieth part higher than the surface of the sea-water, to maintain the æquilibrium betwixt these two liquors.

To make the forementioned experiment the more visible, I thought fit to perform it with frsh water tinged with brasil or logwood; but that it might not be objected, that thereby the specifick gravity of the liquor would be altered or increased, I afterwards chose to make it with claret-wine, which being a liquor lighter than common water, and of a conspicuous colour, is very convenient for our purpose.

And when I made this trial, by placing the orifice of the shorter leg at a convenient distance below the surface of the sea-water, it was not unpleasant to observe, how, upon the removal of the finger that stopped the orifice of the longer leg, the quick descent of the wine contained in that leg impelled the coloured liquor in the shorter leg, and made it spring up, at its orifice, into the incumbent sea-water, in the form of little red clouds, and sometimes of very slender streams. And as this shorter leg of the siphon was raised more and more towards the surface of the water, so there issued out more and more wine at the orifice of it; the liquor in the longer leg proportionably subsiding, but yet continuing manifestly higher than the surface of the salt water, than which it was in specie much lighter.

¶ But here I must give an advertisement to prevent a mistake; for if the siphon be not exceeding slender, after the wine in the longer leg is fallen down to its due station, a heedful observer may perceive, after a while, that though the siphon be kept in the same place, there will issue out of the shorter leg a little red stream, which proceeds not from the former impulse of the wine in the longer leg, but from the ingress of the sea-water, which being much heavier in specie than wine, sinks into the cavity of the siphon, and as it comes in on one side, thrusts up as much wine on the other side of the same cavity. But the red liquor that ascends on this account may be discerned to do so, by its rising more slowly, and after another manner than that which is impelled up by the sudden fall of the tall cylinder of wine in the longer leg.

## THE SECOND SECTION.

## C H A P. I.

AS to the cause of the saltness of the sea, I therein agree with the learned *Gassendus* and some other modern writers, that the sea derives its saltness from the salt that is dissolved in it; but I take that saltness to be supplied, not only from rocks and other masses of salt, which at the beginning were, or in some places may yet be found, either at the bottom of the sea, or at the sides, where the water can reach them, but also (to say nothing here of what may, perhaps, be contributed by subterranean steams) from the salt, which the rains, rivers, and other waters dissolve in their passage through divers parts of the earth, and at length carry along with them into the sea. For not only it is manifest enough that several countries afford divers salt springs, and other running waters, that at length terminate their course in the sea; but I have sometimes suspected, that very frequently the earth itself is impregnated with corpuscles, or, at least, rudiments of common salt, though no such thing be vulgarly taken notice of. Which suspicion may be confirmed (to omit what I have elsewhere delivered on another occasion) partly by the observation of some eminent chymists, who affirm themselves to have found a not inconsiderable quantity of exceeding saline liquor upon the evaporation of large quantities of some waters (for in some others I could not find it) and principally by the quantity of common salt that is usually found in the refining of saltpetre; though that be a salt, which *Sir Francis Bacon*, and other experienced writers teach, that almost every fat earth, kept from the sun and rain, and from spending itself in vegetation, will afford.

BUT having, on another occasion, sufficiently shewed †, that the earth does abound with common salt, in many more places than are wont to be taken notice of; and that it is probable, that by maturation, or otherwise, salt may daily grow in the earth, it will not be necessary to add, in this place, any thing to what I have said already, to prove that our common terrestrial salt, being dissolved, may suffice to make the sea-water brackish; and the rather, if we call to mind what has been formerly said about the possibility of springs rising beneath the surface of the sea, and of lumps of salt that were taken up by divers, undissolved, at the bottom of the sea; the ocean may receive supplies of salt from rocks and springs latent in its own bosom, and unseen even by philosophers. And this may be one reason, I conceive (for I deny not but that there may be others, as the very unequal heat of the sun, &c.) why some seas are so much saltier than others, or, at least, why in some places the sea-water may be much saltier than in others.

AND as we have seen that our common terrestrial salt may be copiously enough communicated to the sea, to impregnate it with as much saltness as we observe it to have; so I do not see that the difference between that salt and sea-salt is so great, but that it may well be supposed to be derived from those changes that the terrestrial salt may be liable to, when it comes into the sea. For that the marine salt, and the terrestrial do very well agree in the main things, may be argued from the resemblance both in shape, taste, &c. that may be observed between the grains that will be produced, if we expose each of them in a distinct glass to such a heat, as may slowly carry off the superfluous moisture, and suffer them to coagulate into cubical, or almost cubical grains; and the lesser differences that may be met with between these two salts, may well enough be

† In a Tract of Subterranean Menstruums.



supposed producible by the plenty of nitrous, urinous, and other saline, to which, in some places, may be added bituminous bodies, that by land-floods, and otherwise, are from time to time carried into the sea, and by several things that happen to it there, especially by the various agitation it is put into by tides, winds, currents, &c. and (which I would by no means omit) by its being in vast quantities exposed to the sun and air.

C H A P. II.

WE may justly be the more careful to determine whether the saltness of the sea-water proceed from common salt dissolved in it, because if it appeared to be so, we might the more hopefully attempt to obtain by distillation sweet water from sea-water; since, if this liquor be made by the bare dissolution of common salt in the other, it is probable that a separation may be made of them, by such a heat, as will easily raise the aqueous parts of sea-water, without raising the saline, whose distillation requires a vehement heat, as chymists well know to their cost. And such a method of separating fresh water from that which was salt, would make our doctrine of use, and be very beneficial to navigation, and consequently to mankind. For, in long voyages, it is but too common for the makers of them, to be liable to hazards and inconveniencies, for want of fresh and sweet water, whereby they are sometimes forced to drink corrupt brackish water, which gives them divers diseases, as particularly the scurvy, and the usual effect of drinking salt water, the dropy. And seamen are wont to receive so many other incommodities by the want of fresh water, that, to prevent or supply it, they are oftentimes forced to change their course, and sail some hundreds of miles to a coast, not only out of their way, but unsafe in itself, and perhaps more dangerous, by being infested by pirates, or in the hands of enemies or savage people; by which means, they often lose the benefit of their Monsoons, and much more easily other winds, and frequently their voyage. And these are inconveniencies which might be in good measure prevented, if potable, and at least tolerably wholesome water could be obtained by distillation, in the midst of the sea itself, to serve the seamen, till they could be supplied with naturally fresh water. To make some trials of this, I remember I took some English sea-water, whence I was able to separate betwixt a thirtieth and fortieth part of dry salt; and having distilled it in a glass head and body, with a moderate fire, till a considerable portion of it was drawn over, we could not discern any saltness in it by the taste; and besides that I found it specifically lighter than such water as is daily drank by persons of quality at London, I exposed it to more chymical examen, and did not by that find any thing of sea-salt in it, though I have at several times, by the same way, manifestly discovered a saltness in inland waters that are drank obviously for sweet waters. If I would have employed a stronger heat, and vessels larger and lower, or otherwise better contrived for copious distillation, I might in a shorter time have obtained much more distilled water; but whether such liquors will be altogether so wholesome, experience must determine. Yet that sea-water distilled even in no very artificial way, may be so far wholesome, as not in haste to be sensibly noxious, but at a pinch useful, at least for a while, may be gathered from (what occurs to me since the writing of the last paper) the testimony of that famous navigator Sir R. Hawkins, who commanded a fleet in the *Indies* for Queen Elizabeth. For he, in the judicious account he gave the world of his voyage, wherein they

they were distressed, even in the admiral's ship, for want of fresh water, has this memorable passage, as I find it verbatim in our diligent *Purchas* \*.

“ALTHOUGH our fresh water had failed us many days (before we saw the shore) by reason of our long navigation without touching any land, and the excessive drinking of the sick and diseased (which could not be excused); yet with an invention I had in my ship, I easily drew out of the water of the sea sufficient quantity of fresh water, to sustain my people, with little expence of fuel; for with four billets I stilled a hoghead of water, and therewith dressed meat for the sick and whole. The water so distilled we found to be wholesome and nourishing.”

AND because the potableness of sea-water may concern the healths and lives of men, I shall here add to what I elsewhere deliver about my ways of examining, whether other waters participate of salt, two or three observations I made upon those few distilled liquors I had occasion to draw from sea-water. Having then, upon some of the distilled liquor, dropped a little oil of tartar per deliquium, I perceived no clouds at all, or precipitation to be made; whereas a small proportion of that liquor being dropped into the undistilled sea-water itself, it would presently trouble and make it opacous, and, though but slowly, strike down a considerable deal of a whitish substance (which, of what nature it is, I need not here declare); I found also, that a very small proportion of an urinous spirit, such as that of sal armoniac, would produce a whitish and curled substance (but not near so copious a one as the other liquor) in sea-water, not yet exposed to distillation, but not in the liquor drawn from it; which argued, that there were but few or no saline particles of sea-salt ascended with the water; for else these alkalizate and urinous salts would in all likelihood have found them out, and had a visible operation on them. And I farther remember, that when the distillation was made in glass vessels, with an easy fire, not only the first running, but the liquor that came over afterwards, was not perceived to be brackish, but good and potable. To which agrees very well, that by a hydrostatical trial I found our distilled sea-water to be lighter in specie than common conduit-water, though it exceeded that in specifick levity, less than it was surpassed in the same quality by distilled rain-water.

BUT to return to the subject, whence we have somewhat, but, I hope, not uselessly digressed; I know it may be objected, that if the terrestrial salts carried by springs, rivers, and land floods into the sea, were the cause of its saline taste, those waters themselves must be made salt by it, before they arrive at the sea. But besides that this objection will not reach the springs and rivers of salt water, that in several places, either immediately or mediately, discharge themselves into the sea; it might conclude against him, that should affirm this imported saltness to be the only cause of that of the sea; but it will not be of force against me, who take it to be only a partial cause, that by its accession contributes to the degree of saltness we observe in the sea, where this imported salt may join itself with the salt it finds there already, and being detained by it contribute to the brinynefs of the water.

If it be urged, that from hence it will follow, that the sea from time to time increases in saltness, I may suspend my answer till it appear by competent observation, that it does not; which, I think, men have not yet made trials that may warrant them to assert. And if the matter of fact were certain, I think it were possible to give a farther answer, and shew probable ways, how so small an accession of salt may be dispersed by nature, and kept from increasing too much.

\* In lib. VII. p. 1378 of *Purchas*, out of Sir R. Hawkins's voyage.



## C H A P. III.

BUT now it is feafonable to confider, that the tafte of fea-water is not fuch a fimple faline tafte as fpring-water would receive from fal gem, or fome other pure terreftrial falt difsolved in it, but a bitterifh tafte, that muft be derived from fome peculiar caufe, that authors are not wont to take notice of. I am not affured by any obfervations of my own, that this recession, from a purely faline tafte, is likely to be of the very fame kind, and to be equally, or very near equally met with in all feas (not to add a doubt, whether it be at all fenfible in fome). The caufe both of the bitternefs, and faltnefs too, of the fea-water, is faid to be affirmed by the learned Mr. *Liddat*, to be aduft and bituminous exhalations afcending out of the earth into the fea. But that there is abundance of actual falt in the fea-water, to give it its faline tafte and ponderoufnefs, the falt that the fun does in many places copioufly feparate from the faltlefs waterifh parts, fufficiently manifefts. But as to the bitterifh tafte, I think it no eafy matter to give a true account of it, but am prone to afcribe it, partly, to the operation of fome catholic agents upon that vaft body of the ocean, and partly, to the alteration that the falt receives from the mixture of fome other things, among which bitumen may be one of the principal.

BUT though I have, in another\* paper fhewn, that in fome places of the fea there are confiderable quantities of bitumen, or bituminous matter to be met with; yet I dare not derive the bitternefs of the fea only from bituminous exhalations, but in good part, at leaft in fome places, from the liquid and other bitumen that is imported by fprings and other waters into the fea; of which we have an eminent inftance in that which our Englifh call *Barbadoes* tar, according to the relation I had of it from an inquisitive gentleman, who is one of the chief planters of the ifland, and took pleafure to obferve this liquid bitumen to be carried in confiderable quantities from the rocks into the fea; and I think it poffible enough, that fome of the fprings that rife under the furface of the fea, may carry up with them bituminous matter, which may help to make the faltnefs of the fea degenerate (of which more perhaps elfewhere) as I not long fince made mention of fprings, as well of hot, as cold water, rifing beneath the furface of the fea. And this minds me to intimate here, that I have fufpected, that in fome places the fulphureous exhalations, and other emissions from the fubmarine parts of the earth, may fometimes contribute to change the faline tafte of the fea-water; for I have elfewhere related, how, not only fulphurous fteam, but fometimes actual flames, have broken through from the lower parts of the fea to the uppermoft; and have fometimes taken pleafure to make, by art, a rude imitation of that phaenomenon. And partly fome experiments of my own, and partly fome other inducements, have perfuaded me, that divers times (for I do not fay always) fea-falt does not obfcurely participate of combuftible fulphur, of which I may fpeak farther on another occafion. But in regard, that the tafte of the fea-water is not in all parts of the ocean uniform, it may here fuffice to take notice, in general, that this difference of tafte may partly be caufed by adventitious bodies of feveral kinds, of which it is probable, that in differing places the fea-water does variously partake. And not to mention here the fragrant finell of violets, which has, by feveral, and particularly by an eminent perfon, of whom I enquired about it, been obferved, in fome hot countries, to proceed from fea-falt; I have divers other inducements to think that it is ufually no fimple falt, nor free from mixture. For by more ways than one, and particularly by

\* In the Tract of Subterranean Menftrua.

cobobating from it its own spirit, we have obtained a dry sublimate, which seemed to be no pure but a compounded body.

AND now to come to that, which I intimated might be one of the causes why the taste of sea-water is not the same with that of common salt dissolved in fresh water; I shall add, that I have suspected, that the various motion of the sea, and its being exposed to the action of the air and sun, may contribute to give it a taste other than saline; which suspicion might be confirmed by the observation I elsewhere mention of the sea-salt, which, by barely being exposed for many months to the air, and sometimes, perhaps, put into a gentle agitation by a digestive heat, I found to have a very manifestly differing taste from the simple solution of sea-salt in common water.

I MIGHT here endeavour the farther confirmation of my discourse, by what I have learned by inquiry from navigators, about the manifestly differing colours and other qualities, of the differing parts of the sea, which seem to argue that it is not every where of such a uniform substance as men vulgarly imagined, and that vast tracts of it are imbued with stupendous multitudes of adventitious corpuscles, which, by several ways diversifying its parts, keep it from being a simple solution of salt. But of this subject I would not leisure to discourse here; only because it is generally thought, that the sea water is, by reason of the saltness it abounds with, incapable of putrefaction, I will add, that having kept a pretty quantity of sea-water, that I had caused to be purposely taken up between the English and French shores, in a good new rundlet, in a place where the summer-sun beat freely upon it, it did, in a few weeks, acquire a strongly stinking smell; though that the experiment had been more satisfactory, I wished that it had been made in a vessel of glass, or earth, instead of wood. But a much better observation I procured from a much esteemed navigator of my acquaintance, who having sailed often in the Indian and African seas, I inquired of him, whether he had ever, in those hot climates, where the sea is supposed to be very salt, observed it to stink, for want of agitation, or otherwise; to which he answered, that once being, though it was but in *March*, becalmed, in a place he named to me, for twelve or fourteen days, the sea, for want of motion, and by reason of the heat, began to stink, in-somuch that he thinks, if the calm had continued much longer, the stench would have poisoned him: they were freed from it as soon as the wind began to agitate the water, and broke the superficies, which also drove away store of the sea-tortoises, and a sort of fish, whose English name I know not, that before lay basking themselves on the top of the water.

AND to this agrees very well the notable observation that I since met with, of the elsewhere commended Sir R. Hawkins, who, among other considerable things he takes notice of in his relations, has this passage, to our present purpose. \* “ Were it not for the moving of the sea by the force of winds, tides, and currents, it would corrupt all the world. The experience I saw, *anno* 1590, lying with a fleet about the islands of *Azores*, almost six months, the greatest part of the time we were becalmed; with which all the sea became so replenished with several sorts of gellies, and forms of serpents, adders, and snakes, as seemed wonderful, some green, some black, some yellow, some white, some of divers colours, and many of them had life; and some there were a yard and a half, and two yards long; which, had I not seen, I could hardly have believed. And hereof are witnesses all the company of the ships, which were then present, so that hardly a man could draw a bucket of water clear of some

\* Purchas's Pilgrims in Sir R. Hawkins's Observations.



“ corruption. In which voyage, towards the end thereof, many of every ship fell sick  
 “ of this disease, and began to die apace, but that the speedy passage into our country  
 “ was a remedy to the crazed, and a preservative for those that were not touched.”

## THE THIRD SECTION.

### C H A P. I.

AS for the various degrees of the saltness of the sea, authors are wont to be silent of it, save that some navigators tell us, that they observed some seas to have a more, and others a less saline taste; which, you will easily believe, has not afforded me much satisfaction. And, on the other side, my want of opportunity to make trials myself will confine me to acquaint you with no more than the few following observations.

1. To a learned man that was to sail to places of differing latitudes in the Torrid Zone, I delivered a glass instrument, elsewhere described, fitted by the greater or lesser emersion of the upper part, to shew, accurately enough for use, the greater or less specific gravity of the salt-water it was put to swim in. This he put, from time to time, into the sea-water, as he sailed towards the *Indies*, whence he wrote me word, “ That he found, by the glass, the sea-water to increase in weight, the nearer he came to the Line, till he arrived at a certain degree of latitude, as he remembers, it was about the thirtieth; after which, the water seemed to retain the same specific gravity, till he came to the *Barbadoes* or *Jamaica*.”

2. ANOTHER observation I obtained by inquiry of an ingenious person, and a scholar, at his return out of the *East-Indies*, who affirmed to me, that he, and a gentleman of my acquaintance, took up bottles full of sea-water, both under the Equinoctial, and also off the *Cape of Good Hope*, which lies in about thirty-four degrees of southern latitude, and found the waters of these distant parts of the ocean to be of the same weight. And though it may well be doubted, whether this observation, being made with ordinary bottles, were so exact as could be wished; yet the persons being curious, and making it for their own satisfaction; and my relator having, in both the recited places, filled with the sea-water he took up, and weighed; having, I say, filled the same bottles, since this vessel held two quarts (which must be above four pounds of salt water) if the disparity of weight had been considerable, it would, in likelihood, have been found, at least manifestly sensible, in such a weight of liquor.

3. ENQUIRING of an observing person, that had been at *Mosambique*, which is thought to be one of the hottest places in the world, whether he did not there find the sea to be more than ordinarily salt? he answered me, that coming thither in a great carack, when he came back from the town to the ship, he observed near two hands breadth of the vessel to be above the ordinary part to which it used to sink; insomuch, that he took notice of it to the captain, as fearing that part of the lading had been by stealth carried to the shore; but the pilot, who had made thirteen or fourteen voyages to the *Indies*, assured him, what he had observed about the ship was not unusual in that place, where the taste itself discovered the water to be exceeding salt.

Nor.

NOR need we scruple to think, that some sea-waters may be very much more impregnated with salt than ours; for water will naturally dissolve, and retain a far greater proportion of salt, than that which is commonly met with in the sea. For whereas a thirty-fifth, or thirtieth, or at most a twenty-fifth part of salt, will make water more saline than is found in many seas, I am, by a friend of mine, that is master of a salt-work, informed, that the water of his springs afford him a twelfth part of good white salt, and that another spring, not far off, yields no less than an eighth part. To which (to avoid anticipation) I shall not here add, what I shall hereafter have occasion to say of the fullest impregnation of water with common salt.

[WHILST I was reviewing these papers, there came seasonably to my hands a letter written from *Muslapatan*, on the gulf of *Bengala* in the *East-Indies*, by an ingenious gentleman, Sir *William Langborn*, that is entrusted with the care of the English factories in those parts; out of which letter the following passage is *verbatim* transcribed. "I did, in order to your command, cause some water to be saved under the Line, at our first access to it, intending, for want of good scales and weights (being none to be come at aboard the ship) to have kept it, until it could be weighed, but by the forgetfulness of a servant, it was thrown away. Off the *Cape*, in 37 d. 00 m. southern latitude, I saved some again, and, through the same want of weights, was fain to keep it, until I came to the Line again; and then made the best shift I could for weights, and compared it with the water there, filling the same bottle again to the same height by a mark, and found it exactly the same weight. The weight I have taken; but accounting this a journey of business, left those notes, and most of the like nature, behind me; in my next it shall be inserted."]

## C H A P. II.

It remains now, that, according to my promise, I set down what I observed myself concerning the saltness of our sea between *England* and *France*; not in comparison with the saltness of other seas, whose waters I had not to compare with, but as to the proportion of salt contained in it to the water. And though one would think it very easy to make trials of this sort, for a person not unacquainted with hydrostatical practices, nor unfurnished with instruments, yet, I confess, that three or four trials that I made, not all of them the same way, made me find it more difficult, than was imagined, to arrive at any thing of certainty in this enquiry.

THIS you will easily believe, if I annex the substance of some experiments, that I remember I made about the gravity of sea-water, which I had ordered to be taken up, some at the depth of about fifteen fathoms somewhat near our shore, and some in another place of the channel, between *England* and *France*.

THE sum of the first experiment is this: we took a phial, fitted with a long and strait neck, purposely made for such trials, and having counterpoised it, filled it to a certain height with common conduit-water: we noted the weight of that liquor, which being poured out, the phial was filled to the same height with sea-water taken up at the surface, and by the difference between the two weights, the sea-water appeared to be about a forty fifth part heavier than the other.

THE second trial (which was far more accurately made hydrostatically) I find registered to this effect: we carefully counterpoised in the scales, formerly made use of, a piece of sulphur in the upper sea-water, formerly mentioned; it weighed  $3\text{ } \frac{1}{2} + \text{gr. x. } \frac{1}{2}$ , and being also weighed in the sea-water fetched from the bottom, gave us the same weight



weight  $\frac{3}{4}$  lb + gr. x. lb which shewed those two waters to be of the same specifick gravity: and then to compare this with the gravity of common water, we weighed the same sulphur in common conduit-water, and found it  $\frac{3}{4}$  lb + gr. xv. lb: by which it appeared, that the sea-water was but about a fifty-third part heavier than this water: which is such a difference from the proportion found out by the former way of trial, that I could not well imagine what to attribute it to, unless the sea-water by long standing in a vessel, which, though covered, was exposed to the hot sun, may both have been rarefied, and have had some separation made of its saline or other heavier parts, on which score that portion we took up for our trial, might appear lighter than else it would have done; or unless the experiment having been made in *London*, where great and sudden rains and other accidents will sometimes visibly vary the consistence of common water, the liquor, I then employed without examining it, might be more ponderous at that time than at another. To which latter suspicion I was the more inclined, because, having afterwards weighed the same piece of sulphur by help of the same balance in distilled rain-water, I found the weight of the former liquor to exceed that of the latter by a good deal less than a thirty-fifth part; which seemed to make it probable, that if the water we chanced to employ, had been free from all saline and other heavy particles, the difference formerly mentioned betwixt this observation and the foregoing would not have been near so great as it was.

THE last way I made use of to examine the proportion betwixt sea-water and fresh, was chymical; whereof my register affords me this account.

A POUND avoirdupoise weight of the upper sea-water was weighed out, and put into a head and body to be distilled in a digestive furnace *ad siccitatem*; and the distillation being leisurely made, the bottom of the glass was almost covered with fair grains of salt, shot into cubical figures, and more white than was expected: in the rest of the coagulated matter, we took not notice of any determinate shape. The salt being weighed, amounted to  $\frac{3}{4}$  lb, avoirdupoise, and gr. x. At which rate, the proportion of the salt to the water will be that of 30 and  $\frac{2}{3}$  to one, and so will amount to near the thirtieth part; which was so much greater than the former ways of trial made us expect, that I know not whether it may not be worth while to try, whether such a slow abstraction, as we employ of the superfluous water, and our doing it in close vessels, may not have afforded us more salt, than else we should have obtained.

To this relation I find this note subjoined; suspecting that there may have somewhat else concurred to our finding so great a proportion of salt, I suffered that which had been weighed, to continue a while in the scale, and soon perceived, that, according to my conjecture, that scale began manifestly to preponderate, and that consequently some of the unexpected weight of salt may be due to the moisture of the air, imbibed after the salt was taken out of the glass, and laid by to be weighed: wherefore, causing it to be very well heated and dried in a crucible, we found it to weigh  $\frac{3}{4}$  liij + lb (that is 210 grains) upon which account the proportion of salt contained in the water, was a thirty-sixth part, and somewhat above half of those parts, and to express it in the nearest whole number, a thirty-seventh part.

FROM whence this greater proportion of salt by distillation, than our other trials invited us to expect, proceeded, seems not so easy to be determined; unless it be supposed (as I have sometimes suspected) that the operation the sea-water was exposed to in distillation, made some kind of change in it, other and greater than before-hand one would have looked for; and that, though the grains of salt we gained out of the sea-water, seemed to be dry before we weighed it, yet the saline corpuscles, upon their concreting into cubes, did so intercept between them many small particles of water, as not to

suffer them to be driven away by a moderate warmth; and consequently such grains of salt may have upon this account been less pure and more ponderous than else they would have been. And I might here add, that I sometimes make a certain artificial salt, which, though being dissolved in water, it will shoot into crystals finely shaped, and dry enough to be reducible into powder, yet coagulates water enough with it to make the water almost, if not quite, as heavy again as before. And I have been assured by a very learned eye-witness, that there is a sort of sea-salt, which they bring to some parts of *England*, from the coast of *Spain* or *Portugal*, which being here dissolved, and reduced by purification and filtration to a much whiter salt, will yield by measure somewhat above two bushels for one. But to satisfy the scruples and suspicions I could suggest, would require more trials than I have now time or opportunity to make. What has been already delivered, may give at least as scrupulous an account of the saltiness of our *English* sea-waters, as most other experimenters would have thought it needful to give. And to make a determination with any certainty about the degrees of the sea's saltiness in general, a great number of observations, made in different climates, and in distant parts of the ocean, would be necessary.

## C H A P. III.

I KNOW not whether I may be so indulgent to my suspicions, as to wish, that observations were heedfully made, whether in the same sea, and about the same part of it, the waters be always equally salt? For, though that be taken for granted, yet since we have no good observations long since made to silence the suspicion, one may suspect, that, at least in many places, the saltiness of the sea may continually, though but very slowly, increase by the accession of those saline corpuscles, that are imported by salt-springs, and those which rivers and land-floods do from time to time rob the earth of. And I suspect it to be not impossible, that this, or that part of the sea, may be sometimes extraordinarily, and perhaps suddenly, impregnated with an additional saltiness from saline steams plentifully ascending into it, from those subterranean fires, about which I have made it elsewhere probable, that they may burn beneath the bottom of the sea, and sometimes send forth copious exhalations into it. But it may prove the more difficult to discern this adventitious saltiness, unless the taste, as well as balance, be employed about it; because the salt, that produces it, may be of such a nature, as to be much lighter in specie than common sea-salt. And the mention of this leads me to give you here the advertisement I promised you not long ago.

THAT though the weight of sea-water be as good a way as is yet employed (and better than some others) to determine what sea-water does most abound in salt; and though it be possible, that in our sea, and perhaps in almost all others, this way be not liable to any considerable uncertainty; yet I think it not impossible, that it may sometimes deceive us, especially in very hot regions; because I have observed, that there may be volatile salts, which, though by reason of their activity, they make smart impressions on the tongue, and give the water imbued with them a strong saline taste, yet they add very little, and much less than one would think, to its specific gravity: as I have tried, by hydrostatically examining distilled liquors, abounding in volatile and urinous salts, some of which I found very little heavier than common water, and consequently nothing near so much heavier, as they would have been made, if they had been brought to so sharp a taste, by having nothing but common sea-salt dissolved in them: so that,

if



if in any particular place, by any other way, or from the steams of the earth beneath, some of which, I elsewhere shew, may be very analogous to those afforded by sal armoniack) the sea should be copiously impregnated with such kind of light salts, the sea-water may be much more salt to the taste, and yet be very little heavier. For confirmation of which I find among my notes, that weighing a sealed bubble of glass, made heavy by an included metal, first in spirit of sal armoniack, that tasted much stronger than sea-water, it weighed  $3\text{ iij} + \text{gr. } 51 \frac{1}{4}$ , and weighing this same body in fair water, it weighed but  $3\text{ iij} + \text{gr. } 45 \frac{1}{4}$ ; so that notwithstanding its great saltness, the spirit was lighter than common water; though a good part of that comparative levity may probably be ascribed to the liquor wherein the saline particles swarm, which, by distillation, was grown more defecated and light, than common, though clean, water.

BUT for a farther proof, we took a hard lump of sal armoniack; and though we could not weigh it in water, because that would have dissolved part of it, yet by a way (I elsewhere teach) I found that weighing in the same liquor this lump of sal armoniack, and a lump of good white sea-salt (brought me as a curiosity out of the Torrid Zone) the proportion of the latter to a bulk of the liquor equal to it, was something (though exceeding little) above that of two and a quarter to one, and the proportion of sal armoniack to as much water, as was equal likewise to it, did not above a centesim exceed that of one and  $\frac{7}{10}$  to one; which falls so short of the other proportion, as may justly seem strange, especially if it be considered, that the sacitious sal armoniack, the chymists generally use, and we employ, consists in good part of sea-salt, which abates much of the comparative levity it might have, if it were made up only of urinous and fuliginous salts, which were its other ingredients.

It were indifcreet for me to propose any more suspitions and trials fitted to clear them, unless I knew those I have already mentioned would not pass for extravagancies; and therefore, I shall here dismiss the subject of this tract of the saltness of the sea, but that, since I have been discoursing of the degrees of it, it will not be impertinent to add, what is the greatest measure of saltness, that I have brought water to, without the help of external heat. On this occasion, I employed two differing ways: the one was, by putting into a well counterpoised phial two ounces of common water, and then putting into it, well dried and white common salt, and shaking them together, till the liquor would, whilst cold, dissolve no more: this liquor, thus glutted with salt, weighed 1150 grains, from which two ounces being deducted, the overplus of weight, arising from the dissolved salt, amounting to 150 gr. so that a parcel of salt will, without heat, be dissolved in about five times its weight, or very little more, of common water. By which proportion we made so strong a brine, that divers pieces of amber being purposely let fall into, emerged, and floated on it. The other and better way, yet more tedious, that we made use of, was to let sea-salt run *per deliquium* (as the chymists speak) that is, to set it in some moist place, till it was dissolved by the aqueous vapours that swim in the air. In this liquor we weighed a piece of sulphur, which we also weighed in sea-water, wherein, finding it to weigh much more than in the former liquor, it appeared, that the sea-water was, in specie, much lighter than the other; though how much their gravities differed, I cannot find among my notes, nor be informed by my memory.

AND because I have not, in any author, met with the proportion of sea-salt to water of the same bulk, nor perceive, that hydrostaticians themselves have yet attempted any way to investigate it (probably deterred by the easy dissolubleness of salt in water) I shall here subjoin, that by the help of an expedient, I have elsewhere taught, I have examined a hard dry lump of sea-salt, and found its proportion in weight, to common

water of the same bulk, to be almost as 2 to 1 (for it exceeded the ratio of  $1\frac{2}{3}$  to 1). And, I remember, I found the specifick weight of a hard and figured lump of *sal gem.* (which sort of salt I suppose to be somewhat more pure and ponderous than sea-salt) to be to that of water (very near) as  $2\frac{1}{3}$  to 1.

## THE FOURTH SECTION

BELONGING TO THE TRACT, ENTITLED,

### Relations about the Bottom of the S E A.

THE presence of the air is not only so necessary to the life of many sorts of animals, but it hath likewise so great a stroke in the growth of vegetables, especially of the larger sorts, that, after what I had experimented about these matters (of which this is not the proper place to give an account) I thought fit to make enquiry about the vegetation and growth of plants of considerable bulk, in those submarine regions, where, if there grow any, they must do it remote from the free contact of an ambient air. And having not now the leisure to repeat what botanists (of whose books I am not now provided) deliver about lesser plants growing under water, I shall now only present you with what information I could procure from navigators, about trees and fruits growing at the bottom of the sea.

To what I have elsewhere had occasion to say to their opinion, that will not allow coral to be really a stony plant, but a lifeless concrete, that is always hard and brittle under water; I shall now add, that, enquiring lately of an eminent and inquisitive person, that had spent some time upon the coast of *Africa*, where he had been present at the fishing of coral, and learning from his answer, that he had seen it not far from *Algiers*; I asked him, whether he had himself observed the coral to be soft, and not red, when it was newly brought from the bottom of the sea. To which he replied, that he had found it soft and flexible; and that, as for the colour, it was for the most part very pale, but with an eye of red, the bark being worse coloured than the substance it covered was; but when the bark was taken off, and the other part exposed to the air, the expected redness of the coral disclosed itself.

WHEN I demanded whether he had observed that any inky sap ascended to nourish the stony plant? and whether he had seen any thing like berries upon it? he ingenuously confessed to me, he had not been so curious, as purposely to make enquiry into those particulars; but that he remembered, that having broken some of the large pieces of coral, he took notice that the more internal substance was much paler than the other, and very whitish; and that at the extreme parts of some branches, or sprigs, he observed little blackish knobs, which he did not then know what to make of: and when I enquired what depth the sea was of in that place? he answered, that it was nine or ten fathom.

But



But as to the fruit of some kinds of coral, if I do not much misremember, I was, not long since, assured by a scholar, that navigated much in the east, that they divers times meet with in those seas a certain sort of coral, but not white, which bears a small fruit like a round berry, of a pleasant colour, and esteemed as rarities.

DISCOURSING with a person that made diving his trade, whether he had not met with any trees or fruit in the depths of the sea? he told me, that in a great ship, where-into he descended, to recover thence some shipwrecked goods, he was surprized to find in several places a certain sort of fruit, that he knew not what to make of, for he found them of a slimy and soft consistence, about the bigness of apples, but not so round in shape; and when he brought them up into the air, as he did many of them, they soon began to shrink up like old rotten apples, but were much harder, and more shrivelled. And it is remarkable, that this happened in a cold northern sea.

ONE that made a considerable stay about *Manar*, a place I have often mentioned, answered me, that he learned from the divers, that in some places thereabouts, there grows at the bottom pretty store of a certain sort of trees, bearing leaves almost like those of laurel, as also a certain fruit; but of what virtue, or other use, he had not the curiosity to enquire.

I WAS also informed by an eye-witness, that near the famous coast of *Mosambique* in *Africa*, there grows at the bottom of the sea store of trees, that bears a certain fruit, which he describes to be very like that, which, in *America*, they are wont to call *Acayu*, the leaves also resembling those of that tree.

BUT the best welcomest information I could procure about submarine plants, is that which concerns the famous Maldivian nut, or Cocoa, which is so highly esteemed in the east, that some write, it is a great present from one king to another, and even much extolled in *Europe* by experienced physicians: for the origin of this dear drug is almost as much controverted, as the alexiterial virtues are extolled. Having then once the good fortune to meet with a man of letters, that had resided in those unfrequented islands, I found he had been as inquisitive, as I could reasonably expect, about these admired productions of the sea, and that he had often learned from the divers, that they are real nuts, or fruits, borne by a sort of cocoa-trees, that grow at the bottom of the sea, and are thence, either torn off, by the agitation of the water, or gathered by the divers. These fruits are smaller than most other sorts of cocoas, whose maturity they do not seem to arrive at. He thinks, the species may have been very differing from what it is, and may have come from nuts fallen into the sea, together with the ruin of some little islands undermined by the water, and so submerged; of which he told me, he saw, at least, three or four instances during his stay there. He told me, that whilst the fruit was under water, they observed no distinct shell and kernel, but the entire nut was so soft, that it may easily enough cut with a knife, and was eaten like their other fruits; but being kept about a week in the hot air, it grows solid, and so hard, as to require good steel tools to work upon it. He added, that though, even upon the place, the fairer sort be of very great esteem, yet not of any such prodigious price as is given out. And he presented me one about the bigness of a large egg, and a fragment of another, which are both very hard; but as for their virtues, I can yet say nothing upon trial, for want of having had fitting opportunities.

OTHER observations made at the bottom of the sea may hereafter follow.

A  
P A R A D O X  
OF THE  
NATURAL AND PRETERNATURAL STATE  
OF  
B O D I E S,  
Especially of the AIR.

**I** Know that not only in living, but even in inanimate bodies, of which alone I here discourse, men have universally admitted the famous distinction between the natural and preternatural, or violent state of bodies, and do daily, without the least scruple, found upon it hypotheses and ratiocinations, as if it were most certain, that, what they call nature, had purposely framed bodies in such a determinate state, and were always watchful, that they should not by any external violence be put out of it.

BUT notwithstanding so general a consent of men in this point, I confess I cannot yet be satisfied about it in the sense wherein it is wont to be taken. It is not, that I believe, that there is no sense in which, or in the account upon which, a body may be said to be in its natural state; but that I think the common distinction of a natural and violent state of bodies has not been clearly explained, and considerably settled, and both is not well grounded, and is oftentimes ill applied. For when I consider that whatever state a body be put into, or kept in, it obtains or retains that state, according to the catholic laws of nature, I cannot think it fit to deny, that in this sense the body proposed is in a natural state, but then, upon the same ground it will be hard to deny, but that those bodies which are said to be in a violent state, may also be in a natural one, since the violence they are presumed to suffer from outward agents, is likewise exercised



no otherwise than according to the established laws of universal nature. It is true, that when men look upon a body as in a preternatural state, they have an idea of it differing from that, which they had whilst they believed it to be in a natural state: but perhaps this difference arises chiefly from hence, that they do not consider the condition of the body, as it results from the catholic laws settled among things corporeal, and relates to the universe, but estimate it with reference to what they suppose is convenient, or inconvenient, for the particular body itself. But however it seems to me, that men's determining a body to be in a natural or preternatural state has much more in it, either of casual, or of arbitrary, or both, than they are aware of. For oftentimes we think a body to be brought into a violent state, not because really the former was not so, but because there is a notable change made in it by some agent, which we also take notice of; whereas before the action of that agent, if the body were under any violence, it was exercised by usual, but often immanifest agents, though perhaps their compulsion were not less, but only less heeded. And sometimes also no more is to be understood by a body's being forced from its natural state, than that it has lost that, which it had immediately, or a pretty while before some notable change. Which conjectures I shall now endeavour to confirm, but with great brevity.

I HAVE already shewn, that matter being devoid of sense and appetite, cannot be truly and properly said to affect one state or condition more than another, and consequently has no true desire to continue in any one state, or to recover it when once lost; and inanimate bodies are such, and in such a state, not as the material parts they consist of, elected or desired to make them, but as the natural agents, that brought together and ranged those parts, actually made them. As a piece of wax is unconcerned, whether you give it the shape of a sphere, or a cone, or a pillar, or a boat; and whether, when it has that form, you change it into any other; the matter still retaining without willingness or unwillingness, because without perception, that figure, or state, which the last action of the agents (your fingers or instruments) determined it to, and left it in.

BUT this will be best understood, as well as confirmed, by particular examples. I need not tell you, that the most usual instance alledged to shew, that a state is natural to a body, and that being put out of it by external causes, it will, upon the cessation of their violence be restored thereunto, is, that water being heated by the fire, as soon as that adventitious heat vanishes, returns to its native coldness; and so when, by an excess of cold, it is congealed into ice, it does upon a thaw lose that preternatural hardness, and recover the fluidity, that naturally belongs to it: and the same may be likewise said of butter, which, being melted by external heat into a liquor, does upon the cessation of that heat grow a consistent body again. But perhaps these instances will rather countenance our paradox than disprove it. For as to the coldness, whereto water heated by the fire returns, when it is removed thence, it may be said, that the acquired heat consisting but in the various and brisk agitation of the corpuscles of the water by an external agent, it need be no wonder, that when that agent ceases to operate, the effect of its operation should cease too, and the water be left in its former condition, whether we suppose it to have been heated by the actual pervasion of the corpuscles of the fire, which must by degrees fly away into the air; or that the heat proceeds from an agitation imparted by the fire to the aqueous corpuscles, which must, by degrees, lose that new agitation, by communicating it little by little to the contiguous air and vessel; so that if the former agitation of the particles of the water were, as is usual, much more languid than that of our organs of feeling, in which faintness of motion the coldness of water consisted, there will be no need of any positive internal form, or any care of nature to account for the water's growing cold again. This will be confirmed by

by the consideration of what happens to ice, which is said to be water brought into a preternatural state by an excess of cold. For I doubt it will not be easily demonstrated, that in reference to the nature of things, and not to our arbitrary ideas of them, ice is water preternaturally hardened by cold, and not water ice preternaturally thawed by heat. For if you urge, that ice left to itself will, when the frigorific agents are removed, return to water; I shall readily answer, that, not to mention the snow and ice, that lie all the summer long unthawed upon the tops of the Alps and other high mountains, I have learned, by enquiry purposely made, from a doctor of physick, who for divers years practised in *Muscovy*, that in some parts of *Siberia* (a large province belonging to the Russian emperor) the surface of the ground continues more months of the year frozen, by what is called the natural temperature of the climate, than thawed by the heat of the sun; and that a little beneath the surface of the ground, the water that chanceth to be lodged in the cavities of the soil, continues frozen all the year; so that, when in the heat of summer the fields are covered with corn, if then you dig a foot or two, perhaps less, you shall easily find ice and a frozen soil: so that a man born and bred in the inland part of that country, and informed only by his own observation, may probably look upon water as ice violently melted by that celestial fire, the sun, whose heat is there so vehement in their short summer, as to ripen their harvest in less time than in our temperate climates will easily be credited.

On the other side, we in *England* look upon melted butter, as brought into a violent state by the operation of the fire, and therefore think, that when being removed from the fire it becomes a consistent body again, it has but recovered its native constitution. Whereas there are divers parts of the *East Indies*, and, I doubt not, of other hot countries, whose inhabitants, if they should see consistent butter (as sometimes by the care and industry of the Europeans they may do) they would think it to be brought to a preternatural state by some artificial way of refrigeration. For in those parts of the *Indies* I speak of (though not in all others) the constant temper of the air being capable to entertain as much of agitation as suffices for fluidity in the parts of what, in our climate, would be butter, it would be in vain to expect, that, by being left to itself in the air, it should become a consistent body. And I have learned by diligent enquiry of seamen and travellers, both English, and others that were eye-witnesses of what they told me, that in divers parts of those hot regions, butter, unless by the Europeans, or their disciples, purposely made in the cold, is all the year fluid, and sold, or dispensed, not as consistent bodies, by weight, but as liquors, by measure. To strengthen this observation, I shall add what was affirmed to me by a learned man, that practised physick in the warmer parts of *America*, namely, that he met in some places with several drugs, which, though they there seemed to be balsams, as turpentine, &c. are with us, and retained that consistence in those climates; yet when they come into our colder regions, harden into gums, and continue such both winter and summer. On the other side, enquiring also of a traveller, versed in physical things, about the effects of great heat in the inland parts of *Africa*, where he had lately been; he told me, among other things, that resin of jalap, which, when he carried it out of *England*, was of a consistence not only dry, but brittle, did, when, and a while before he came to *Morocco*, melt into a substance like turpentine; so that some of it, that he had made up into pills, would no more at all retain that shape, but remain, as it were, melted all the while he staid in that city, and the neighbouring country, though when he came back to the borders of *Spain*, it returned to its former consistence. Which I the less wondered at, because, having had the curiosity to consider some parcels of gum lacca (of which sealing-wax is made) newly brought ashore from the *East Indies*, though it be a hard and solid gum, yet I found by several instances,



nstances, that, passing through the Torrid Zone, divers pieces of it, notwithstanding the shelter afforded it by the great ship it came in, had been, by the heat of the climate, melted, and made to stick together, though afterwards they regained their former consistence, though not altogether their former colour. And on this occasion I shall add, that I learned by enquiry from a particular acquaintance of mine, who brought me divers rarities out of *America*, that having at the place where it was made, among other things, furnished himself with a quantity of the best aloes, he observed, that whilst he sailed through very hot climates, it was so soft, that, like liquid pitch, it would often have fallen out of the wide-mouthed vessel he kept it in, if he had not from time to time been careful to prevent it. But when he came within a hundred leagues of the coast of *England*, it grew hard, and so continued, though this were in a very warm season of the year, being about the dog-days.

For further confirmation of what has been hitherto discoursed, be pleased to consider with me that most obvious body the air, or the atmosphere we live and breathe in. For though several opinions and argumentations are founded upon what their authors call the natural and preternatural or violent state of the air, yet he that considers, shall find it no easy thing to determine what state of the air ought to be reputed its truly natural state, unless in the sense I formerly told you I employ that expression in. I will not insist on the heat and coldness of the air; for that being manifestly very differing in the heart of winter, and in the heat of summer, and in differing regions of the air, as at the top and bottom of high mountains, at the same time, and constantly in differing regions of the earth, as in *Barbary* and *Greenland*, it will not be so easy to determine, what state is natural to the air. But that only, which I shall now consider, is its state or tone in reference to rarity and density. For since the air is believed to be condensed by cold, and expanded by heat, I demand at what time of the year, and in what country, the air shall be reputed to be in its natural state? For if you name any one time, as the winter or the summer, I will ask, why that must be the standard of the tone of the air rather than another season, or at least exclusively to all others? And the like difficulty may be made about the climate or the place. And these scruples are the more allowable to be proposed, because learned men have delivered, that in some countries the mercury in the Torricellian experiment is kept higher than in others (as in *Sweden* than in *Italy*) and our baroscopes inform us, that oftentimes, in the same place and day, the quicksilver in the same instrument does considerably vary its height; which shews, that the air or atmosphere must necessarily vary its weight, and therefore probably its degree of rarity or density.

BUT I have yet to propose a further consideration in this affair: for what if it shall appear, that neither in winter nor in summer, in *Sweden* or in *Italy*, or in whatever country, region, or season you please, the air we breathe in is in any other than a preternatural state? nay, that even when we have vehemently agitated and expanded it by an intense heat of the fire, it is not yet violently rarefied, but yet violently condensed, unless, in our sense before declared, you understand with me the preternatural state of rarefaction in the air, in reference to the tone it had before the last notable change was produced in it. This will, I question not, seem a surprizing, if not a wild, paradox: but yet to make it probable, I shall only desire you to reflect upon two or three of my physico-mechanical experiments; and there you will see, first, that the air being a body abounding with springy particles, not devoid of gravity, the inferior must be compressed by the weight of all the incumbent. And next, that this compression is so great, that though by the heat of the fire neither others, nor we, could bring a portion of included air to be expanded to above fourscore times its former space; yet without

heat, by barely taking off the pressure of the superior air, by the help of our pneumatical engine, the air was rarefied more than twice as much: and since those experiments were published, I more than once rarefied it to above five hundred times its usual dimensions; so that if, according to what is generally agreed on and taught, a body be then in a preternatural state, when, by an external force, it is kept in a condition, from which it incessantly tends to get free; and if it be then most near its natural state, when it has the most prosperously endeavoured to free itself from external force, and comply with its never ceasing tendency; if this be so, I say, then the air we live in is constantly in a preternatural state of compression by external force. And when it is most of all rarefied by the fire, or by our engine, its spring having then far more conveniency than before to display themselves, which they continually tend to do, it answerably approaches to its natural state, which is to be yet less compressed or not at all. And I have carefully tried, for many months together, that when the air has been rarefied much more than even a vehement heat will bring it to be, yet if it were fenced from the pressure of the external air, it would not shrink to its former dimensions, as if it had been put into a violent state, from whence nature would reduce it to them, but continued in that great and seemingly preternatural degree of extension, as long as I had occasion to observe it. One might here shew, that this odd constitution of the air is so expedient, if not necessary for the motion, respiration, and other uses of animals, and in particular of men, that the providence and goodness of the wise Author of the universe is thereby signally declared; if it were not improper, in such a paper as this, to employ final causes. Wherefore to avoid the imputation of impertinence, I will conclude, by taking notice, that, from what has been delivered, we may learn two things considerable enough, if not in themselves, yet to some passages of the treatise, whereof this paper makes a part. And first, we may deduce from what has been said of the air, that, according to what is noted above, that may sometimes generally be granted, and believed to be the natural state of a body, not which it really affects to be in, or (to speak more properly) has a tendency to attain, but that which it is brought into, and kept in by the action, or resistance of neighbouring bodies, or by such a concurrence of agents and causes, as will not suffer it to pass into another state. And the second thing we may hence learn, is, that whatever men say of nature's never missing her aim, and that nothing violent is durable; yet, bating an inconsiderable portion of aerial particles at the upper surface, for aught we know, the whole mass of the air we live in, and which invirons the whole terraqueous globe, has been from the world's beginning, and will be to its end, kept in a state of violent compression.



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A  
STATICAL HYGROSCOPE

Proposed to be further Tried.

Together with

A BRIEF ACCOUNT  
OF THE  
UTILITIES OF HYGROSCOPES.

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A  
STATICAL HYGROSCOPE, &c.

In a LETTER to H. OLDENBURG, Esq; Secretary to the  
ROYAL SOCIETY.

SIR,

THOUGH I writ to you from *Stanton*, an account of those hygrosopes, whereof I now present you one; yet since I remember, that it was in the year 1665, that I sent you that paper, I fear you may, by this time, have forgotten much of what it contained, and thereby made it fit for me, in this letter, both to remind you of some former passages, and to add some observations, that lately occurred to me; and this the rather, because I do not present you with this trifle, merely to gratify your curiosity, but that you, and some of your ingenious friends, may, by your remarks, help me to discover, to what inconveniencies our instrument is liable, how far they may be avoided, or lessened, or what the uses, or advantages, of it may be, notwithstanding its inevitable inconveniencies, or imperfections.

HAVING had occasion, amongst other subjects relating to the air, to consider its moisture, and its dryness, I easily discerned, that they had no small influence upon divers bodies, and, among the rest, upon those of men, as the ambient air we breathe in, either passes from one of those qualities to the other, or even from one degree to the other, in the same quality.

WHEREFORE, I began to cast about somewhat solicitously, for a way that might, better than any I had yet tried, or elsewhere met with, discover the changes of the air, as to moisture and dryness, and the degrees of either quality. For which purpose, it seemed to me, that if a statical hygroscope could be had, it would be very convenient, in regard of its fitness, both to determine the degrees of the moisture, or dryness of the air, and to transmit the observations made of them to others. Whereupon, considering further, that among bodies, otherwise well qualified for such a purpose, that was likeliest to give the sensibliest informations of the changes of the air, which, in respect of its bulk, had the most of its surface exposed thereunto; I quickly pitched upon a fine sponge, as that, which is easily portable, not easy to be divided or dissipated, which, by its readiness to soak in water, seemed likely to imbibe the aqueous particles, that it may meet with dispersed in the air, and which, by its great porousness throughout, has much more of superficies, in reference to its bulk, than any body, not otherwise less fit for the intended use, that came into my thoughts.

If you recal to mind, when, and whence I first gave you notice, that I employed our little instrument, you will easily believe, that the inducements I had to pitch upon it, were, that I should need but such light and parable things, as I could easily both procure in the country (where I then was) and carry about with me, in the frequent removes I was obliged to make; and therefore, that I did not present this trifle as the best hygroscope, that could be devised, or even as the best, that, perhaps, I myself could have propounded, if I would have framed an elaborate engine with wheels, springs, or equivalent weights, pallies, indices, and other contrivances, some of which I divers years ago made use of. For I little doubt, but that mechanical heads may frame hygroscopes much curiousest and perfectest, than that I now send you, or any other I have used, or seen, if they may be accommodated with sufficient room, and dextrous artificers, that will work exactly according to directions; whereas, my design being not so much to make a machinal, or engine-like, as a statical hygroscope, and such an one as may be simple, cheap, contained, and set up in a little room, easy to be made and transported, I thought it might be of some use, especially to those that are not furnished with curiosities and mechanical accommodations, if among the several forms of hygroscopes that I had in my mind, I chose one, that being statical and easy, might be as commodious by its simplicity, as some others by their elaborateness; especially if we consider, that, as slight an instrument as it seems, it may be applied to various uses, some of which are not slight, as will ere long be made probable.

If I should be here told by one that grants the preferableness of statical hygroscopes in the general, that there are divers bodies, other than that pitched upon by me, whose weight may vary, when the temperature of the air is considerably altered, as to dryness and moisture, and that, perhaps, among these, some one may be found, that may imbibe the aqueous particles of the air better than our sponge; I shall not resolutely deny it, and therefore shall leave you to make trials with what other bodies you shall think fit, contenting myself to have suggested, in general, the conveniency of making hygroscopes, where the differing changes of the air may be estimated by weight: but this I shall tell you, in favour of our sponge, that when I was considering what bodies were the fittest to be employed for the making of statical hygroscopes, I made trial of  
more



more than one, that seemed not the least promising. I know, that common, or sea-salt, will much relent in moist air, and salt of tartar will do it much more; but then those salts, especially the latter, will not so easily, as they should, part with the aqueous corpuscles they have once imbibed, and are in other regards (which it were not worth while to insist on) less convenient than a sponge. I made trial also with lute-strings, which were purposely chosen very slender, that they might have the greater surface, in respect of their bulk: these I found, at first, to do very well, as to the imbibing of the moisture of the air, but afterwards they did not continue to answer my expectation. I caused likewise to be turned out of a light wood a cup, which, that it might less burden a tender balance, had, instead of a foot, a little button, to which a hair might be tied, to suspend it by; and this cup being purposely turned very thin, that it might have much surface exposed to the air, proved for a pretty while so good a hygroscope, as invited me to make divers observations with it, some of which I have still by me. It agreed also with several trials, that I had made on other occasions, of the porousness of such bodies, that white sheeps leather, such as surgeons used to spread plaisters upon, would be very convenient for my purpose. And indeed I found by many observations, whose success you may command a sight of, that if this leather were a substance as little obnoxious to corruption as a sponge, it would, by its copious imbibitions, and emissions of the aerial moisture, be a fitter matter, than any other I had employed for a hygroscope.

BUT taking all things together, I found no body so convenient for my purpose as a sponge; which you will, perhaps, the more easily believe, if I add, that to help me to make some estimate of the porosity of it [we weighed out a drachm of fine sponge, and having suffered it to soak up what water it could, it was held in the air, not only whilst the weight of the water would easily make it run out, but till it dropped so very slowly, that a hundred was reckoned after one drop, before another fell; then putting it into the balance it had been weighed in before, we found that as its dimensions were increased to the eye, so its weight was increased upon the scale, amounting now to somewhat above two ounces and two drachms; so that one drachm of sponge, though it seemed not altogether so fine as the portion we had chosen out for our hygroscopes, did imbibe and retain seventeen-times its weight of water.]

Now when one is resolved to employ a sponge, there will not need to be much added about the turning it into a hygroscope. For, having weighed it, when the air is of a moderate temperature, it requires but to be put into one of the scales of a good balance, suspended on a gibbet, as they call it, or some other fixed and stable supporter. For the sponge being carefully counterpoised, at first, with a metalline weight (because that alters not sensibly with the changes of the air) it will, by its decrement, or increase of weight, shew how much the neighbouring air is grown dryer, or moister, in the place where the instrument is kept. The weight of the sponge may be greater, or less, according to the bigness and goodness of the balance, and the accurateness you desire in the discoveries it is to make you. For my part, though I have, for curiosity's sake, with very tender scales employed, for a good while, but half a drachm of sponge, and I found it to answer my expectation well enough; and though, when I used a bulk divers times as great, in a stronger, but proportionably less accurate balance, I found not the experiment successful: yet after trials with differing quantities of sponge, I preferred, both to a greater and lesser weight, that of a drachm, as not being heavy enough to overburden the finer sort of goldsmiths scales, and yet great enough to discover changes considerably minute, since they would turn, discernibly, with a sixteenth, or twentieth part, and manifestly, with half a quarter of a grain.

WITH

WITH such hygroscopes as these (wherein the balance ought to be still kept suspended and charged) I made several trials, as my removes and accommodations would permit, sometimes in the spring, and sometimes in the autumn, and sometimes also in the summer and winter. But, nevertheless, it would be very welcome to me, if you, and some of your friends, would be pleased to make trials yourselves, and compare them with mine, and especially take notice, if you can, whether, in any reasonable tract of time, there will be any loss, worth noting, of the substance of the sponge itself; I having not hitherto discovered any. In the mean time, to invite you to give yourselves this trouble, after I have told you, that having once, among divers removes, had the opportunity to keep a drachm of sponge suspended during a whole spring, and a great part of the preceding winter and subsequent summer, I did not think my pains lost, though divers of the observations they afforded me have unhappily been so, among many other memorials about experiments of differing kinds; notwithstanding which unseasonable loss, I shall venture to suggest some things to you, that occurred to me about the utilities of the instruments I am treating of.

## A

## B R I E F A C C O U N T

## O F T H E

## U T I L I T I E S O F H Y G R O S C O P E S .

**T**HE use of a hygroscope is either general, or particular; the former is almost coincident with the qualifications to be wished for, and aimed at, in the instrument itself; the latter points out the particular applications that may be made of it, when it is duly qualified. Of each of these, I shall briefly subjoin what readily occurs to me.

THE general use of a hygroscope is, “ To estimate the changes of the air, as to moisture, and dryness, by ways of measuring them, easy to be known, provided, and communicated.”

I MIGHT here pretend, that as these are the principal things that have been desired in hygroscopes, so it is obvious, from the description and account we have given of our instrument, that these advantages belong to it in no very despicable degree: and that to make such hygroscopes, as will perform all these things in perfection, whatever it may seem to a mental contriver, will, I fear, prove no easy task to those that really attempt it. To these things I might add, that if such allowances be made, as what I have represented may invite you to grant, the qualifications lately mentioned, as desirable in a hygroscope, may, in a tolerable measure, be found in ours, when we shall come to mention the particular uses of it. And as for that of conveying to others the observations made



made with it, you may please to consider, that the things I employ to measure the degrees of dryness and moisture in the air, being grains, parts of grains, and greater weights, the accession of moisture which the sponge receives, or the losses that it suffers, can be easily, and at the same time, both found and determined. And as the weights employed to determine these differences are easily procurable; so the observations made with them may (together with patterns, if it should be needful, of the weights themselves) with the same facility be communicated by letters even to remote parts. In which convenience, whether, and how far our instrument has the advantage of that made with an oaten beard, and some others that I have employed, I leave you to consider.

I MIGHT farther alledge, on the behalf of our instrument, that whereas, besides the qualifications above mentioned, there is another, namely, durableness, which, though not so necessary to constitute a hygroscope, yet is necessary, as will ere long appear, to some of the considerablest uses of it; and whereas such a durableness is wished, as may not only keep the instrument from having its substance rotted or corrupted by the air, but may also preserve it in a capacity to continue pretty uniformly its informations of the air's moisture, even when that increases very much, or lasts very long; whereas, I say, these things are much desired in a hygroscope, our sponge seems herein preferable to the oaten beard, lute-strings, &c. For in those and the like bodies, the self-contracting, or relaxing power (as it is supposed) or the disposition to imbibe, and part with the moisture of the air uniformly, or after a due manner, is wont to be in no very long time altered or impaired; and particularly, when they have imbibed much aerial moisture, they are very faintly affected by the supervening degrees of it, and so the operation is too disproportionate to what the like cause would have produced, when the instrument was well disposed; whereas, in our sponge, neither the degree of springiness, nor any such-like quality is considered, and it is capable of imbibing so much more of the aqueous particles, than even moist airs and seasons are wont to supply it with, that there is little fear that it will be glutted, or have its pores choaked up with them, so that the decrements and accessions of weight will be more proportionate to the degree of moisture in the air, and more reducible to known and determinate measures.

BUT though these, and the like specious things, may be represented in favour of our statical hygroscope; yet, to deal ingenuously with you, I much fear, that it will be very difficult to bring either statical ones, or perhaps any other, to be so complete, as to satisfy a nice and severe critick. And you would perhaps easily assent to my opinion, if it were not too tedious to entertain you with all the speculative doubts and scruples, as well mechanical as physical, which my accustomed diffidence has now and then suggested to me. But because such a sceptical discourse would be too tedious, and also somewhat improper, to be proposed by one that would recommend hygroscopes, I shall only now take notice of one great imperfection, which all that I have been acquainted with, are liable to; namely, that men have not yet found, nor perhaps so much as dreamed of seeking a standard of the dryness and moisture of the air, by relation to which hygrometers may at first be adjusted, and so be compared with one another, as we see many of those sealed thermoscopes that have been made and justified by Mr. *Scholgrave*, the dextrous operator of the Royal Society. I deny not, that, by virtue of a standard to estimate moisture by, I have endeavoured to remedy this inconvenience; but, as my hopes were but small, so neither was my success great, but I am not sure that happier wits, or I myself, at some other and luckier time, may not more prosperously attempt it. In the mean while, perchance, you will not think it altogether nothing, if the trifle I present you, perform, at least, some of the things desired in a hygrometer less imperfectly than any you have yet met with. And that you may not be discouraged by  
what

## Of the Utilities of Hygroscopes.

what I have lately acknowledged of the defects of such instruments, I think it now reasonable to proceed to the mention of the particular uses, for which, notwithstanding any inevitable defects, a hygroscope, and even such a one as I now present you, may be made easily to serve.

### U S E I.

*To know the differing variations of weather in the same month, day, and hour.*

It may be useful for divers purposes to know, both that the air is wont to be less moist at one part of the artificial day (and so of the night) than at any other, and at what particular time of the day or night it most usually is so. And on this occasion I remember, that usually, when the weather was at a stand, it was observed that the sponge had manifestly gained in the night, though it were kept in a bed-chamber, and grew lighter again between the morning and noon. This observation, which was made towards the end of winter, would not hold, in case frosty nights or some other powerful cause intervened. It were not amiss, also, to observe, whether there be not a correspondence betwixt the hygroscope and baroscope; and if there be, in what kind of weather or constitution of air it is most or least to be discerned. And this inquiry seems the more dubious, because the same changes of the atmosphere may, upon differing accounts, have either the like, or quite contrary operations upon these two instruments. For in summer, when the atmosphere is usually heavier, the hygroscope is usually lighter; some strong winds, as with us the north-west, may make both the atmosphere and baroscope lighter, whereas southerly winds, especially if accompanied with rain, often make the atmosphere lighter and the sponge heavier. And on the other side I observe, that easterly winds, especially when they begin to blow in winter, though, by reason of their dryness, they are wont to make the hygroscope lighter, yet they are wont, at least here, at the west end of *London*, to make the baroscope shew the air to be heavier. It were likewise fit to be observed, particularly by those that live on the sea-coast, whether the daily ebbing or flowing of the sea do not sensibly alter the weight of the hygroscope. It were very well worth while also to take notice, at what time of the day or night, *ceteris paribus*, the air is the most damp and moist dry, and not only in several parts of the same day, but in several days of the same month; especially on those days, wherein the full and new moons happen. And this seems a more hopeful way of discovering, whether the full moon diffuses a moisture in the air, than those vulgar traditions of the plumpness of oysters and shell-fish, and brains in the heads of some animals, and of marrow in their bones, and divers other phenomena, which, as I have shewn in another paper, it is not easy to be sure of. It may also be noted, whether monthly spring-tides, especially when they fall out near the middle of *March* or *September*, have any sensible operation upon our instrument.

### U S E II.

*To know how much one year and season is dryer or moisture than another.*

THIS cannot be so well performed by the hygroscope made of an oaten beard, if they that have made use of them more than I, do complain with reason, that after some months



months (for I cannot tell you precisely how many) they begin to dry up and shrink; so that their sense of the varying degrees of the moisture of the air is not so quick as before, and the informations they give of the degrees of it, especially towards the utmost bounds of their power, to shew the air's alterations, recede more and more from uniformity. But the lastingness and other convenient qualifications of our sponge making its capacity of doing service more durable, may the better help us to compare the greatest moisture and dryness, both of the same season, and of the seasons of one year with the correspondent ones of another. And if the weight of the sponge at a convenient time, when the temperature of the air is neither considerably moist, nor considerably dry, be taken for a standard, a person, that should think it worth his pains, may, by computing how many days at such an hour, and how much at that hour, it was heavier or lighter than the standard, and also by comparing the result of such an account in one year with the result of the like account in another year, be assisted to make a more particular and near estimate of the differing temperature of the air, as to moisture and dryness, in one year than in another, and in any correspondent season or month, assigned in each of the two years proposed. And how much the collation or continuance of such observations, both in the same place, and also in differing countries and climates, may be of use to physicians in reference to those diseases, where the moisture and dryness of the air has much interest; and the husbandman to foresee what seasons will prove friendly or unkind to such and such soils and vegetables; it must be the work of time to teach us, though in the mean while we have no reason to despair, that the uses to be made of such observations may prove considerable. And the rather, because if by help of the result of many observations men be enabled to foresee (though at no great distance off) the temperature of a year, or even of a season, it may advantage not only physicians and ploughmen, but other professions of men, who receive much profit or prejudice by the dryness or excessive moisture of the seasons. And not to mention those who cultivate hops, saffron, and other plants that are tender and bear a great price; such a foresight, as we are speaking of, may be of great use to shepherds, who, in divers parts of *England*, are sometimes much damnified, if not quite undone, by the rot of sheep, which usually happens through excess of moisture in certain months of the year. And in order to the providing of foundations whereupon to build predictions, it may not be amiss to register the number, bigness, and duration of the considerabler spots that may at this or that time of the year happen to appear, or be dissipated, on or near the sun, or to take notice of any extraordinary absence of them, and to observe whether their apparition or dissipation produce any changes in the hygroscope; which curiosity I should not venture to propose, but that (as I elsewhere note) I find, that eminent astronomers have casually observed great drynesses to attend the extraordinary absence or fewness of the solar spots. And those persons that are astrologically given, may, if they please, extend their curiosity in the use of this instrument, to observe whether eclipses of the sun and moon, and the great conjunctions of the superior planets, have any notable operation upon it.

### U S E III.

*To discover and compare the changes of the temperature of the air made by winds, strong or weak; frosty, snowy, and other weather.*

THIS may conveniently enough be done as to winds, either by our whole instruments, or (perhaps better and more safely) by the sponge alone, which may be taken off and

hung by a string, for as long time as is thought fit, in the wind, and then restored to its former place. For I found, by removing it into the wind, that it soon received a very considerable alteration in point of weight, as also it did, when removed out of a room into a garden where the sun shined; for though the season were not warm, it being then the month of *January*; yet in three quarters of an hour the sponge lost the 24th part of its weight. We may also in some cases usefully substitute to a sponge a somewhat broad piece of good sheeps-leather displayed to the wind. For this having, by reason of its thinness (or very small depth) in proportion to its breadth, a very large superficies immediately exposed to the wind, we found it to be notably altered thereby, inasmuch, that half an ounce of well prepared sheeps-leather (that we had long employed as an hygroscope) being kept an hour in a place, where the sun-beams might not beat upon it, did, in a strong wind, vary in that short time an eighteenth part of its original weight. But though I think it very possible to make such observations of the temperature of particular winds, as will frequently enough prove so true as to be useful, at least to those that live in the places where they are made; yet I am of opinion, that, to be able to settle rules any thing general, to determine with any certainty the qualities of winds, according to the corners whence they blow, as from the east or west, north-east, south-west, &c. there will be a great deal of wariness required; and he that has not some competent skill in physics and cosmography, will easily be subject to mistakes in forming his rules. To countenance which advertisement, I shall now make use but of these two considerations, whereof the first is; that winds that blow from the same quarter, are not in some countries of the same quality that they are in most others, the wind participating much of the nature of the region over which it blows in its passage to us. At the famous port of *Archangel* they observe, that whereas a northerly wind, almost every where else without the tropicks, produces frost in winter, there it is wont to be attended with a thaw, so as to make the eves to drop. Of which the reason seems to be, that this wind comes over the sea, which lies north from that place; and on the contrary, a southerly wind, blowing over a thousand or twelve hundred miles of frozen land, does rather increase the frost than bring a thaw. This was by the inhabitants averred to the Russian emperor's physician, who was more than once at *Archangel*, and from whom I had the account. The northern winds, that are elsewhere wont to be drying, are said in *Egypt* to be moist. I remember Mr. *Sandys*, in his excellent Travels, giving an account of what he observed about the largest of the famed Egyptian pyramids, has this considerable passage; "Yet this hath been too great a morsel for time to devour, having stood, as may be probably conjectured, about three thousand and two hundred years, and now rather old than ruinous: yet the north side most worn, by reason of the humidity of the northern wind, which here is the moistest." *Sandys* in *Purchas's Pilgrimage*. And it is yet more considerable to our purpose what I find related by Monsieur *De Serres*, in his useful book of husbandry, since by that it appears, that even in not very distant provinces of the same kingdom, the winds, that blow from the same quarter, may have very differing qualities and effects. For, speaking of the changes of the air, in reference to husbandry, in several parts of *France*, he informs us, that it is observed, that in the quarters about *Toulouse* the south wind dries the ground, and the north gives rains. Whereas on the contrary, from *Narbonne* to *Lyons*, all over *Provence* and *Dauphiné*, this last named wind causes dryness, and the other brings moisture. And this may suffice for my first consideration. My second is this, that the vehemence or the faintness of the winds, though blowing over the same country, may much diversify its operation on the hygroscope; and the same wind, which, when it blows but faintly, or even moderately, is wont to appear moist by the hygroscope, may, when

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ture. lib. I.  
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vehement or impetuous, make the instrument grow lighter, discussing and driving away more vapours by the agitation of parts it makes in the sponge, than is countervailed by those aqueous vapours that are brought along with it. But on such things as these I have not leisure to insist, and therefore I shall proceed to take notice, in very few words, of some other operations of differing weathers on our instrument, and tell you, that frosty weather often made the hygroscope grow lighter even at night; snowy weather, which lasted not long, added something to the weight of the sponge. And it has been observed, that mists and foggy weather used to add weight to it, even notwithstanding frost.

To which may be added an observation made by my amanuensis, who having a convenient chamber than mine (wherein a fire was daily made) was diligent and curious to set down the changes of the hygroscope that was left in his lodging; for this observation makes it probable that a transient cloud in fair weather may be (for I say not that it always is) manifestly observable by our instrument. For, by his diary, it appears that the ninth of *September*, being for the most part a very fair sun-shiny day, though about ten a clock in the morning the sun shone brightly, the sponge began to preponderate, which unexpected phenomenon made him look out at the window, where he discovered a cloud that darkened the sun, but after a while, that being past, the balance returned to an equilibrium. On this occasion I shall intimate, that I have more than once or twice observed, especially in summer, that when the air grew heavier, the hygroscope either continued at a stand, or perhaps, also grew lighter; as if, when such cases happen, the effluvia that get into the air, either from the terrestrial or some other mundane globe, were not fit, like vapours, to enter and lodge in the pores of the sponge, and so were corpuscles of another nature, with which, when we find by the baroscope that the air is plentifully stocked, it may be worth while to observe, whether any, and if any, what kind of meteor, as wind, or rain itself, or hail, or in the winter snow or frost, will commonly be signified and produced.

#### U S E IV.

*To compare the temperature of differing houses, and differing rooms in the same house.*

As this is of great use, both in respect of men's health, especially if they be of a tender, or sickly constitution, and in respect of conveniency for the keeping flesh, sweet-meats, and several sorts of wares and goods, and even household-stuff, that are subject to be indamaged by moist air; so it is readily and manifestly derivable from our instrument. For, by removing it into several houses, or into several parts of the same house, and letting it stand in each a competent time, to be affected with the temperature of the air of that particular place, we have divers times observed a notable difference, as you may guess by the two or three notes I met with among some old papers.

*October 13.* [THREE or four days ago, a piece of fine sponge being taken out of a cabinet, and clipped, till it came to weigh just half a drachm in a nice pair of scales, and a warm room, was afterwards removed into a neighbouring room destitute of a chimney (and yet within three or four yards of a chimney seldom without fire); this statical hygroscope, consisting of the scales and the frame they hung on, was yesterday night removed into the former room, and the sponge was found to have gained three grains and  $\frac{1}{4}$ , or better, and consequently more than a tenth part, in reference to its first weight; but being suffered to stand in this warm room, in less than twelve hours it lost a grain

and about  $\frac{1}{8}$  of its former weight, though the time it stood in this room were, for the most part, night and rainy weather.]

[We took a piece of very fine sponge, which formerly had weighed just a drachm, but having been many months kept in a very warm room, where fires were kept every day, it was grown much lighter; for, removing it into an upper chamber in a neighbouring house, and weighing it in tender scales, in the evening it was found to want of a drachm, four grains, and  $\frac{1}{2}$  of a grain; and though there was a fire in the room, and the scales stood not far from it, yet, in a short time (the day being foggy and rainy) the sponge visibly depressed its scale  $\frac{3}{4}$ , and the next morning was found to want but one grain and a half of a drachm, so that it had gained about three grains and a quarter, and the following evening, being the second of *January*, it weighed one drachm, a grain, and almost half a grain. So that in about one natural day the sponge had acquired six grains from the moisture of the air, that is, a tenth part of its first weight (I mean a drachm) and a greater proportion in reference to the weight it had the day before. The third of *January*, the weather being yet moist, the weight exceeded two grains, but about three or four of the clock in the afternoon it began to lose of that great weight, which diminished more by the next morning, the weather having changed that night, and become somewhat frosty.]

In another paper I also find this note. [The drachm of a sponge, that had for divers weeks been kept in a dry room, was (*January* the tenth) carried out into a room, where fire is not wont to be kept, the weather being extraordinarily foggy; this morning, being brought into the former room, though now the weather be clear (yet not frosty) it appears to have gained in weight about eleven grains; yet it soon lost two grains by standing in this room all the while in the balance.]

#### U S E V.

*To observe in a chamber the effects of the presence or absence of a fire in a chimney or stove.*

THIS is easily done, and the more easily, if the room be small. For in such chambers, I have often observed a moderate fire to alter the weight of the instrument placed at a distance from it, after it had been well kindled but a very little while; but in wet weather, if the fire were not seasonably renewed with fresh fuel, the decay of it would, in no long time, begin to be discernible by the instrument.

#### U S E VI.

*To keep a chamber at the same degree, or at an assigned degree of dryness.*

SUPPOSING the alteration of weight in our sponge to depend only upon the degree of the moisture of the air, the last named use will be but an obvious corollary of the former. For, if a convenient part of the room be chosen for the hygroscope, and it be kept constantly there, it is easy, by casting one's eye on it from time to time, to perceive when it will be requisite to increase or moderate the fire, so as to keep the sponge at that weight it was of, when the temperature of the air of the chamber, as to dryness and moisture, was such as was desired. I will not trouble you with some scruples, which, I confess, the



the consideration of this use of our instrument suggested to me, because I have not now the leisure to discuss them. I had thoughts to try, whether, and how far a good quantity of salt of tartar, or even dried sea-salt, being kept in a closet or some closer room, might, by imbibing, lessen the moisture of the air in it, but I did not perfect any observation of this kind. But I will add to what I have already referred to this sixth head, that I have sometimes noted with pleasure, how manifest and great a change in the weight of our sponge would be made when the room was washed, and a good while after, notwithstanding that a good fire was kept in it, to hasten the drying of it.

BESIDES the hitherto mentioned uses of our hygroscope, I know not, whether there may not be divers others, and whether we may not, by a little altering and helping it, make it capable of shewing us some difference betwixt steams of differing natures, as those of water, spirit of wine, chymical oils, and perhaps new kinds of substances (such as we have not yet taken notice of) in the air, in which I confess, I suspect there may sometimes be dispersed store of corpuscles, that I do not yet well know what to think of. For I have more than once observed (not without some wonder) the hygroscope not to be affected with the alteration of weather, answerably to what the manifest constitutions or variations of it seem plainly to require; whether unobserved corpuscles performed this, by making the other steams in point of figure, or size, incongruous to the minute pores of the sponge, and so unfit to enter them; or by dissipating, or otherwise procuring the avolation of more of the watery particles than they could countervail, I now examine not. And I am not sure, but by associating this instrument with the thermoscope, baroscope, and some others that may be proposed, it might be so improved, as to help us to foresee divers considerable things, that either are themselves changes of the air, or are wont to be consequences of them; as sickly and healthful constitutions of the air, both as to man and cattle; and healthful, barren, or plentiful seasons in particular places or countries; and perhaps also strong hurricanes, earthquakes, inundations, and their ill effects, especially those accidents that depend much upon the surcharge of the air, with other exhalations and moist vapours, which operate before sensibly upon our instrument, and therefore may be discernible by it a good while before they arrive at that height, that makes them formidable meteors. And if it were but the foretelling approaching rain, this very thing may, on divers occasions, prove very serviceable, and recommend our instrument, which often receives much earlier impressions from the steams that swim up and down in the air, than our senses do; so that I have been able to foresee a shower of rain, especially in dry weather, a not inconsiderable while before it fell.

AND here I should dismiss our subject, which I have already dwelt on longer than I designed, but that remembering a caution I gave you, when I was speaking of winds, *See Use* I think it but fit to add two or three lines, to keep you from being by that advertisement *III.* discouraged from endeavouring to make, in the general, such hygroscopical observations, as may be reduced to hypotheses. For, as I elsewhere discoursed concerning barometrical theories, if I may so call them; so I shall here represent, concerning hygroscopical ones, that if a theory or hypothesis that is itself rational, be found agreeable to what happens the most usually in observation; it ought not lightly to be rejected, or so much as laid aside, though sometimes we find particular instances that seem to call it in question. For it is very possible that the theory or hypothesis may be as good as a wise man would require about so mutable a subject as the weather. And the cause assigned by the hypothesis may really act suitably to what that requires, though a contrary effect ensue by reason of that cause's being accidentally mastered and over-ruled by some more powerful cause or agent, that happens for that time to invade the air. As we know that tides do, for the main, correspond with the motions of the moon (whose phases are therefore argued  
from

from them) and do generally ebb and flow at such times, and in such measures, as the theory that has been grounded on that correspondency requires; but yet seamen find that in this or that particular harbour, or mouth of a river, fierce contrary winds, great land-floods, and other casually intervening causes, do sometimes both very much disturb the regular course of the tides, and encrease or lessen them.

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A  
 NEW EXPERIMENT  
 AND  
 OTHER INSTANCES  
 OF THE  
 EFFICACY OF THE AIR'S MOISTURE.

Subjoined by Way of

APPENDIX to his STATICAL HYGROSCOPE.

SINCE it may probably recommend hygrosopes to you, if that quality of the air, which these instruments are useful to give us an account of, be made appear to be more powerful, and have considerabler effects than is commonly believed; it will not be from my purpose to present you here some instances, that have led me to think that the effects of the moisture of the air may be considerable, not only upon men's healths, but upon subjects far less tender, and less curiously contrived than human bodies. But I hope you will easily believe, that by the moisture of the air, I mean not a mere and abstracted quality, but moist air itself, or rather those humid corpuscles (chiefly of an aqueous nature) that abound, and rove to and fro in our common air.

THAT the moisture of the air may have no small influence, and usually a bad one, upon men's healths, is that, which, though experience did not so often teach us, I should venture to argue from what I have observed of the operation of moist air upon the dry and firmly context parts of animal, and even in those cases, where, for want of time or other impediments, this moisture cannot produce any sensible degree of putrefaction.

THAT the skins of animals may be easily invaded by the moist particles of the air, is the more probable, because of the numerousness of their pores, which may be concluded from



from their hairiness, or their sweat, or both. And I formerly observed to you, that I found sheeps leather to imbibe the moisture of the air, and increase in weight upon it, as plentifully as almost any body I exposed to it.

BUT to shew you, that much closer membranes, and which nature made to be impervious to such a liquor as urine itself, may be affected by the vapours of the air, I shall add, that having purposely taken pieces of bladders, fine and well blown, and, as far as appeared, of a very close texture, and counterpoised them in a good balance, I found, according to expectation, that they would considerably increase their weight in moist, and lose it again in dry weather; so that I might have employed the most membranous part of a bladder (for I thought not fit to make use of the neck or the adjoining part) to make a statical hygroscope.

AND as for other membranes and fibres, I shall have by and by occasion to take notice, that even when they are strongly and artificially wreathed together into gut-strings, they may imbibe enough of the moisture of the air to be broken by it. And I remember, I formerly told you, that I had observed lute-strings to grow heavier in moist air.

AND whereas bones are by all confessed to be the firmest and solidest parts of animals, and, as it were, the pillars by which the fabrick is sustained; yet it seems, that even they may be pierced into, and sensibly affected by the moisture of the air. For I remember, that having caused the skeleton of a human body to be so made by a famous and very skilful artist, that, by the help only of slender wires artificially ordered, the motions which the muscles make of the bones of a living body, might be well imitated in the skeleton, I observed, that though in dry and fair weather the flexures of the limbs might be readily made, yet in very moist weather the joints were not easily bent, as if the parts were grown stiff and rigid; which seemed to proceed hence, that moist particles of the air, having plentifully insinuated themselves at the pores into the bones, had every way distended them, and thereby made the parts bear hard against one another (which they did not at all before) at the junctures or articulations.

BUT it will be the more readily believed that the moisture of the air may operate considerably upon the tender and curiously contrived bodies of men and other animals, if, proceeding to the observations I chiefly design, I make it appear that the moistening particles that rove up and down in the air, are able to exercise a notable (and, if I may so call it, a mechanical) force, even upon inanimate and inorganic bodies; which may well suggest a suspicion, that hygroscopes being the proper instruments to discover a quality in the air, whose efficacy reaches farther than is commonly taken notice of, they may in time be found useful to divers other purposes, besides those that relate to the health of men.

THAT wood, especially when it has been seasoned, is a solid of a strong and firm texture, if it were not obvious by the daily use made of it in building ships, houses, &c. might be easily concluded from the weight or force required to alter its texture, by making any considerable, or, perhaps, sensible compression of it. And yet that wood may suffer a kind of divulsion of the multitude of its parts, and be manifestly distended by aqueous corpuscles getting into its pores, I remember, I proved by this experiment. I got a piece of sound and seasoned wood of about an inch (or an inch and half) in diameter, to be by a skilful artist made cylindrical, and also a ring of some solid matter, as brass or ivory, to be exactly turned to fit this cylinder, so that it might, without much ease, or much difficulty, be put on and taken off again; then we put the turned piece of wood into fair water, and left it to soak there for many hours; at the end of which it was visibly swelled; and though I cannot now tell you (for want of a paper concerning that experiment) how much it was increased in diameter, yet I well remember, the

the increment was considerable, and that the ring that was adjusted to it before, was manifestly too little to be put again upon it, or with its orifice to cover the whole basis of the distended cylinder, which afterwards being dried in the air shrank into a capacity of entering the ring again. And in this experiment I took notice, that the great intumescence of the wood was not produced all at once, or soon after it was put into water, but it swelled by degrees, and lay soaking there many hours before it arrived at its utmost distention, the aqueous corpuscles requiring, it seems, so much time to insinuate themselves sufficiently into the wood; which argues that the internal parts were likewise affected, though, when even they came to swell, they had a good thickness of wood about them to hinder their dilatation.

I EXPECT you should now tell me that this distention of so firm a body was made by water itself, and not by the humid vapours of the air. On which occasion I might represent to you, that by the sweating (as men commonly call the adhesion of waterish drops to the surface) of polished marble and some other cold and smooth bodies that sometimes happens even in the heat of summer, if they be cold, and the ambient air be moist enough, it appears, that both in hot weather the air may be plentifully stocked with aqueous vapours, and that these vapours need to do no more than convene together, to constitute visible and tangible water. And on this occasion, if I were sure I had not told you of it already, I should subjoin an experiment, which would detect the vulgar error of those that think the adhering drops, lately mentioned, to come from internal moisture derived by its pressure or percolation from the marble or the other body they are fastened to; and at the same time I shall shew (what is not wont to be imagined) that in the heat of summer the air is furnished with invisible and yet aqueous steams. The experiment I long since tried in winter with snow and salt included in a glass vessel, and then put to dissolve in a balance. But because neither ice nor snow is at all easy to be come by among us in *England* in summer; and because, at that season, the air in fair weather is presumed to be dry as well as hot, I chose, within some days of Midsummer, and in clear sun-shiny weather, to make the following trial.

WE took a pint glass bottle, and having put into it a convenient quantity of water (for room must be left for the salt) we placed them and four ounces of beaten sal armoniac in one scale of a good balance, and a counterpoise in the other; and then, putting the salt into the water, I observed, that though for a while the æquilibrium remained, yet when the frigorifick mixture had sufficiently cooled the outside of the bottle, the roving vapours of the air that chanced to pass along the surface of the vessel, were, by the contact of that cold body, arrested, and turned into a kind of a dew, which from time to time made the scale that held the glass preponderate more and more, and at length the drops growing greater and greater, ran down in small rivulets the sides of the glass, and in less than an hour, by my estimate, the condensed steams amounted to near a drachm, which weight was afterwards much increased within about two hours more; whereby it sufficiently appears, both that this dew came from without (since if it had been a transudation, it would not have added weight to the scale that received it) and that there is, even in clear summer weather, a vast number of moist particles dispersed through the air, since in about an hour's time, such a multitude of them, as the liquor produced, may be supposed to consist of, and may by heat be actually resolved into, could in course come to touch so small a surface as that of that part of so small a bottle, which contained the frigorifick mixture. For the rest of the vessel's surface was not cold enough to condense the vapours into liquor. But to return to what we were saying of wood swelled by water; because, notwithstanding these considerations, I am willing to allow that the experiment of the cylinder does not fully come home to our purpose, and



when I produced it not so much to prove directly the force of moist air, as to counterbalance what I am about to say, by shewing what a sufficient number of aqueous corpuscles may do in the solid wood they penetrate; I shall now add some instances of the force these particles may exercise upon solids, when they invade them but in the form of vapours.

THAT in this form the multitude, figures, and motions of these insinuating particles may enable them to display no small force in their operations on some bodies, we have one instance, that often happens, though but seldom reflected on, in the breaking of the strings of musical instruments first brought to a good tension, upon the supervening of rainy weather. For the cause seems to be, that the vapours that then wander through the air, insinuating themselves into these strings (which the musician often forgers to let down or relax after having skrewed them up) distend and swell them, and thereby endeavour to shorten them, and that so forcibly, that they not seldom break with a smart noise and great violence; which, because it happens without any visible efficient, men commonly think and say that such strings break of themselves. But to take no farther notice of this popular surmise, if we consider how much weight some of those bigger strings, especially of base viols, that have been observed to break in rainy weather, will require to stretch any of them to a rupture, you will easily be induced to think that this operation of the moist air exacts, and therefore argues more than a languid force.

BUT here, probably, you will tell me, that the instances you expected were concerning wood, which is a far solidier body than gut-strings. To this I say, that the newly-recited instance belongs directly to the title of this paper; and being above referred to, ought not to be pretermitted. And as to your expecting instances concerning wood, I might content myself to refer you to what is observed about the uneasy opening and shutting some doors, well adjusted to the door-case, in very rainy weather. But though this observation favours my design, yet I had rather give you instances in wood, purposely and carefully seasoned. And therefore I shall now inform you of these two things; one, that I found by trial, as I have elsewhere noted, that wood, counterpoised in a good balance, would grow sensibly heavier in wet weather, and lighter again in dry; and the other, that, to satisfy myself yet further, I consulted an ancient musician to whom I had once been a disciple, and a famous organ-maker, to know, whether they had not observed, that the wood itself, &c. of musical instruments, would receive such alterations from the moisture of the air, as might be discerned by the ear? upon which inquiries, the master of musick answered me, that though metalline strings will not change with the weather like gut-strings; yet virginals (for instance) though furnished with wire-strings, will, for the most part of them (for some he has observed to be so well seasoned that they are not altered by the weather) be out of tune in wet weather, the strings generally then affording their notes sharper than they should, or are wont to do. And the organ-maker confessed to me, that, upon great changes of weather, divers organs would (after they had been long ago tuned) grow out of tune, and that not only the wooden pipes would be thereby swelled, but the metalline pipes untuned.

BUT if bodies be of such a constitution, as not only to admit but assist the operation of the moist air, the penetrancy and efficacy of this may be found much more considerable than in the foregoing instances. For there are some kinds of those marshalites that yield vitriol, which, whilst they lie under ground, or are covered with the sea-water, on whose shores they are, in some places, to be found, retain a stone-like hardness, and are often taken for mere stones; and yet some credible persons that are conversant about vitriol have casually observed, that these, being exposed to the air, would, in tract of time, be so penetrated by the moist particles of it, though perhaps not merely as moist, that

(probably by the help of the vitriolate corpuscles they met with among the stony matter) these hard and solid marchasites are brought to swell so much as to burst. That this will happen to such kind of stones (though they be of a close and heavy nature) by the help of rain, experience has persuaded me; and that it may also happen even to very hard and stone-like marchasites (for many are not such) when they are merely exposed to the air, I am apt to think, upon some trials of my own. For, from shining marchasites, though but kept in my chamber-window, I have had vitriolate efflorescences that seemed to be produced by the action of the piercing moisture of the air upon the mineral. And I remember, that very hard and heavy lumps that were of a marchasitical substance, though not at all glistening, which seemed to be stony, were so disposed to be wrought on by the air, that though they were kept partly in my own chamber, and partly in other covered places, yet in no very long time they were so penetrated by the moist corpuscles of the air, that they were not only burst, but broken into many pieces; inasmuch that many of them did of themselves fall off from one another, and several of the divided portions would easily be crumbled betwixt one's fingers. And of some of these I have observed with pleasure, that a vitriolate substance was produced more copiously in their innermost parts, than on or near their outside. So that, when I considered how great an external force would have been requisite to make such a comminution of minerals so solid and hard, it was obvious for me to look upon the air's moisture, as capable, when it meets with fitly-disposed bodies, to exercise a far greater force than is wont to be conceived.

To these phenomena I might add some others to the same purpose; but because the marchasites and other bodies required to the producing of them, are not easy to be come by, and the success often exacts a good length of time, I shall conclude this paper, by subjoining a far shorter experiment, that I devised not only to shew, in general, that the moisture of the air may have a considerable efficacy, but to assist a virtuoso to make some estimate in known measures of the mechanical force of the aerial moisture. And though I now find, to my trouble, that I want some of the notes that concern the circumstances, and the progress of the trial, yet enough having escaped to furnish me with the following account of it, what I shall set down, may, I hope, at least put you in the way of repairing my misfortune.

THINKING it then probable that ropes themselves would considerably imbibe and diminish the moisture of the air, and that so, as to shrink in rainy weather, though clogged with a weight fastened at the lower end, I was discouraged from attempting the following trial, by considering that the weight would stretch the rope, and consequently hinder the presumed effect of the air's moisture to be perceived. For I supposed, that after a time, this unusual stretch of the rope would cease; and when the weight, as such, could not lengthen it any more, it would then be capable of being contracted or relaxed, according as the weather should be moist or dry, and so afford me a kind of hygroscope. Upon these grounds I first caused a rope that was about twenty or twenty-two yards in length, but of no great thickness, to have one of its ends fastened to an immoveable body at a convenient height from the ground, and then caused a pulley to be so fastened to another stable body, at the distance of eighteen or twenty yards from the first, that the rope, resting upon the pulley, lay almost horizontally. But to the end of that part of the rope, which from the pulley reached within two or three feet of the ground, was fastened, by a ring, a leaden weight of at least fifty pounds. To which was also fastened a light index placed horizontally, whose end moved along an erected board, which, by transverse lines, was divided into inches and parts of inches, reaching both a good way upwards and down-



downwards, that the index might, within those bounds, have room to play up and down, according to the alterations of the weather.

It being then summer, this trial was made in a garden, though partly under a pent-house; that the rope might be more exposed to the air, than it would have been within doors; and two or three days, if I misremember not the time, were spent before the weight had brought the rope to the utmost stretch it was able to give it, after which it began manifestly to shrink, and lengthen according to the weather. And I find, in one of my notes, that once I looked, when I was ready to go to bed, upon the suspended weight, and marked how low it reached upon the divided board; and that a great part of the night having been rainy, looking again about an half an hour after eight in the morning, I found the cord so shrunk that the weight was raised above five inches, and yet the day growing dry and windy, and sometimes warm, the weight had at night stretched the rope more than the moisture had contracted it the day before.

AFTERWARDS having procured a far greater weight, but therefore unapt to be near so much raised, I substituted it in the place of that formerly mentioned; and having suffered it to stretch the rope as far as it could, I made and registered some observations, two whereof having been preserved, I shall transcribe them just as I find them.

*June* the 4th. At half an hour after nine of the clock at night, I looked upon the hundred pounds weight that hung at the bottom of the rope, the weather being then fair, and a mark being put at that part of the erected board where the bottom of the weight touched; I perceived the sky a while after to grow cloudy and overcast, but without rain; wherefore, going to view the weight again, I found it to be risen a quarter of an inch, or more, and looking on my watch, perceived there had passed an hour and quarter since the mark was made.

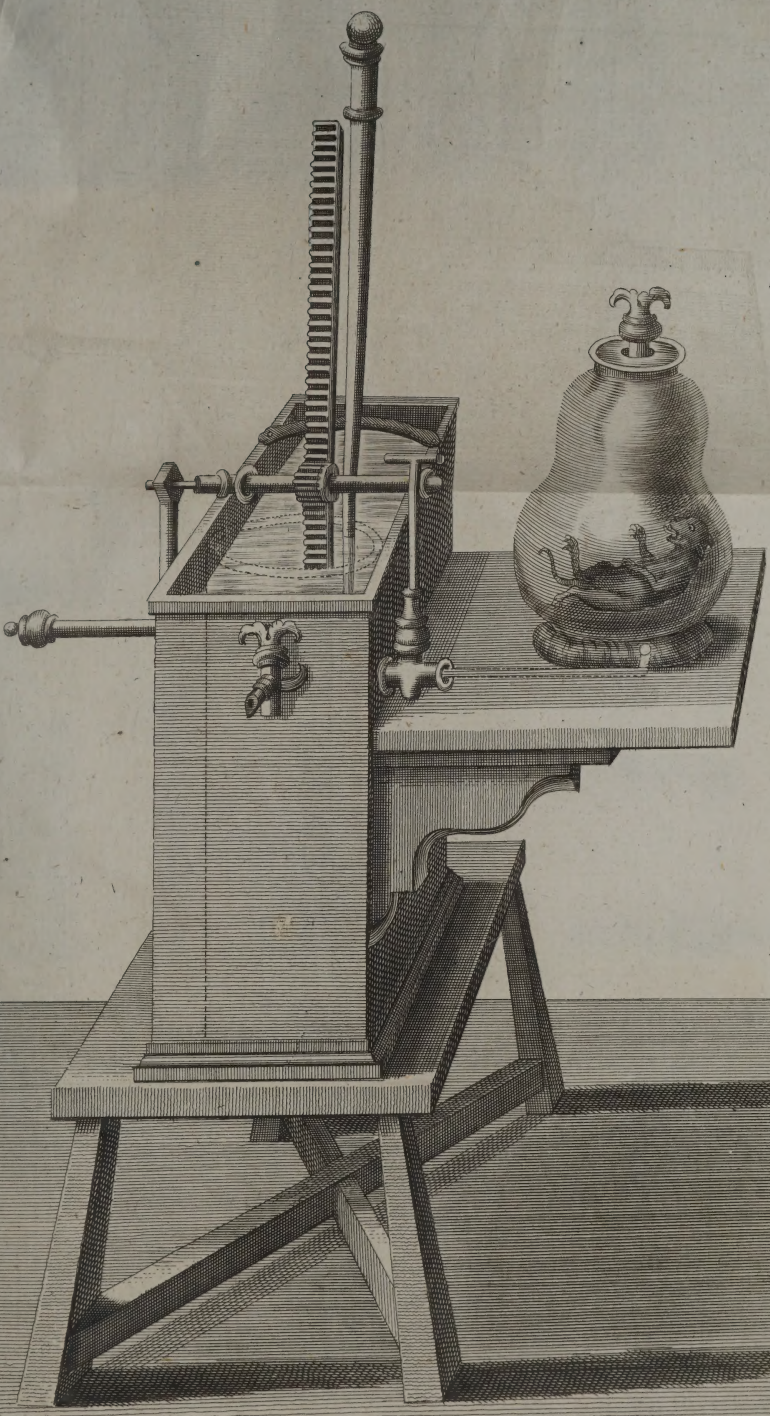
*June* the 6th. BEING not well yesterday, the weight was observed by two of my servants, and it then rested at the eleventh inch of the erected board. This morning, about eight of clock I visited it myself, and found it to be risen about half a quarter of an inch above the eighth inch, the morning being cloudy, though the ground very dry and dusty. The weather being more overcast, within somewhat less than an hour afterwards, I visited the weight again (some scattered drops of rain then beginning to fall) and found it to be risen about half an inch above the newly-mentioned eighth mark. How much more the rope would have been contracted in such lasting moist weather as usually happens in winter, I cannot say, having been reduced to break off the experiment, upon a removal I was, long before that season, obliged to make.

I AM sorry I cannot add my other observations; but these I hope may suffice to let you see that the force of the air's moisture is not small, since it could raise such a weight as an hundred pounds, especially considering the slenderness of the rope it affected. For, having measured the diameter near the weight, I found it (as one of my notes informs me) to be but about the  $\frac{1}{3}$  third part of an inch.

† It was  $\frac{3}{10}$  and 4 decimal parts of  $\frac{1}{10}$











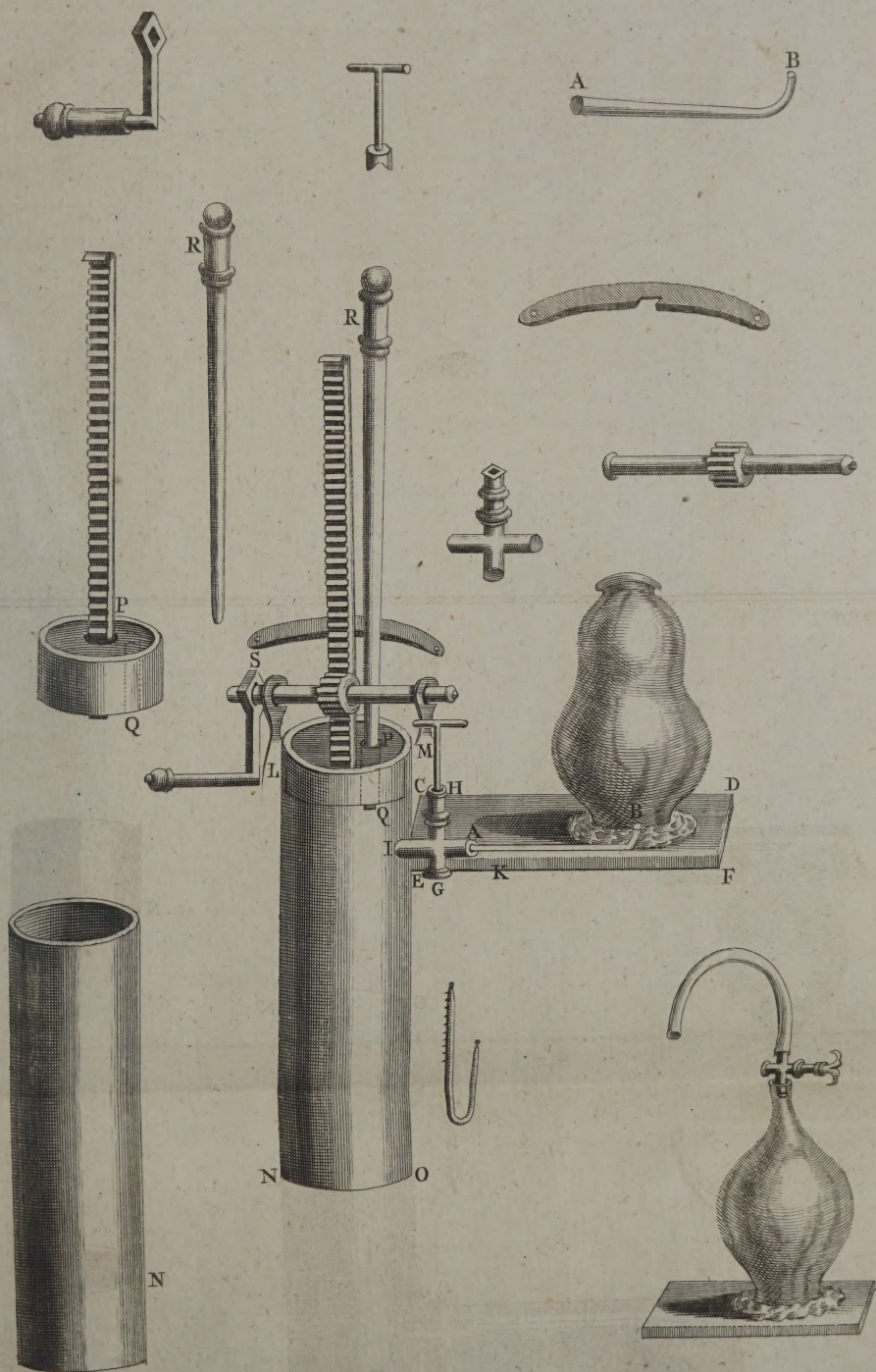






Fig. 1.  
p. 184.

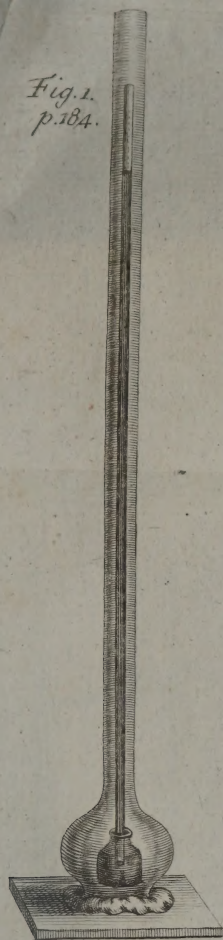


Fig. 3.  
p. 201.

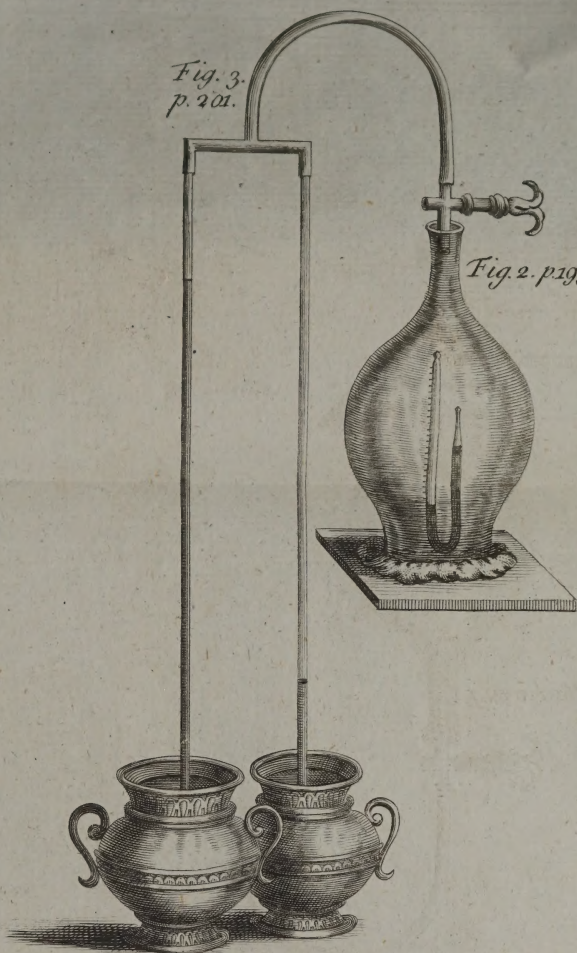


Fig. 2. p. 199, 200.

Fig. 1.  
p. 190.

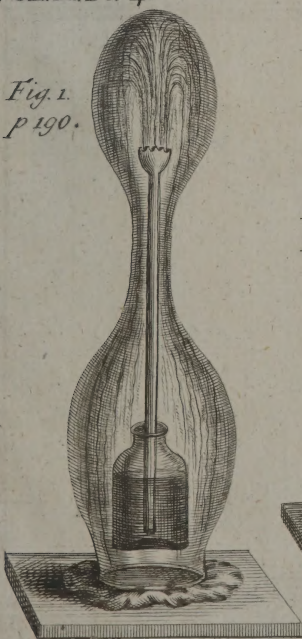


Fig. 2.  
p. 190.



Fig. 3.  
p. 265

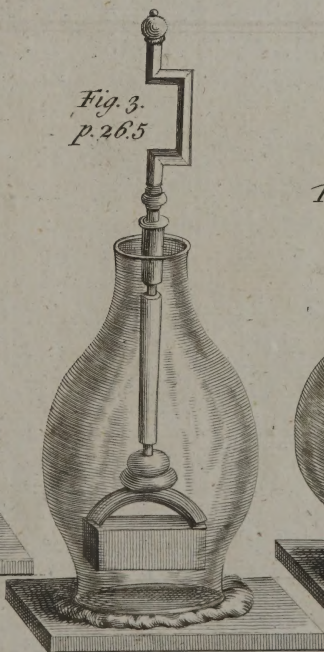
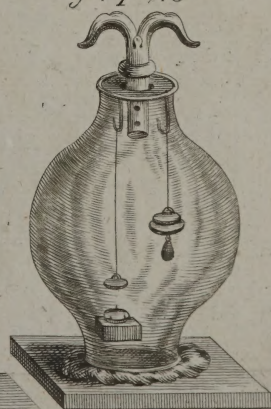
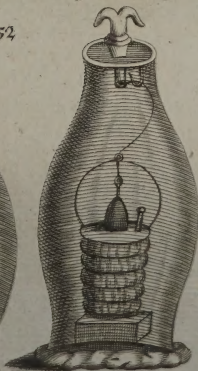
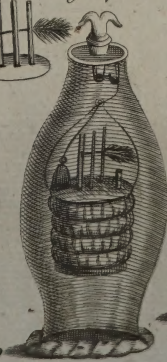
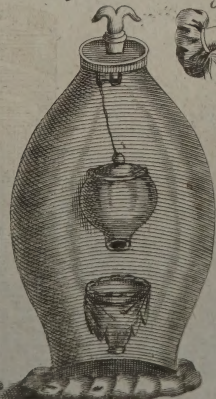
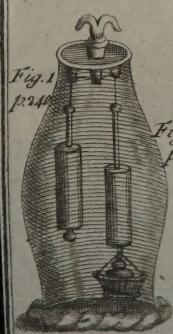
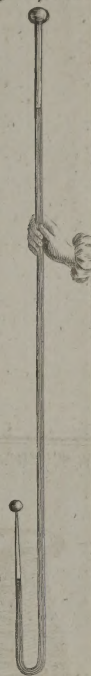
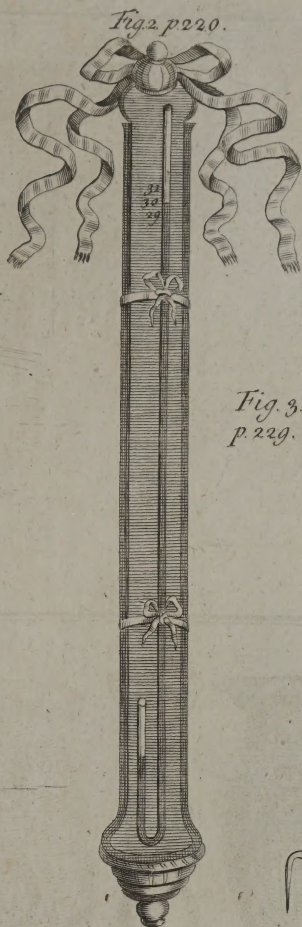
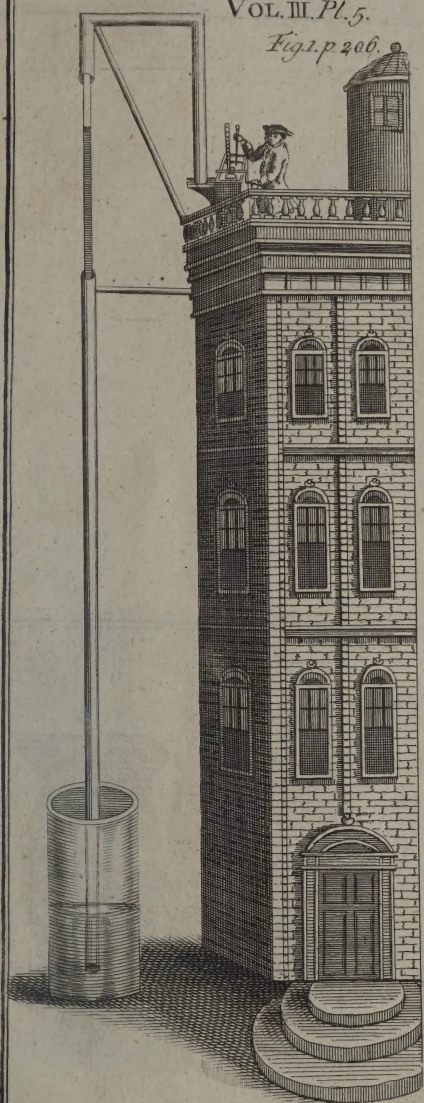


Fig. 4. p. 275.













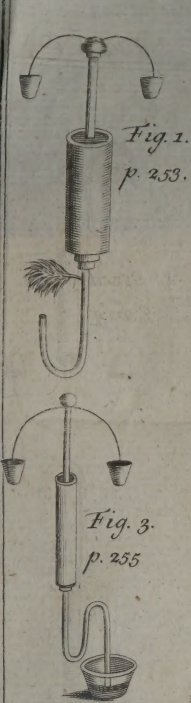


Fig. 2. p. 243.

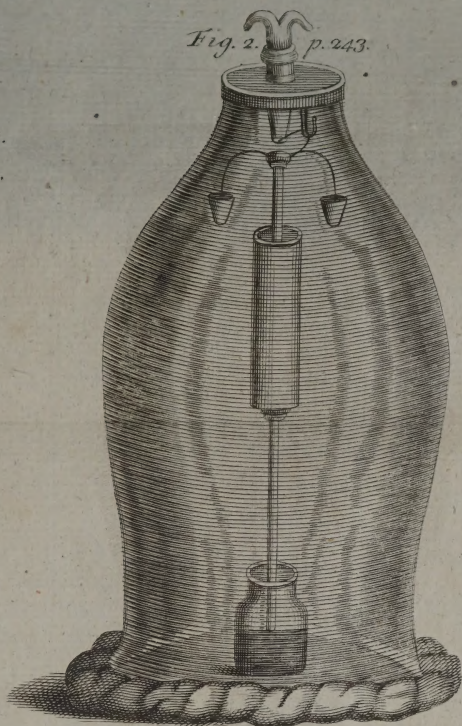


Fig. 4. p. 257.

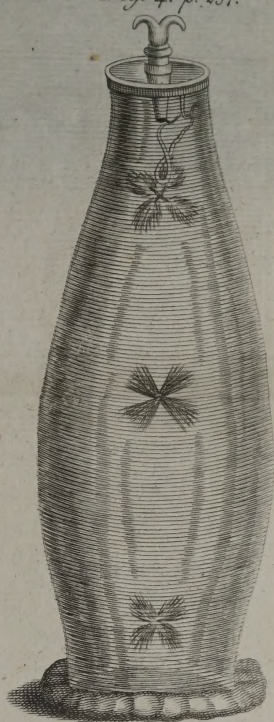


Fig. 1.  
p. 259.

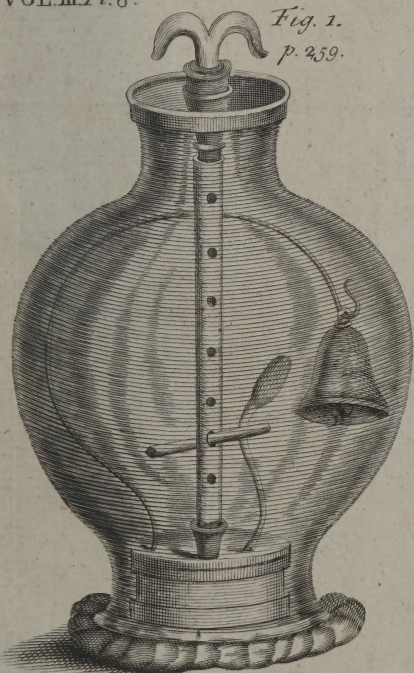


Fig. 2. p. 268.



Fig. 3. p. 270.



Fig. 4. p. 270.



Fig. 5.  
p. 271.

